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*Representation of Social Processes by Markov Models—Singer
and Spilerman*

The Sociology of National Development—Portes

*Size and the Density of Interaction in Human Aggregates—Mayhew
and Levinger*

Occupational Structure and Alienation—Kohn

Industrial Violence in Italy, 1878-1903—Snyder and Kelly

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A Call for Papers

The editors of the *American Journal of Sociology* are actively seeking articles that make major contributions to theory building and research in sociology or set important new agenda for the discipline. The overwhelming proportion of papers that we now receive report research. We will always be eager to publish research papers if they are truly first-rate. But there are other kinds of papers, equally important, that we see all too seldom. If we are currently receiving our share of these papers, then we would like to have more than our share.

We are looking especially for presentations of original theoretical arguments, descriptions and illustrations of important new methods, and critical syntheses and evaluations of the state of knowledge or method in major areas of the discipline. We also would like to publish papers that point up important connections between theory and method in sociology and other disciplines.

We will do our best to insure that these papers receive prompt, trenchant, and appropriate reviews.

IN THIS ISSUE

BURTON SINGER is associate professor of mathematical statistics at Columbia University. He is currently developing some new techniques for discriminating among multiple dynamic models of social processes using multiwave panel data. He is also constructing a mathematical formulation of Weber's sociology of religion. His other interests include stochastic processes, genetics, and theories of social structure.

SEYMOUR SPILERMAN is professor of sociology at the University of Wisconsin. He continues to do research on labor market structure and the situation of ethnics, on racial violence, and on mathematical models of social structure.

ALEJANDRO PORTES is professor of sociology at Duke University. He is currently engaged in a longitudinal study of the adjustment of recent Latin American immigrants to the United States and is also continuing research on problems of urbanization and national development.

BRUCE H. MAYHEW is professor of comparative sociology at the University of South Carolina. He is currently working on a theory of structural complexity.

ROGER L. LEVINGER is a graduate student in sociology at Temple University. His primary interest is in mathematical models of social systems.

MELVIN L. KOHN is chief of the Laboratory of Socio-environmental Studies of the National Institute of Mental Health. He and Carmi Schooler are pursuing their research on the relationship between occupational structure and psychological functioning by means of a follow-up to the study on which the paper in this issue is based.

DAVID SNYDER is assistant professor of sociology at Indiana University. His current research includes continuation of several investigations of collective protest and violence. He is also engaged in a comparative and historical project with Paula M. Hudis on the sources and (especially income) consequences of occupational sex and race segregation.

WILLIAM R. KELLY is a graduate student in the Department of Sociology, Indiana University. His current research focuses on the relationship between racial violence and social change and on cross-national analyses of fertility differentials.

STEPHEN COBB is a member of the House of Representatives of the Tennessee General Assembly and a former assistant professor of sociology and head of the Department of Criminal Justice at Tennessee State University. His research interest is empirical studies of the military-industrial complex. He is working on a law degree at the Vanderbilt University School of Law.

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RALPH R. SELL is a graduate student in the Department of Sociology and a research assistant with the Population Issues Research Office at the Pennsylvania State University. His current research is concerned with the effect of motives on decision-making models of human spatial movement.

Erratum

In the review of *Sociology as Social Criticism* (March 1976), the next to the last sentence (p. 1223) should read: "In evaluating nonradical sociology, Bottomore conflates theoretical and ideological issues."

Information for Contributors

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- Davis, K. 1963*a*. "The Theory of Change and Response in Modern Demographic History." *Population Index* 29 (October): 345-66.
- . 1963*b*. "Social Demography." Pp. 124-37 in *The Behavioral Sciences Today*, edited by Bernard Berelson. New York: Basic Books.
- Goode, W. J. 1967. "The Protection of the Inept." *American Sociological Review* 32 (February): 5-19.
- Moore, Wilbert E., and Arnold S. Feldman. 1960. *Labor Commitment and Social Change in Developing Areas*. New York: Social Science Research Council.
- Sanford, Nevitt, ed. 1962. *The American College*. New York: Wiley.
- Weber, Max. (1921) 1968. "Society's Problems." Pp. 12-16 in *Economy and Society*, edited by Guenther Ross and Claus Wittich. New York: Bedminster.

The Representation of Social Processes by Markov Models¹

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In this paper we consider a class of issues which are central to modeling social phenomena by continuous-time Markov structures. In particular, we discuss (a) *embeddability*, or how to determine whether observations on an empirical process could have arisen via the evolution of a continuous-time Markov structure; and (b) *identification*, or what to do if the observations are consistent with more than one continuous-time Markov structure. With respect to the latter topic, we discuss how to select the specific structure from the list of alternatives which should be associated with the empirical process. We point out that the issues of *embeddability* and *identification* are especially pertinent to modeling empirical processes when one has available only fragmentary data and when the observations contain "noise" or other sources of error. These characteristics, of course, describe the typical work situation of sociologists. Finally, we note the type of situation in which a continuous-time model is the proper structure to employ and indicate that issues analogous to the ones we describe here apply to modeling social processes with discrete-time structures.

1. INTRODUCTION

Markov models provide a convenient framework for analyzing the structural mechanisms which underlie social change and for extrapolating shifts in the state distribution of a population. For reviews of applications and discussions of some pertinent mathematical issues, the reader is referred to Boudon (1973), Bartholomew (1973), and Singer and Spilerman (1974). Although most commonly employed in the study of mobility, Markov models have been applied to diverse substantive topics; they

¹The work reported here was supported by grants NSF-GP-31505X and NSF-GS-38574 at Columbia University and by funds granted to the Institute for Research on Poverty of the University of Wisconsin by the Office of Economic Opportunity, pursuant to the Economic Opportunity Act of 1964. We also acknowledge computation funds from National Institute of Child Health and Human Development (NICHD) grant 1-PO1-HDO5876. Earl Kinmonth aided with the computations. Comments by Ken Land and the assistance of Harrison White and Hal Winsborough are gratefully acknowledged.

have been used, for instance, to study the influence of group norms on conformity (Cohen 1963), to measure distance in social networks (Besheers and Laumann 1967), and to analyze recidivism among delinquent juveniles (Wolfgang, Figlio, and Sellin 1972). The attractiveness of this mathematical formulation derives from the fact that it permits a researcher to focus upon the dynamic properties of a social process and ascertain the long-range consequences of particular institutional arrangements. An instructive example of this sort of inquiry is provided by Lieberman and Fuguitt (1967).

Several technical issues relating to the sensitive use of Markov models have begun only recently to receive an amount of attention that is commensurate with their importance. One matter concerns the phenomenon of population heterogeneity. In the initial attempts at modeling mobility processes by time-stationary Markov chains, socially heterogeneous populations were treated as though a single transition rule governed the movements of all individuals. Special kinds of discrepancies observed between the empirical data and predictions from these one-type Markov models were suggestive about the form of stochastic process which might provide a more realistic theoretical framework in which to view mobility (Blumen, Kogan, and McCarthy 1955). The main attempts at modifying the Markov model so it would provide a suitable description of movements by a heterogeneous population have involved viewing the population as consisting of a mixture of independent Markov processes, one for each individual or each distinct social group (McFarland 1970; Ginsberg 1971; Spilerman 1972*a*, 1972*b*; Singer and Spilerman 1974).

A second issue concerns strategies for testing whether empirical observations are, in fact, compatible with an assumed class of models, such as general finite-state Markov processes, or a subset of them, such as birth and death processes. An example of this sort of inquiry is presented in Singer and Spilerman (1974, pp. 360-63), where an observed two-step matrix² $\hat{P}(2)$ —representing occupational change between grandfathers' and respondents' generations—was examined for compatibility with a stationary discrete-time Markov structure. Conceptually, the problem is to decide whether the empirically determined matrix could have arisen via the evolution of the postulated process. Stated technically, it is to ascertain whether there exists a one-step transition matrix $P(1)$ —which would be identified with grandfather-to-father transitions or, equivalently, with father-to-son transitions—such that $\hat{P}(2) = [P(1)]^2$. Where the answer is negative, it would be incorrect to predict future population dis-

² The symbol " $\hat{}$ " over a stochastic matrix or over an element in a matrix will mean that the quantity should be thought of as estimated directly from data. Matrices without this symbol should be viewed as obtained from a mathematical model.

tributions from a Markov model,³ such as by raising the observed matrix to integer powers.

The same kind of issue must be faced with respect to compatibility of observed data with other model structures, and it is this fundamental sort of inquiry that we address in the present paper. We will concentrate on conditions for compatibility with a finite-state continuous-time Markov process, a mathematical structure which holds special interest for two reasons. First, although discrete-time formulations have been used in most applications of Markov models, the empirical processes under consideration commonly evolve continuously, and the appropriate technical apparatus would be a continuous-time model (Coleman 1964a, p. 129). The reason for the greater popularity of the discrete-time structure stems from its simpler mathematical nature, not from considerations of verisimilitude. Second, continuous-time Markov processes provide the underlying mathematical framework for James S. Coleman's (1964a) influential volume in mathematical sociology as well as for a number of more recent publications (Coleman 1968; Mayer 1972; Bartholomew 1973). Because of a neglect of the representation considerations that are discussed here, serious deficiencies exist with the estimation procedures used in these works. An additional reason for concentrating on compatibility with a continuous-time Markov framework is that the conceptual issues which must be addressed with more complicated mathematical structures, such as models of heterogeneous processes, already reveal themselves in this comparatively simple setting.

Representation becomes an issue when we have available only fragmentary data on population movements. Unfortunately, in the study of social phenomena, the common situation is to have very incomplete information about the evolution of an empirical process; frequently, observations have been taken at only two time points, $t = 0$ and $t = t_1$, yielding a single transition matrix⁴ $\hat{P}(0, t_1)$. What we wish to determine, then, are the conditions which this observed matrix must satisfy in order for it to be viewed as an outcome of a continuous-time Markov model. For matrices satisfying the requisite criteria, we wish further to recover the parameters of the particular Markov structure that underlies the empirical process. These issues can be posed most effectively in terms of two sequential considerations—embeddability tests and the identification problem. In practice, a single calculation is usually informative on both matters.

Embeddability.—This issue refers to whether an observed transition

³ In most applications of Markov models, tests of this sort are not made. Hodge (1966) provides an exception.

⁴ Where it is understood that the initial observation is at $t = 0$, we will simplify our notation and write $\hat{P}(t_1)$, or even \hat{P} , in place of $\hat{P}(0, t_1)$.

matrix $\hat{P}(t_1)$ could have arisen via the evolution of a stationary continuous-time Markov process. It is well known that certain stochastic matrices are not compatible with such a formulation; this is the case, for instance, if $\hat{P}(t_1)$ has an element $\hat{p}_{ij}(t_1) = 0$, but some power of the matrix, say $\hat{P}(t_1)^n$, has a nonzero entry in the same position, that is, $\hat{p}_{ij}^{(n)}(t_1) \neq 0$ (Chung 1967, p. 126). Also, according to Coleman (1964a, p. 179; 1964b, p. 4), a stochastic matrix in which some main diagonal element is less than another entry in its column could not have been generated by a continuous-time Markov process. We shall show that Coleman's claim is in error.⁵ For the present discussion, however, the essential point is that, while it is recognized that certain transition matrices cannot be represented by this mathematical structure, there is confusion over the full scope of the requirements for embeddability. Our first task, then, is to devise tests for ascertaining compatibility of an empirically determined matrix with a continuous-time Markov formulation.

Identification.—If the embeddability tests are passed, then we are guaranteed that $\hat{P}(t_1)$ could have been generated by at least one continuous-time Markov process. The identification problem refers to the possibility that the matrix could have originated from the operation of more than one Markov process. Consequently, our second task is to delineate the conditions under which the solution for the parameters of the Markov model will be unique. Also, for instances in which these conditions are not satisfied, we will require procedures for recovering the several Markov structures that could have produced the observed matrix and identifying the particular model from this list which should be associated with the data.

Sampling error and data-collection design.—Overlying the questions of embeddability and multiple solutions is the issue of sampling error. In most applications, an empirically determined transition matrix $\hat{P}(t_1)$ will have been constructed from a population sample. Repeated surveys of the population would produce somewhat different transition arrays, so we would be well advised to investigate the *sensitivity* of our estimate of the underlying Markov structure to sampling error. In particular, with respect to the matter of embeddability, we might wish to inquire whether a nonembeddable $\hat{P}(t_1)$ is "within error distance" of some embeddable matrix \tilde{P} . If it is, we could choose to carry out an analysis in which Markov methods are employed using the adjusted (embeddable) matrix \tilde{P} instead of the observed array $\hat{P}(t_1)$.

⁵ We wish to emphasize at the outset that our extensive criticism of estimation procedures used in Coleman's work in no way detracts from the utility of the mathematical formulations he employs or from his strategies in translating sociological theory into mathematical statements. Indeed, his work has been a source of inspiration to both of us.

The question of data error leads to more intriguing considerations with respect to the phenomenon of multiple solutions. Even if $\hat{P}(t_1)$ is compatible with a unique Markov process, it is possible that a slightly modified matrix \tilde{P} —within error distance of the original array—will produce a very different Markov structure from the one that has been identified. As a result, if the data derive from a population *sample*, then because of sampling variability we may have recovered the wrong Markov structure for the *population-level* process! We therefore discuss strategies for treating an empirically determined matrix as data containing considerable “noise” and identifying from it the particular Markov model to be associated with the substantive process.

Finally, there are crucial considerations regarding when to survey a population in order to facilitate model identification and parameter estimation. It is widely known, for instance, that if the interval between successive observations is very large (with respect to the rate of evolution of the empirical process), $\hat{P}(t_1)$ will resemble the equilibrium matrix, and the parameters of the continuous-time Markov model which produced the observed array cannot be recovered (Coleman 1968, p. 472). Yet the issue of data-collection design is considerably more complex than this simple remark conveys and involves decisions concerning the number of observations to be taken, the spacing between them, and interactions between these considerations.

2. MATHEMATICAL PRELIMINARIES AND EXAMPLES

Consider a stochastic process with a finite number of states whose transition probabilities are governed by the system of ordinary differential equations

$$\frac{dP(t)}{dt} = QP(t), \quad P(0) = I, \quad (2.1)$$

where $P(t)$ and Q are $r \times r$ matrices. It is well known (Coleman 1964a, pp. 127–30; Chung 1967, pp. 251–57) that if Q has the structure

$$q_{ij} \geq 0 \text{ for } i \neq j, \quad q_{ii} \leq 0, \quad \sum_{j=1}^r q_{ij} = 0 \text{ for } i = 1, \dots, r, \quad (2.2)$$

then the functions $P(t)$, $t > 0$, which are solutions of (2.1) comprise the transition matrices of continuous-time stationary Markov chains. A typical element, $p_{ij}(t)$, of $P(t)$ has the interpretation:

$p_{ij}(t)$ = probability that an individual starting in state i at time 0 will be in state j at time t .

The Q -arrays, which are known as "intensity matrices," provide structural information about the population:

- i) $q_{ij}/-q_{ii}$ = probability that an individual in state i will move to state j , given the occurrence of a transition.
- ii) $1/-q_{ii}$ = expected length of time for an individual in state i to remain in that state.

We will denote the class of intensity matrices (arrays of the form [2.2]) by the symbol \underline{Q} .

Solutions of (2.1) are given by the exponential formula

$$P(t) = e^{Qt}, \quad t > 0, \quad (2.3)$$

where the matrix exponential e^A (A being an arbitrary $r \times r$ matrix) is defined by

$$e^A = \sum_{k=0}^{\infty} A^k/k!$$

The problem of finding simple test criteria on the entries of an observed stochastic matrix $\hat{P}(t_1)$, $t_1 < \infty$, which will guarantee that it can be written in the form (2.3) with $Q \in \underline{Q}$, was first posed by Elfving (1937). It has come to be known as the *embedding problem* for continuous-time Markov chains.

An obvious description of the subclass \underline{Z} of stochastic matrices that are embeddable is given by

$$\underline{Z} = \{P \text{ such that } \log P \in \underline{Q}\}.$$

Attempts to develop practical test criteria or computer programs to determine membership in \underline{Z} are reported in Coleman (1964a, pp. 177-82), Mayer (1972, pp. 327-28), and Zahl (1955, p. 97). However, all these investigations suffer from a confusion about the full scope of the embedding problem, as well as from using an incomplete description of the logarithm function of matrix argument. This situation has resulted in a number of erroneous statements about the conditions under which an empirically determined matrix $\hat{P}(t_1)$ is, or is not, compatible with a continuous-time Markov process.

Example 1. Coleman (1964a, p. 179) has asserted that "the most obvious incompatibility is one in which for some state i , n_{ii}/n_i is less than some n_{jj}/n_j for some state j ."⁸ This statement suggests that a Markov

⁸ n_{ij} = number of persons starting in state i at a reference time $t=0$ who are in state j at a later time $t=1$; $n_i = \sum_{j=1}^r n_{ij}$. In our notation, $n_{ii}/n_i = \hat{p}_{ii}$. Actually,

Representation of Social Processes by Markov Models

structure would not be a suitable model for a large class of mobility matrices (e.g., Prais 1955, table 1; Coleman 1964a, table 14.8) or, indeed, for any array in which some off-diagonal element exceeds the main diagonal entry in its column. That this assertion is incorrect can be seen from the matrix

$$P = \begin{bmatrix} .260 & .169 & .248 & .323 \\ .327 & .275 & .146 & .252 \\ .269 & .346 & .232 & .153 \\ .162 & .285 & .305 & .248 \end{bmatrix}. \quad (2.4)$$

In every column there is a violation of Coleman's necessary criterion, yet this matrix can be represented as e^Q with

$$Q = \begin{bmatrix} -1.700 & 0.034 & 0.025 & 1.641 \\ 1.573 & -1.657 & 0.059 & 0.025 \\ 0.051 & 1.785 & -1.853 & 0.017 \\ 0.017 & 0.085 & 1.649 & -1.751 \end{bmatrix}.$$

Example 2. Elsewhere, Coleman (1973, p. 21) has written, "It is not the case that any discrete-time Markov chain can be generated by an appropriate continuous-time process. Heuristically, those discrete-time chains that cannot be generated by a continuous-time process are those in which the equilibrium distribution is approached through a damped wave, rather than approached asymptotically."⁷ Coleman's statement characterizing nonembeddable matrices is incorrect, as the following computations illustrate.

By exponentiating the intensity matrix Q from example 1 with $t = 1$, $P(1.00) = e^{1.00Q}$, the transition array (2.4) is reproduced. At time $t = 1.41$,

$$P(1.41) = e^{1.41Q} = \begin{bmatrix} .231 & .233 & .261 & .275 \\ .284 & .244 & .201 & .271 \\ .285 & .296 & .206 & .213 \\ .224 & .299 & .257 & .220 \end{bmatrix},$$

and, at time $t = 2.24$,

$$P(2.24) = e^{2.24Q} = \begin{bmatrix} .248 & .271 & .239 & .242 \\ .252 & .259 & .235 & .254 \\ .265 & .262 & .223 & .250 \\ .261 & .275 & .226 & .238 \end{bmatrix}.$$

Coleman wrote n_{ij}/n_j in place of n_{ji}/n_j . This is obviously in error, and elsewhere (Coleman 1964b, p. 4) he makes clear his intention.

⁷ From the context, we interpret the word "asymptotically" to mean monotone, rather than oscillatory, convergence.

Note that each main diagonal entry $p_{ii}(t)$, observed over the three matrices, has the property $p_{ii}(1.00) > p_{ii}(1.41) < p_{ii}(2.24)$. This means that $p_{ii}(t)$ approaches an equilibrium value as $t \rightarrow \infty$ through damped oscillations and not asymptotically. Yet, because of the manner by which the sequence of P -matrices was constructed, they depict the evolution of a continuous-time Markov process.

Example 3. In attempting to represent an observed matrix $\hat{P}(t)$ in the form (2.3), Zahl (1955, p. 97) states that "the estimate of Q is taken to be

$$\frac{1}{t} \log \hat{P}(t) = \frac{1}{t} \sum_{k=1}^{\infty} \frac{(-1)^{k-1} [\hat{P}(t) - I]^k}{k} \quad (2.5)$$

provided the series converges." Coleman (1968, p. 472) makes essentially the same claim. Yet, although convergence of (2.5) does provide a representation of $\log \hat{P}$, it *does not* guarantee that⁸ $\log \hat{P} \in \underline{Q}$. In particular, consider

$$\hat{P} = \begin{bmatrix} .600 & .330 & .070 \\ .302 & .560 & .138 \\ .380 & .040 & .580 \end{bmatrix}$$

The series representation for $\log \hat{P}$ converges to

$$\log \hat{P} = \begin{bmatrix} -.692 & .639 & .053 \\ .496 & -.733 & .237 \\ .707 & -.144 & -.563 \end{bmatrix}$$

which is not in \underline{Q} , since $(\log \hat{P})_{32} = -.144 < 0$.

Example 4. In possibly the most serious of the misunderstandings, Coleman (1968, p. 472) has asserted that, "when [(2.5)] does not converge, this means that the data are not compatible with the assumptions of a continuous-time Markov process, or that the moves of the panel are too widely spaced." Mayer (1972, p. 328) makes essentially the same point: "The failure of [(2.5)] to converge for all transition matrices $P(t)$ reflects the fact that not all such matrices can arise from a continuous-time stationary Markov chain." These statements are in error. Equation (2.5) may fail to converge for matrices P , not resembling the equilibrium matrix, which nonetheless can be represented in the form $e^{Q\tau}$ with $Q \in \underline{Q}$.

⁸ In different contexts, we speak of checking whether $Q = (1/t) \log \hat{P} \in \underline{Q}$ or whether $\log \hat{P} \in \underline{Q}$. Because multiplication of a matrix by a real-valued quantity does not alter its character with respect to satisfying conditions (2.2), the two tests are equivalent.

Consider

$$\hat{P} = \begin{bmatrix} .3654 & .3762 & .2584 \\ .3292 & .3567 & .3141 \\ .4040 & .3188 & .2772 \end{bmatrix}.$$

The series representation (2.5) converges if and only if $|\lambda_i - 1| < 1$ for all eigenvalues λ_i of \hat{P} . The matrix above has eigenvalues $\lambda_1 = 1$, $\lambda_2 = .053i$, $\lambda_3 = -.053i$. Thus $|\lambda_2 - 1| = |\lambda_3 - 1| > 1$ and (2.5) diverges. Nevertheless, $\hat{P} = e^Q$ for

$$Q = \begin{bmatrix} -1.805 & 1.718 & 0.087 \\ 0.044 & -1.784 & 1.740 \\ 2.262 & 0.017 & -2.279 \end{bmatrix},$$

and it is therefore embeddable.

The preceding examples highlight the confusions that exist concerning which transition matrices can be represented as outcomes of the evolution of a continuous-time Markov process. In particular, we have indicated that the standard recipe for estimating Q (the matrix of structural parameters which govern population movements)—via the power-series representation (2.5)—is highly deficient. The series does not provide a complete description of the logarithm of a matrix; as a result, it fails to converge for transition arrays that are compatible with a Markov formulation.

In fact, the inadequacy of equation (2.5) as a procedure for estimating the intensity matrix Q is even more fundamental than the illustrations above suggest. While the power series will converge to at most one version of $\log \hat{P} \in \underline{Q}$, the equation $\hat{P} = e^Q$ can have multiple solutions $Q \in \underline{Q}$. This is a matter of great importance in sociological investigations, because the conventional strategy in using Markov models for theory construction emphasizes decomposing the q_{ij} elements of Q among theoretically postulated effect parameters (Coleman 1964a, chap. 5; 1964b, chap. 2; McDill and Coleman 1963). Clearly, one can hardly begin this task without ensuring that the *correct* Q has been recovered for the substantive process under study. Before considering the issues of multiple solutions and model identification, we address the conceptually prior question of embeddability of \hat{P} ; that is, we seek to determine which transition matrices are compatible with a continuous-time Markov process.

3. EMBEDDABILITY OF \hat{P}

In the case of 2×2 matrices, a complete and practical solution to the question of embeddability was given by D. G. Kendall (see Kingman 1962, p. 15), who proved that

$$\hat{P} = \begin{bmatrix} \hat{p}_{11} & \hat{p}_{12} \\ \hat{p}_{21} & \hat{p}_{22} \end{bmatrix}, \quad \hat{p}_{ij} \geq 0, \quad \sum_j \hat{p}_{ij} = 1$$

is in \underline{Q} (or, equivalently, can be represented as e^Q , $Q \in \underline{Q}$) if and only if $\hat{p}_{11} + \hat{p}_{22} > 1$.

A solution to the embedding problem for stochastic matrices with an arbitrary finite number of states was provided by Kingman (1962). In particular, he proved that \hat{P} can be written in the form e^Q , with $Q \in \underline{Q}$, if and only if (i) $\det \hat{P} > 0$, and (ii) for every positive integer n , there is a stochastic matrix P_n such that $(P_n)^n = \hat{P}$. Unfortunately, condition (ii) does not lead to *practical* test procedures to be applied to \hat{P} , and Kingman pointed out the impossibility of obtaining general tests as simple as those in the 2×2 case for matrices of order greater than or equal to 3. A further mathematically interesting solution to the embedding problem has recently been given by Johansen (1973, p. 180); however, in keeping with Kingman's remarks, it too is not useful for practical computation.

This impasse has led to the development of a considerable number of easily applicable *necessary* conditions for an $r \times r$ stochastic matrix \hat{P} to be in \underline{Q} . These conditions are presented in Section 3.1, with illustrations of their use. A common feature of the tests is that they can be used only to assert that a particular matrix is *not* compatible with a Markov model. An empirically determined matrix which passes all the tests in Section 3.1 must still be subject to an examination based on *sufficiency* conditions for embeddability, if one hopes to pass on to the stage of model identification. With the results of Kingman (1962) and Johansen (1973) at hand, our only recourse is to develop simple computational procedures for obtaining all branches of $\log \hat{P}$ compatible with the criteria in Section 3.1 and test these versions of the logarithm for membership in \underline{Q} . This seemingly straightforward program leads to some surprisingly subtle phenomena, which are delineated in Section 3.2. General practical recommendations for testing an observed matrix \hat{P} for embeddability are outlined in Section 3.3.

3.1 Necessary Conditions

Test criteria which empirically determined matrices must satisfy to be compatible with a family of mathematical models can be viewed usefully as devices for isolating matrices generated by these models from the class of all stochastic arrays. The necessary conditions listed below are the simplest such tests for distinguishing the subclass of matrices generated by continuous-time Markov models.

Condition 1.—(Austin and Ornstein; see Chung [1967, p. 126] for de-

tails.) If $\hat{p}_{ij}(t_1) = 0$, then $\hat{p}_{ij}^{(n)}(t_1) = 0$ for every integer n . If $\hat{p}_{ij}(t_1) \neq 0$, then $\hat{p}_{ij}^{(n)}(t_1) \neq 0$ for any integer n .

Condition 2.—(Kingman 1962) $\det \hat{P} > 0$.

Condition 3.—(Elfving 1937) No eigenvalue λ_i of \hat{P} can satisfy $|\lambda_i| = 1$ other than $\lambda_i = 1$. In addition, any negative eigenvalue must have even (algebraic) multiplicity.

Condition 4.—(Runnenberg 1962) All eigenvalues of \hat{P} must lie inside a heart-shaped region H_r in the complex plane whose boundary is the curve $x(v) + iy(v)$, where

$$\left. \begin{aligned} x(v) &= \left[\exp\left(-v + v \cos \frac{2\pi}{r}\right) \right] \cos\left(v \sin \frac{2\pi}{r}\right) \\ y(v) &= \left[\exp\left(-v + v \cos \frac{2\pi}{r}\right) \right] \sin\left(v \sin \frac{2\pi}{r}\right) \end{aligned} \right\} \quad (3.1)$$

together with its symmetric image with respect to the real axis. In this parametrized formulation, r = order of the matrix \hat{P} , and v is restricted by $0 \leq v \leq \pi/\sin(2\pi/r)$. The regions H_3 , H_6 , and H_{12} are displayed in figures 1, 2, and 3. The larger cone-shaped zones K_3 , K_6 , and K_{12} show the

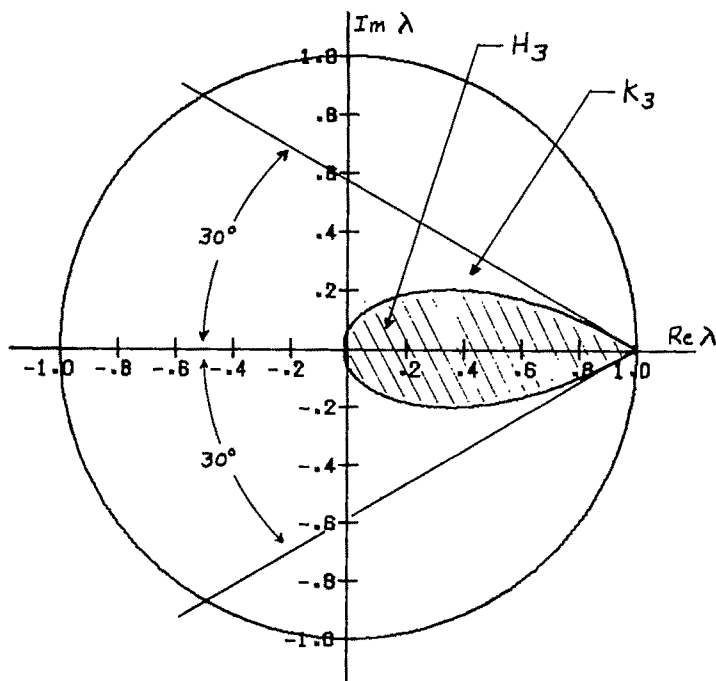


FIG. 1.—Eigenvalue regions for 3×3 stochastic matrices (K_3) and for the subset of them which is in \mathbb{Z} (H_3). A necessary condition for \hat{P} to be embeddable is that all its eigenvalues lie in the shaded zone.

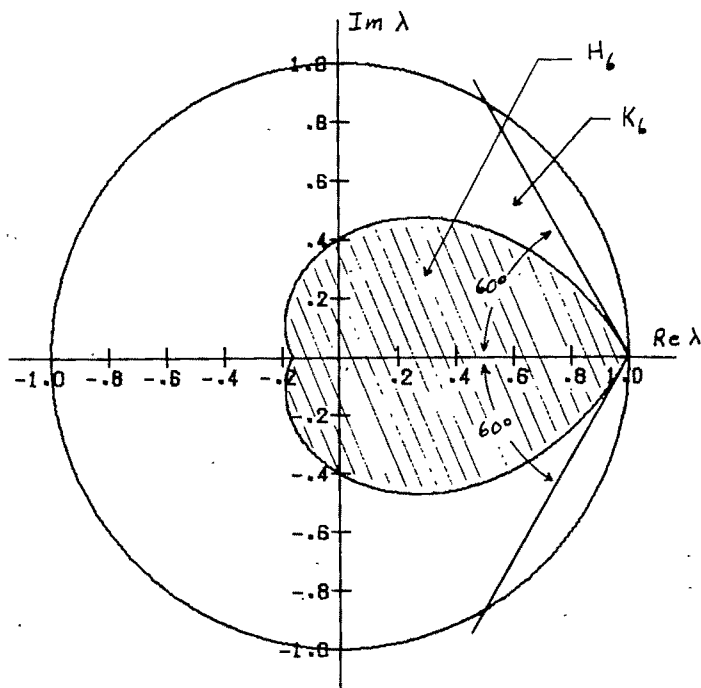


FIG. 2.—Eigenvalue regions for 6×6 stochastic matrices (K_6) and for the subset of them which is in \mathcal{Z} (H_6). A necessary condition for \hat{P} to be embeddable is that all its eigenvalues lie in the shaded zone.

bounds on the eigenvalues of arbitrary 3×3 , 6×6 , and 12×12 stochastic matrices.

The cone-shaped zones arise from the requirement that the eigenvalues of an arbitrary stochastic matrix \hat{P} must satisfy⁹

$$\left(\frac{1}{2} + \frac{1}{r}\right)\pi \leq \arg(\lambda - 1) \leq \left(\frac{3}{2} - \frac{1}{r}\right)\pi \quad (3.2)$$

(where the argument¹⁰ is in radians), together with the condition $|\lambda| \leq 1$. The additional limitation to the heart-shaped set H_r , contained in K_r , arises from the continuous-time Markov assumptions. This restriction can also be described by saying that the eigenvalues of \hat{P} must satisfy (3.2) and

$$\left(\frac{1}{2} + \frac{1}{r}\right)\pi \leq \arg(\log \lambda) \leq \left(\frac{3}{2} - \frac{1}{r}\right)\pi \quad (3.3)$$

⁹ These inequalities were established by Karpelewitsch (1951); they represent a considerable strengthening of the well-known restriction that all eigenvalues of a stochastic matrix must lie inside the unit circle.

¹⁰ For a complex number $\mu = a + bi$, we define $\arg(\mu) = \tan^{-1}(b/a)$.

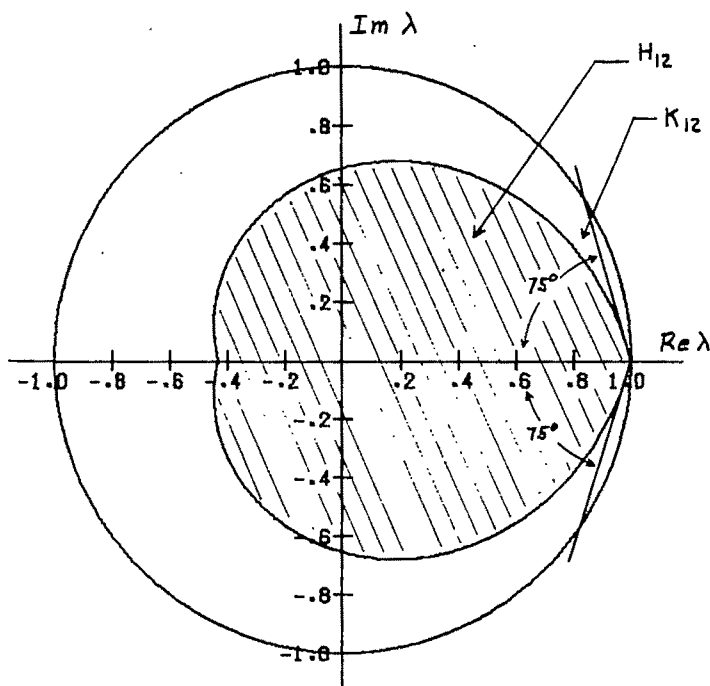


FIG. 3.—Eigenvalue regions for 12×12 stochastic matrices (K_{12}) and for the subset of them which is in \mathcal{Z} (H_{12}). A necessary condition for \hat{P} to be embeddable is that all its eigenvalues lie in the shaded zone.

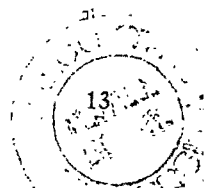
Examination of H_3 explains why failure of the series (2.5) to converge in example 4 did not rule out compatibility of \hat{P} with a continuous-time Markov process. The region of convergence of (2.5) is $|\lambda_t - 1| < 1$, i.e., the unit circle centered at (1, 0), and the complex eigenvalues of the matrix in that example, while exterior to this region, are inside H_3 .

Example 5. Suppose you observe the matrix

$$\hat{P} = \begin{bmatrix} .15 & .35 & .50 \\ .37 & .45 & .18 \\ .20 & .60 & .20 \end{bmatrix}.$$

Since $\det \hat{P} = .05 > 0$, condition 2 is satisfied. However, \hat{P} has eigenvalues $\lambda_1 = 1$, $\lambda_2 = -.1 + .2i$, $\lambda_3 = -.1 - .2i$ which, by (3.2), lie inside the cone K_3 , but they are outside the heart-shaped zone H_3 . Thus \hat{P} cannot be represented as e^Q for any $Q \in \mathcal{Q}$; in other words, it is not compatible with a continuous-time Markov model.

Example 6. Consider the matrix



$$\hat{P} = \begin{bmatrix} .20 & .40 & .40 \\ .35 & .20 & .45 \\ .40 & .40 & .20 \end{bmatrix}.$$

Here, $\det \hat{P} = .04 > 0$, satisfying condition 2. The eigenvalues of \hat{P} are $\lambda_1 = 1$, $\lambda_2 = \lambda_3 = -.2$, so that condition 3 applies and is satisfied. Nevertheless, λ_2 and λ_3 are outside the zone H_3 . Thus \hat{P} is not compatible with a continuous-time Markov model.

Example 7. Recall the matrix of example 3,

$$\hat{P} = \begin{bmatrix} .600 & .330 & .070 \\ .302 & .560 & .138 \\ .380 & .040 & .580 \end{bmatrix}.$$

This matrix satisfies the *necessary* conditions 1-4; however, it is still not representable as e^Q for any $Q \in \underline{Q}$. This assertion is based on an examination of all versions of $\log \hat{P}$ which are candidates for membership in \underline{Q} . An understanding of these tests requires a complete description of $\log \hat{P}$. This is the subject of the next section.

3.2 The Matrix Equation $\hat{P} = e^Q$

We require a definition of a function of matrix argument¹¹ which is sufficiently general to include analytic functions such as e^x and $\log x$. It is useful to motivate the definition by an important property of polynomial functions $g(x)$. In particular, if

$$g(x) = a_0 + a_1x + a_2x^2 + \dots a_nx^n$$

and A is an arbitrary square matrix, a natural definition of $g(A)$ is given by

$$g(A) = a_0I + a_1A + a_2A^2 + \dots a_nA^n.$$

In addition, A can always be reduced to Jordan form J by some non-singular matrix H , that is,

$$A = HJH^{-1}. \quad (3.4)$$

Finally, it is readily verified that

$$g(A) = Hg(J)H^{-1}. \quad (3.5)$$

Every Jordan matrix J has the following block structure:

¹¹ For a lucid and detailed mathematical exposition, the reader should consult Gantmacher (1960, chap. 5).

$$J = \begin{bmatrix} J_1 & & & \mathbf{0} \\ & J_2 & & \\ & & \ddots & \\ \mathbf{0} & & & J_k \end{bmatrix}, \quad J_i = \begin{bmatrix} \lambda_i & 1 & & \mathbf{0} \\ & \lambda_i & 1 & \\ & & \ddots & \ddots \\ \mathbf{0} & & & 1 \\ & & & & \lambda_i \end{bmatrix}, \quad (3.6)$$

where λ_i is the i th eigenvalue of matrix A and occurs in J_i with multiplicity v_i , the order of J_i . (The λ_i appearing in different blocks J_i are not

necessarily distinct.) Also, $\sum_1^k v_i = r$, the order of A .

The expression (3.5) will be useful in a wider context¹² than just polynomials, provided that we have a representation of $g(J)$ for arbitrary Jordan matrices J , which generalizes to analytic functions¹³ $f(J)$. Then our program will be to define $f(A)$ according to (3.5), with g replaced by f , adding appropriate conventions for multiple-valued functions. For a polynomial function $g(x)$, we introduce its Taylor series expansion about $x = \lambda_i$ and write

$$g(J) = \begin{bmatrix} g(J_1) & & & \mathbf{0} \\ & g(J_2) & & \\ & & \ddots & \\ \mathbf{0} & & & g(J_k) \end{bmatrix}, \quad (3.7)$$

where¹⁴

$$g(J_i) = g(\lambda_i)I + g'(\lambda_i)(J_i - \lambda_i I) + \dots + \frac{g^{(v_i-1)}(\lambda_i)}{(v_i-1)!} (J_i - \lambda_i I)^{v_i-1}$$

$$= \begin{bmatrix} g(\lambda_i) & g'(\lambda_i) & \frac{g''(\lambda_i)}{2!} & \dots & \frac{g^{(v_i-1)}(\lambda_i)}{(v_i-1)!} \\ 0 & g(\lambda_i) & g'(\lambda_i) & \dots & \frac{g^{(v_i-2)}(\lambda_i)}{(v_i-2)!} \\ \vdots & 0 & \vdots & \ddots & \vdots \\ \vdots & \vdots & \vdots & \ddots & \vdots \\ 0 & 0 & 0 & \dots & g(\lambda_i) \end{bmatrix}.$$

¹² The remainder of this section is more difficult mathematically and can be skipped at a first reading. Continue with Section 3.2a, "Distinct Eigenvalues."

¹³ A function is said to be analytic at x if it has a derivative in a neighborhood containing the point.

¹⁴ Although the Taylor series expansion has an infinite number of terms, $(J_i - \lambda_i I)^n = 0$ for all values of $n \geq v_i$.

Formula (3.7) has meaning for any function f which is analytic in a neighborhood of the eigenvalue λ_i . Thus, if f is *single valued* and analytic in a region of the complex plane containing the eigenvalues of A (e.g., $f(x) = e^x$), we define

$$f(A) = Hf(J)H^{-1} \quad (3.8)$$

where $f(J)$ is specified by (3.7) with g replaced by f .

If f is *multiple valued* (e.g., $f(x) = \sqrt{x}$, or $f(x) = \log x$), we define a *branch* of $f(A)$ corresponding to the similarity transformation H by

$$f_a(A) = Hf_a(J)H^{-1} \quad (3.9)$$

where

$$f_a(J) = \begin{bmatrix} f_{a_1}(J_1) & & & \mathbf{0} \\ & f_{a_2}(J_2) & & \\ & & \ddots & \\ \mathbf{0} & & & f_{a_k}(J_k) \end{bmatrix}$$

and $f_{a_i}(x)$ is *any* single-valued branch of $f(x)$. Notice that different branches of $f(x)$ may be used with distinct Jordan blocks J_i and that each combination of $(f_{a_1}, f_{a_2}, \dots, f_{a_k})$ will generate a different version of $f(A)$. Furthermore, the value of $f(A)$ may depend on the choice of H , a point to which we will have cause to return.¹⁵ This definition was introduced by Cipolla (1932)—see also Rinehart (1955)—and represents the necessary level of generality for a discussion of solutions of the matrix equation $e^Q = \hat{P}$ (\hat{P} is identified with A in the preceding discussion). We now specialize to the case where the eigenvalues of A are distinct. The repeated eigenvalue condition, while crucial to a complete understanding of embeddability, is more involved mathematically and will be considered separately.

3.2a. Distinct Eigenvalues

In this case, the Jordan matrix J reduces to a diagonal matrix D , in which the nonzero entries are the eigenvalues of A . Analogous to (3.4), we have

$$A = HDH^{-1} \quad (3.10)$$

¹⁵ This matter is discussed in proposition 2.

where

$$D = \begin{bmatrix} \lambda_1 & & & 0 \\ & \lambda_2 & & \\ & & \ddots & \\ 0 & & & \lambda_r \end{bmatrix}.$$

Also, the *eigenvector* corresponding to λ_i is contained in the i th column of H . The foregoing discussion regarding analytic functions of matrix argument carries over in its entirety, with the functions of Jordan blocks $f(J_i)$ replaced by functions of eigenvalues $f(\lambda_i)$. In particular, when f is multiple valued, (3.9) reduces to

$$f_a(A) = H f_a(D) H^{-1} \quad (3.11)$$

where

$$f_a(D) = \begin{bmatrix} f_{a_1}(\lambda_1) & & & 0 \\ & f_{a_2}(\lambda_2) & & \\ & & \ddots & \\ 0 & & & f_{a_r}(\lambda_r) \end{bmatrix}.$$

A different version of $f(A)$ is obtained from each combination of branches of $(f_{a_1}, f_{a_2}, \dots, f_{a_r})$.

This discussion is relevant in the following way to the determination of embeddability. Ascertaining compatibility of an observed matrix \hat{P} with a continuous-time Markov process requires investigating whether there exists an array $Q \in \underline{Q}$ such that $\hat{P} = e^Q$. Lacking readily-computable *sufficiency* conditions for general $r \times r$ stochastic matrices, our strategy must be to compute $\log \hat{P}$ and examine it for membership in \underline{Q} . Now, the logarithm function is multiple valued,¹⁶

$$\log_k z = \log |z| + i(\theta + 2\pi k), \quad k = 0, \pm 1, \pm 2, \dots, \quad (3.12)$$

where z is an arbitrary complex number, $z = a + bi$; $|z| = \sqrt{a^2 + b^2}$; and $\theta = \tan^{-1} b/a$. Each value of k generates a different version of $\log z$, called a *branch* of the logarithm. In general, an infinity of branches will exist.

¹⁶ The simplest way to appreciate the multiple-valued character of the logarithm is to begin with the definition: $x = \log y$ if x is a solution of the equation $e^x = y$ for a given y . Suppose x is such a solution. Then, for any integer k , $e^{x+2\pi ki} = e^x e^{2\pi ki} = e^x = y$ (since $e^{2\pi ki} = \cos 2\pi k + i \sin 2\pi k = 1$). Therefore, $\log y$ takes on the values $x, x \pm 2\pi i, x \pm 4\pi i$, etc.

From equations (3.11) and (3.12), we have

$$\log_K \hat{P} = H \log_K D H^{-1} \quad (3.13)$$

where¹⁷

$$\log_K D = \begin{bmatrix} \log_{k_1} \lambda_1 & & & & 0 \\ & \log_{k_2} \lambda_2 & & & \\ & & \ddots & & \\ & & & \ddots & \\ 0 & & & & \log_{k_r} \lambda_r \end{bmatrix}.$$

Every combination of values of $(\log_{k_1} \lambda_1, \log_{k_2} \lambda_2, \dots, \log_{k_r} \lambda_r)$ in (3.13) will yield a version of $\log \hat{P}$, so to determine embeddability one must check whether at least one branch is in \underline{Q} . An important implication of *necessary condition 4* in Section 3.1 is that only *finitely* many branches of $\log \hat{P}$ need be checked for membership in \underline{Q} . It is this feature which makes the computational tests described in detail in Section 3.3 feasible. Furthermore, in many applications, the number of branches which must be computed is quite small.

Sylvester's formula.—If A is an $r \times r$ matrix with distinct eigenvalues $\lambda_1, \lambda_2, \dots, \lambda_r$, and if f is single valued in a neighborhood of each of the eigenvalues, then equation (3.8) is equivalent to (Sylvester 1883)

$$f(A) = \sum_{i=1}^r f(\lambda_i) \prod_{j \neq i} \frac{(A - \lambda_j I)}{(\lambda_i - \lambda_j)}. \quad (3.14)$$

In addition, if f is multiple valued, then (3.14), with $f(\lambda_i)$ replaced by $f_{a_i}(\lambda_i)$, defines a version of $f(A)$ for each combination of branches of $(f_{a_1}, f_{a_2}, \dots, f_{a_r})$; in other words, this equation is equivalent to (3.11).

Example 8. Consider the matrix

$$\hat{P} = \begin{bmatrix} .3654 & .3762 & .2584 \\ .3292 & .3567 & .3141 \\ .4040 & .3188 & .2772 \end{bmatrix},$$

which also appeared in example 4, and identify \hat{P} with A in the discussion above. In order to solve the equation $\hat{P} = e^Q$, observe that \hat{P} has distinct eigenvalues $\lambda_1 = 1$, $\lambda_2 = .053i$, $\lambda_3 = -.053i$. Setting $f(x) = \log x$ in Sylvester's formula, we obtain

¹⁷ All logarithms are to base e . The subscript k denotes the branch number of the logarithm of a scalar quantity and takes on the values $k = 0, \pm 1, \pm 2, \dots$. The subscript K denotes a version of the logarithm of a matrix and specifies a combination of branches of the logarithm of the eigenvalues.

$$\log \hat{P} = \log(\lambda_1) \frac{(\hat{P} - \lambda_2 I)(\hat{P} - \lambda_3 I)}{(\lambda_1 - \lambda_2)(\lambda_1 - \lambda_3)} + \log(\lambda_2) \frac{(\hat{P} - \lambda_1 I)(\hat{P} - \lambda_3 I)}{(\lambda_2 - \lambda_1)(\lambda_2 - \lambda_3)} \\ + \log(\lambda_3) \frac{(\hat{P} - \lambda_1 I)(\hat{P} - \lambda_2 I)}{(\lambda_3 - \lambda_1)(\lambda_3 - \lambda_2)} \\ = \begin{bmatrix} -1.805 & 1.718 & 0.087 \\ 0.044 & -1.784 & 1.740 \\ 2.262 & 0.017 & -2.279 \end{bmatrix},$$

which satisfies criterion (2.2) for membership in \underline{Q} . In this calculation we used the principal branches of $\log \lambda_2$ and $\log \lambda_3$; namely, $\log \lambda_2 = \log(.053) + i\pi/2$ and $\log \lambda_3 = \log(.053) - i\pi/2$. Any other branch, e.g., $\log \lambda_2 = \log(.053) + i(\frac{\pi}{2} + 2\pi k)$ for an integer $k \neq 0$, would yield a version of $\log \hat{P}$ which is not in \underline{Q} . For a similar reason, we use the principal branch of $\log(\lambda_1) = \log(1) = 0$.

An important feature of this example and of Sylvester's formula in general is that the logarithm of a matrix is well defined even when the power series (2.5) diverges, as it does here. For matrices with distinct eigenvalues λ_i satisfying $|\lambda_i - 1| < 1$, the series (2.5) is equivalent to the principal branch solution of (3.14)— $k = 0$ in equation (3.12). However, Sylvester's formula is more general, in that it will generate all branches of $\log \hat{P}$ as k is varied.¹⁸ Furthermore, it leads to an evaluation of analytic functions of matrix argument as *finite* polynomials in the original matrix \hat{P} . The transcendental nature of $f(\hat{P})$ is incorporated entirely in the coefficients of this polynomial and involves only functions of eigenvalues. In particular, by rearranging terms, Sylvester's formula for general $r \times r$ matrices (3.14) can be written in the form

$$f(\hat{P}) = c_0 I + c_1 \hat{P} + c_2 \hat{P}^2 + \dots + c_{r-1} \hat{P}^{r-1}$$

in which the c_i 's are scalar functions of the eigenvalues of \hat{P} .

3.2b. Repeated Eigenvalues

When \hat{P} has one or more sets of equal eigenvalues, the computations to determine embeddability can be considerably more involved. Unfortunately, even though the occurrence of repeated eigenvalues in an observed matrix \hat{P} would be a rare event, we will have reason to consider adjustment

¹⁸ Sylvester's formula has been effectively employed by Johansen (1974) in a recent study of the embedding problem. His results, however, are less general than the ones presented here, because Sylvester's formula also provides a less than complete description of the logarithm of a matrix. This point is elaborated in proposition 2.

strategies which make use of this condition. We therefore outline the main issues and analytic procedures at this point; some elaborations are found in Section 4.2 and in the Appendix.

It is useful to categorize matrices with repeated eigenvalues according to whether or not their elementary divisors¹⁹ are distinct. Elementary divisors are said to be distinct if each eigenvalue λ_i appears in exactly one Jordan block $J_i(\lambda_i)$ in equation (3.6). They are said to be *nondistinct* if a repeated eigenvalue λ_i can serve as the diagonal element in more than one Jordan block. The importance of this distinction derives from the fact that the eigenvalues in a block are constrained to be on the same branch of a multiple-valued function—that is, they must have the same value of k in expression (3.12). The presence of nondistinct elementary divisors therefore permits different branches of $\log \lambda_i$ to be present simultaneously in $\log J$, via the presence of λ_i in more than one Jordan block. It is this condition which creates exceptional difficulties in the calculation of $\log \hat{P}$. The following propositions and examples outline the computations for the two multiple-eigenvalue cases:

Proposition 1.—If A is an $r \times r$ matrix with m different eigenvalues $\lambda_1, \dots, \lambda_m$ having multiplicities r_1, \dots, r_m and elementary divisors $(\lambda - \lambda_1)^{r_1}, \dots, (\lambda - \lambda_m)^{r_m}$ —i.e., distinct elementary divisors—and if f is a function that is single valued and analytic in a neighborhood of each of the eigenvalues, then $f(A)$ may be computed via (3.8) or by using the equivalent but computationally often simpler formula²⁰

$$f(A) = \sum_{k=1}^m \sum_{s=1}^{r_k} c_{ks} \left[f(\lambda_k) + (A - \lambda_k I)' f'(\lambda_k) + \dots + \frac{(A - \lambda_k I)^{s-1}}{(s-1)!} f^{(s-1)}(\lambda_k) \right] \prod_{j \neq k} (A - \lambda_j I)^{r_j} (A - \lambda_k I)^{r_k - s} \quad (3.15)$$

where the terms c_{ks} are the coefficients in the partial-fraction expression

$$\frac{1}{\prod_{k=1}^m (\lambda - \lambda_k)^{r_k}} = \sum_{k=1}^m \sum_{s=1}^{r_k} \frac{c_{ks}}{(\lambda - \lambda_k)^s}.$$

When f is multiple valued, the various branches $f_a(A)$ may be found by computing (3.15) for all combinations of branches of $(f_{a_1}, f_{a_2}, \dots, f_{a_r})$ —that is, $f_{a_i}^{(v)}(\lambda_i)$ replaces $f^{(v)}(\lambda_i)$, $v = 0, 1, \dots, s-1$, in equation

¹⁹ On computing the elementary divisors of a matrix, consult Gantmacher (1960, pp. 139–45).

²⁰ When $r_k = 1$ for $k = 1, \dots, m$, then (3.15) reduces to Sylvester's formula (3.14).

(3.15). With respect to determining embeddability of \hat{P} , the number of versions of $\log \hat{P} = f(\hat{P})$ which need to be examined is discussed in Section 3.3.

Example 9. Consider the matrix

$$\hat{P} = \begin{bmatrix} .1600 & .5300 & .3100 \\ .0527 & .4900 & .4577 \\ .1100 & .1400 & .7500 \end{bmatrix},$$

and identify \hat{P} with A in the preceding discussion. The eigenvalues of \hat{P} are $\lambda_1 = 1$ and $\lambda_2 = .2$, with multiplicities $r_1 = 1$ and $r_2 = 2$, respectively. First note that both eigenvalues lie in H_r (fig. 1). It is also the case that the elementary divisors of \hat{P} are distinct; they are $(\lambda - 1)$ and $(\lambda - .2)^2$. We may therefore solve for all solutions to $\hat{P} = e^Q$ by using equation (3.15) and setting $f(\lambda_i) = \log \lambda_i$:

$$\begin{aligned} \log \hat{P} = & c_{11}(\log 1)(\hat{P} - \lambda_2 I)^2 + c_{21}(\log \lambda_2)(\hat{P} - \lambda_1 I)(\hat{P} - \lambda_2 I) \\ & + c_{22} \left[(\log \lambda_2)I + \frac{1}{\lambda_2}(\hat{P} - \lambda_2 I) \right] (\hat{P} - \lambda_1 I). \end{aligned} \quad (3.16)$$

Selecting the principal branch of the logarithm for each eigenvalue, the first term in expression (3.16) disappears, since $\log 1 = 0$. From the remaining terms, we obtain

$$Q = \log \hat{P} = \begin{bmatrix} -2.046 & 1.993 & 0.053 \\ 0.024 & -0.818 & 0.794 \\ 0.315 & 0.043 & -0.358 \end{bmatrix}.$$

As in the previous example, we could have chosen some other branch of the logarithm function, $\log .2 \pm 2\pi ki$, for an integer $k \neq 0$. However, (3.16) would then produce matrices with complex entries, and these have no meaning in the context of Markov models (i.e., they are not in \underline{Q}).

Proposition 2.²¹—All solutions of the equation $e^Q = A$ are called branches of the logarithm function of A , and they are given by (Gantmacher 1960, pp. 239–41)

$$Q = \log A = HB \log J B^{-1}H^{-1} \quad (3.17)$$

where

- i) H is any nonsingular matrix which reduces A to Jordan form, $A = HJH^{-1}$.
- ii) B is an arbitrary nonsingular matrix that commutes with J ; that is, $BJ = JB = 0$.

²¹ The remainder of this section is more difficult mathematically and can be skipped at a first reading. Continue with Section 3.3.

iii)

$$\log J = \begin{bmatrix} \log J_1 & & & & 0 \\ & \log J_2 & & & \\ & & \ddots & & \\ & & & \ddots & \\ 0 & & & & \log J_k \end{bmatrix},$$

where

$$\log J_j = \begin{bmatrix} \log \lambda_j & \frac{-1}{\lambda_j} & \frac{1}{\lambda_j^2} & \cdot & \cdot & \cdot & \frac{(-1)^{v_j-1}}{\lambda_j^{v_j-1}(v_j-1)!} \\ 0 & \log \lambda_j & \frac{-1}{\lambda_j} & \cdot & \cdot & \cdot & \frac{(-1)^{v_j-2}}{\lambda_j^{v_j-2}(v_j-2)!} \\ \cdot & 0 & \cdot & & & & \cdot \\ \cdot & \cdot & \cdot & \cdot & & & \cdot \\ \cdot & \cdot & \cdot & & \cdot & & \cdot \\ \cdot & \cdot & \cdot & & & \cdot & \cdot \\ 0 & 0 & 0 & \cdot & \cdot & \cdot & \log \lambda_j \end{bmatrix},$$

$\log \lambda_j = \log |\lambda_j| + i(\arg \lambda_j + 2\pi k)$, k is an integer, and $v_j =$ multiplicity of λ_j in the elementary divisor $(\lambda - \lambda_j)^{v_j}$.

If the elementary divisors of A are *distinct*, B may be replaced by the identity in (3.17), and $\log A$ is independent of the choice of H . It is this property which permits the simpler representations (3.14) and (3.16). When the elementary divisors of A are *nondistinct*, computation of all versions of $\log A$ requires a knowledge of the matrices B which satisfy $BJ - JB = 0$. These matrices contain a finite number of parameters, each of which can be an arbitrary complex number. Every product HB represents a similarity transformation which reduces A to Jordan form and, at the same time, generates a distinct version of $\log A$. This leads to uncountably many versions of $\log A$; and there may, in fact, be a continuum of such matrices, all or part of which is in \underline{Q} . It is precisely these matrices with nondistinct elementary divisors which prevent the development of simple general solutions to the embedding problem. In any other situation, a researcher need only compute polynomials in \hat{P} to evaluate $\log \hat{P}$, and test a finite number of branches of the logarithm for membership in \underline{Q} .

Example 10. Consider the matrix

$$\hat{P} = \frac{1}{3} \begin{bmatrix} 1 + 2X & 1 - X & 1 - X \\ 1 - X & 1 + 2X & 1 - X \\ 1 - X & 1 - X & 1 + 2X \end{bmatrix}, \quad (3.18)$$

where $X = -e^{-2\sqrt{3}\pi}$, and identify \hat{P} with A in the preceding discussion. The eigenvalues of \hat{P} are $\lambda_1 = 1$, and $\lambda_2 = \lambda_3 = X$; the elementary divisors are $(\lambda - 1)$, $(\lambda - X)$, $(\lambda - X)$, which are nondistinct. Consequently, the Jordan matrix associated with \hat{P} is

$$J = \begin{bmatrix} J_1 & 0 & 0 \\ 0 & J_2 & 0 \\ 0 & 0 & J_3 \end{bmatrix} = \begin{bmatrix} 1 & 0 & 0 \\ 0 & X & 0 \\ 0 & 0 & X \end{bmatrix}.$$

Also, a similarity transformation H , such that $\hat{P} = HJH^{-1}$, is given by

$$H = \begin{bmatrix} 1 & 1 & 1 \\ 1 & \frac{1}{2}(-1 + i\sqrt{3}) & \frac{1}{2}(-1 - i\sqrt{3}) \\ 1 & \frac{1}{2}(-1 - i\sqrt{3}) & \frac{1}{2}(-1 + i\sqrt{3}) \end{bmatrix}. \quad (3.19)$$

In computing $\log \hat{P} = Q$, choose $\log J_1 = \log 1 = 0$; $\log J_2 = \log X = -2\sqrt{3}\pi + i\pi$; and $\log J_3 = \log X = -2\sqrt{3}\pi - i\pi$. Now, formula (3.17) with $B = I$, the identity matrix, yields

$$\log \hat{P} = 2\pi\sqrt{3} \begin{bmatrix} -\frac{2}{3} & \frac{1}{2} & \frac{1}{6} \\ \frac{1}{6} & -\frac{2}{3} & \frac{1}{2} \\ \frac{1}{2} & \frac{1}{6} & -\frac{2}{3} \end{bmatrix}, \quad (3.20)$$

which belongs to \underline{Q} .

To manufacture other versions of $\log \hat{P}$ which are also in \underline{Q} , observe that the matrices which commute with J are all of the form

$$B = \begin{bmatrix} a & 0 & 0 \\ 0 & c_{11} & c_{12} \\ 0 & c_{21} & c_{22} \end{bmatrix}, \quad (3.21)$$

where $\{c_{ij}\}$ and a are arbitrary complex numbers subject only to the restriction that B be invertible. For $\log \hat{P}$ to be in \underline{Q} , we may limit consideration to matrices B with entries satisfying,

$$\left. \begin{array}{l} \text{i) } c_{11} c_{12} - c_{21} c_{22} = 0 \\ \text{ii) } \begin{array}{l} u \text{ is real, where } u = \frac{c_{11} c_{22} + c_{12} c_{21} + 2c_{21} c_{22}}{c_{11} c_{22} - c_{12} c_{21}} \\ v \text{ is real, where } v = \frac{c_{11} c_{22} + c_{12} c_{21} - 2c_{21} c_{22}}{c_{11} c_{22} - c_{12} c_{21}} \end{array} \\ \text{and} \\ \text{iii) } |u| \leq 2 \text{ and } |v| \leq 2. \end{array} \right\} \quad (3.22)$$

Conditions (i) and (ii) guarantee that $\log \hat{P}$ will be real valued, while (iii) ensures that the entries will satisfy criteria (2.2). Each choice of $\{c_{ij}\}$ then yields a version of $\log \hat{P}$ which is a member of \underline{Q} , and they are all given by

$$\log \hat{P} = HB \log J B^{-1} H^{-1}$$

$$= 2\pi\sqrt{3} \begin{bmatrix} -\frac{2}{3} & \frac{1}{3} + \frac{u}{6} & \frac{1}{3} - \frac{u}{6} \\ \frac{1}{3} - \frac{v}{6} & \frac{2}{3} - \frac{(u-v)}{12} & \frac{1}{3} + \frac{(u+v)}{12} \\ \frac{1}{3} + \frac{v}{6} & \frac{1}{3} - \frac{(u+v)}{12} & -\frac{2}{3} + \frac{(u-v)}{12} \end{bmatrix}. \quad (3.23)$$

The matrix (3.20) arises in the special case where $c_{11} = c_{22} = 1$, $c_{12} = c_{21} = 0$, and thus $u = v = 1$. The nonzero constant a in matrix B does not enter into the formula for $\log \hat{P}$, because it can only multiply the first row of $\log J$, all of whose entries are 0.

With this example at hand,²² some remarks concerning the role of such matrices in social mobility studies are in order (these comments will be elaborated upon in Section 4). If the primary purpose of an investigation is to obtain structural information about the propensity of individuals in a population to move between particular states, then our major concern must center on the possible values of $q_{ij} - q_u$ for $i \neq j$ in branches of $\log \hat{P} = Q$ which are in \underline{Q} . These ratios have the interpretation "propensity to move from state i to state j when a change in state occurs." The continuum of branches of $\log \hat{P}$ which are given by (3.23) represents a continuum of propensities to move between states, all compatible with the observed matrix \hat{P} . Focusing on mobility out of state 1 in (3.23), we see that

$$0 \leq \frac{q_{12}}{-q_{11}} = \frac{1/3 + u/6}{2/3} \leq 1$$

and

$$0 \leq \frac{q_{13}}{-q_{11}} = \frac{1/3 - u/6}{2/3} \leq 1.$$

Thus, on the basis of observations at two time points which give rise to \hat{P}

²² The matrix (3.18) was introduced by Cuthbert (1973) in order to exhibit an example of a stochastic matrix compatible with a continuum of Markov models. Cuthbert's continuum arises when you choose $c_{11} = c_{22} = 1$ and $c_{12} = c_{21}$, real. Then the constraints on u and v entail that $|c_{12}| = |c_{21}| \leq 1/3$. This choice does not, however, lead to all of the branches of $\log \hat{P}$ given in (3.23), which represents an exhaustive list in \underline{Q} .

given by (3.18), we cannot even say whether individuals who start out in state 1 tend to favor state 2 or state 3 when they move. Clearly, this situation is totally uninformative about the underlying mobility mechanism, and the present example thereby serves to highlight the unusual difficulties which can arise in the case of repeated eigenvalues with non-distinct elementary divisors.

3.3 Summary of Steps to Determine Embeddability

3.3a. Distinct Eigenvalues

The most common eigenvalue configuration for \hat{P} , an empirically determined stochastic matrix, is one in which the roots are distinct. In testing $\log \hat{P}$ for membership in \underline{Q} , we therefore start with this case.

Step 1.—Check that the necessary conditions (2) and (3) in Section 3.1 are satisfied.

Step 2.—Check the eigenvalues of \hat{P} for membership in the heart-shaped zone H_r described in Section 3.1. If this test is passed, proceed to step 3 or 4.

Step 3.—If the eigenvalues of \hat{P} are all real and positive, compute $\log \hat{P}$ using either the power series (2.5), Sylvester's formula (3.14), or the diagonalization transformation (3.11). Only the principal branch of the logarithm ($k=0$ in equation [3.12]) will be real valued, and any of the procedures will yield the *unique* version of $\log \hat{P}$ that can possibly be in \underline{Q} .

Step 4.—If \hat{P} has complex eigenvalues, they must occur in conjugate pairs. For each such pair $(\lambda, \bar{\lambda})$, determine all branches of their logarithms which satisfy Runnenberg's condition,

$$\pi\left(\frac{1}{2} + \frac{1}{r}\right) \leq \arg(\log_k \lambda) \leq \pi\left(\frac{3}{2} - \frac{1}{r}\right), \quad (3.24)$$

where r = order of matrix \hat{P} , $\arg(\log_k \lambda) = \tan^{-1} \left(\frac{\theta + 2\pi k}{\log \rho} \right)$, and k specifies a branch of $\log_k \lambda$ according to²³

$$\log_k \lambda = \log \rho + i(\theta + 2\pi k); \quad k = 0, \pm 1, \pm 2, \dots, \quad 0 < \theta < \pi. \quad (3.25)$$

Now select one of the branches for each complex conjugate pair, and compute $\log \hat{P}$ via (3.11) or by using Sylvester's formula (3.14). Check the resulting matrix for membership in \underline{Q} . Repeat this calculation for all

²³ If $\lambda = a + bi$, then $\rho = |\lambda| = \sqrt{a^2 + b^2}$ and $\theta = \tan^{-1}(b/a)$.

branches satisfying (3.24). Clearly, there are only a finite number of such computations to be performed, and they will yield all versions of $\log \hat{P} \in \underline{Q}$.

In particular, if we represent a pair of complex conjugate eigenvalues $(\lambda, \bar{\lambda})$ by $(\rho e^{i\theta}, \rho e^{-i\theta})$, $0 < \rho < 1$ and $0 < \theta < \pi$, then the number of branches of $\log \lambda$ which need to be examined in testing $\log \hat{P}$ for membership in \underline{Q} is $U(r) + L(r) + 1$, where

$$\left. \begin{aligned} U(r) &= \text{integer part of } \left\lceil \frac{(\log \rho) \tan \left[\pi \left(\frac{1}{2} + \frac{1}{r} \right) \right] - \theta}{2\pi} \right\rceil \\ L(r) &= \text{integer part of } \left\lfloor \frac{(\log \rho) \tan \left[\pi \left(\frac{3}{2} - \frac{1}{r} \right) \right] - \theta}{2\pi} \right\rfloor \end{aligned} \right\} \quad (3.26)$$

and r is the order of the matrix.²⁴ Here $U(r)$ specifies the upper bound to $+k$, and $L(r)$ the lower bound to $-k$, with respect to the multiple-valued logarithm function (3.25). Since the computation of $U(r)$ and $L(r)$ is to be performed for each pair of complex conjugate eigenvalues of \hat{P} , the number of versions of $\log \hat{P}$ that must be examined is

$$\prod_{j=1}^v [U_j(r) + L_j(r) + 1],$$

where v , the upper limit, denotes \hat{P} 's number of complex conjugate eigenvalue pairs. The value of this product will usually be small (frequently $U_j(r) = L_j(r) = 0$ for most j 's). In Section 4.2 we indicate why it is especially rare for a branch other than the principal branch of $\log \hat{P}$ to be in \underline{Q} when the matrix is of low order ($r \leq 3$). In larger arrays, however, one might have to examine multiple versions of $\log \hat{P}$ to determine embeddability.

3.3b. Data Noise and Repeated Eigenvalues

Because our data are commonly contaminated by the effects of sampling variability and measurement error, one cannot be certain that an empirically determined matrix \hat{P} is the correct transition matrix for the population of interest. As a consequence, if the preceding calculations indicate that \hat{P} is not embeddable, but the violations in $\log \hat{P}$ are not severe, a researcher should consider adjusting the observed matrix to a nearby \tilde{P} which is embeddable and continuing his analysis with the modified matrix.

²⁴ These formulas were computed from (3.24) by solving for k (in the arc tangent) at each bound.

Strategies for making such an adjustment usually operate on $\log \hat{P}$, perturbing it to a matrix $Q_0 \in \underline{Q}$, and then estimate \hat{P} , the modified array, via $e^{Q_0} = \hat{P}$.

There are several procedures for altering $\log \hat{P}$ so it will satisfy a priori chosen conditions, such as membership in \underline{Q} . Zahl (1955, p. 98) suggests setting the offending elements (negative q_{ij} 's, $i \neq j$, in the present context) to zero and modifying the main diagonal entries so that the row sum condition, $\sum_j q_{ij} = 0$, will be satisfied. Coleman (1964a, pp. 178-80) uses an iterative routine which forces selected q_{ij} elements to zero in the computation of $\log \hat{P}$, thereby smearing the compensatory adjustments over the remaining nonzero entries. In example 11, we illustrate the adjustment process using yet another procedure, one which minimizes the sum of squared differences between $\log \hat{P}$ and $Q \in \underline{Q}$. General recommendations regarding which of the techniques is advantageous in a particular problem are currently being prepared.

Example 11. Suppose you observe the matrix

$$\hat{P} = \begin{bmatrix} .600 & .330 & .070 \\ .302 & .560 & .138 \\ .380 & .040 & .580 \end{bmatrix},$$

which also appeared in example 3. This matrix has eigenvalues $\lambda_1 = 1$, $\lambda_2 = .370 + .011i$, $\lambda_3 = .370 - .011i$. Applying Runnenberg's condition in the form (3.26), we find that $U = L = 0$; hence only the principal branch of the logarithm needs to be examined for membership in \underline{Q} . Calculating this branch,

$$\log \hat{P} = \begin{bmatrix} -.692 & .639 & .053 \\ .496 & -.733 & .237 \\ .707 & -.144 & -.563 \end{bmatrix},$$

which is not in \underline{Q} since $(\log \hat{P})_{32} = -.144 < 0$. This raises the question of whether a small perturbation of \hat{P} would yield a logarithm in \underline{Q} . To this end, we determine the nearest intensity matrix Q_0 to $\log \hat{P}$, and check whether or not e^{Q_0} represents a "small perturbation" of \hat{P} . The notion of "nearest" will be defined by $\min_{Q \in \underline{Q}} \|\log \hat{P} - Q\|$, where $\|A - B\| =$

$$\sqrt{\sum_{i,j} (a_{ij} - b_{ij})^2}.$$

In the present example, the minimum is obtained for

$$Q_0 = \begin{bmatrix} -.692 & .639 & .053 \\ .496 & -.733 & .237 \\ .635 & 0 & -.635 \end{bmatrix}.$$

Calculation of $e^{Q_0} = \tilde{P}$ yields

$$\hat{P} + (\text{small perturbation}) = \tilde{P} = \begin{bmatrix} .598 & .334 & .068 \\ .298 & .568 & .134 \\ .349 & .104 & .547 \end{bmatrix},$$

and a case might now be made that \hat{P} was not embeddable only because of sampling error or other data noise. To *conclude* that the substantive process actually is Markovian with Q_0 as the governing intensity matrix, tests of the sort described in Section 5, based on three or more time points, must be passed.

Repeated eigenvalues.—From a computational point of view, the notion of repeated eigenvalues means that they agree to within a prescribed finite number of digits. If you take a large random sample of stochastic matrices, then those matrices with repeated eigenvalues tend to occur with a frequency close to zero. On the other hand, the entries in \hat{P} which arise in mobility investigations are often subject to considerable sampling variability and other sources of error. Our concern, therefore, is to know whether a small perturbation in \hat{P} , call it \tilde{P} , would lead to branches of $\log \tilde{P}$ radically different from those of $\log \hat{P}$. These radical differences can occur in passing from a distinct to a repeated eigenvalue matrix, which in turn can be viewed as being "within error distance" of the original distinct eigenvalue matrix. This suggests that a distinct eigenvalue matrix \hat{P} which is compatible with a Markov model *and* has a pair of eigenvalues within a prescribed number of digits of each other should be perturbed to a \tilde{P} with repeated eigenvalues. Then the structure of the continuum should be displayed as in example 10. If the branches of $\log \tilde{P}$ which are in \underline{Q} are sufficiently varied, this would lead us to report that our observations \hat{P} based on data collected at two time points are uninformative about the underlying mobility mechanism.

The additional tasks to be undertaken, then, in a situation where \hat{P} has eigenvalues which are close to being repeated, consist of carrying out the following procedures:

Step 5.—Adjust the observed stochastic matrix \hat{P} so that it will have repeated eigenvalues.

Step 6.—Determine the structure of the continuum using the simulation strategy described in the Appendix, and check whether some part of the continuum is in \underline{Q} .

The task of adjusting \hat{P} so that it will have repeated eigenvalues is not difficult when the eigenvalues close together are complex conjugates. Fortunately, it is this situation which is of primary practical interest. If we represent these eigenvalues in polar form, $(\lambda, \bar{\lambda}) = (\rho e^{i\theta}, \rho e^{-i\theta})$, $0 < \theta < \pi$, where $\theta \approx 0$ or $\theta \approx \pi$, then the corresponding eigenvalues

in $\log \hat{P}$ are $\log \rho \pm i(\theta + 2\pi k)$. We now want to alter \hat{P} so that one of the approximate equalities is replaced by an *exact* equality. For a scalar t , $t \log \hat{P} = Ht \log DH^{-1}$ will have among its eigenvalues $t \log \rho \pm i(t\theta + 2\pi kt)$. Therefore, if we choose $t = t_1 = 2\pi k / (2\pi k + \theta)$ or $t = t_2 = (\pi + 2\pi k) / (\theta + 2\pi k)$, where k is the largest branch number²⁵ that satisfies (3.24), the matrix $\tilde{P} = e^{t \log \hat{P}}$ will have repeated real eigenvalues, either $(\rho^{t_1} e^{i2\pi k}, \rho^{t_1} e^{-i2\pi k}) = (\rho^{t_1}, \rho^{t_1})$ or $(\rho^{t_2} e^{i\pi(2k+1)}, \rho^{t_2} e^{-i\pi(2k+1)}) = (-\rho^{t_2}, -\rho^{t_2})$. This technique is called "riding log \hat{P} ." It was employed in example 2, and it is applied again in Section 4.2.

4. MULTIPLE SOLUTIONS OF $\hat{P} = e^Q$

4.1 Conceptual Overview

The tests outlined in the preceding section permit a researcher to ascertain whether or not an empirically determined matrix $\hat{P}(t_1)$, constructed from observations at times $t = 0$ and $t = t_1$, is compatible with a continuous-time Markov process. When the answer is affirmative, at least one version of $\log \hat{P}(t_1)$ will be in \underline{Q} . In general, as we have observed, it may be necessary to examine several branches of $\log \hat{P}(t_1)$ to resolve the question of embeddability. For instance, when $\hat{P}(t_1)$ has complex eigenvalues, each complex conjugate pair will generate $U + L + 1$ candidates for membership in \underline{Q} .

In discussing the tests in Section 3, our objective was to investigate embeddability; we sought to determine whether *any* of the $\log \hat{P}(t_1)$ candidates was, in fact, a bona fide member of \underline{Q} . In the present section, we shift emphasis and inquire into *how many* versions of $\log \hat{P}(t_1)$ can belong to \underline{Q} . Stated technically, we wish to compute the number of different solutions Q to the equation $e^{Qt_1} = \hat{P}(t_1)$ which have the required structure (2.2). In the discussion that follows, we shall assume $\hat{P}(t_1)$ is embeddable; that is, at least one version of the logarithm is in \underline{Q} .

Under certain conditions, it is possible to guarantee that this solution $Q \in \underline{Q}$ will be *unique*. In particular, this is so whenever one of the following sufficiency conditions is satisfied:

- i) The eigenvalues of $\hat{P}(t_1)$ are distinct, real, and positive.
- ii) $\min \{ \hat{p}_{ii}(t_1) \} > 1/2$, where $\hat{p}_{ii}(t_1)$ is the diagonal element in the i th row of $\hat{P}(t_1)$.
- iii) $\det \hat{P}(t_1) > e^{-\pi} = .0432$.

The first criterion derives from the fact that only the principal branch of $\log \hat{P}(t_1)$ is real valued under the indicated eigenvalue constraints.

²⁵ k may be positive or negative. The sign is chosen according to whether one wants to "move backward" to a repeated eigenvalue situation ($+k$) or "move forward" ($-k$). Note also that $k = 0$ will not generate a continuum at $\theta = 0$. These issues are addressed in greater detail in Sections 4.2 and 4.3.

Also, in this circumstance, the assessment that $Q = (1/t_1) \log \hat{P}(t_1) \in \underline{Q}$ will be unique is independent of the choice of t_1 , since the eigenvalues of $P(t)$ generated by such a Q retain the specified properties for all times t . Additionally, when the eigenvalues satisfy (i), the series formula (2.5) will converge to the unique version of the logarithm in \underline{Q} .

The second and third criteria were established by Cuthbert (1972, 1973) and refer to the specific times t in the evolution of $P(t) = e^{Qt}$ at which the solution $Q \in \underline{Q}$ will be unique. For the purpose of model identification, conditions (ii) and (iii) reveal that *every* Markov chain (identified by a matrix $Q \in \underline{Q}$ via the relation $P(t) = e^{Qt}$) has an interval of time $[0, T]$ during which only one version of $\log P(t_1)$, $0 < t_1 < T$, is in \underline{Q} . [The location of the uniqueness interval at the origin follows from the fact that $L(t)$, the number of branches of $\log P(t)$ in \underline{Q} , is a nondecreasing function of time—see fig. 6, Section 4.2.]

These comments suggest that, in planning an observational study where Markov models are to be utilized for identifying non-directly observable mobility mechanisms (Q -matrices), it is advisable to take the first two observations as close together as possible, while still allowing a representative amount of movement to occur. The question of what constitutes an appropriate time interval is clearly tied to the nature of the particular substantive process. The point to be highlighted here is that, because of the complications which arise when there are multiple solutions, this sort of consideration is consequential in developing sampling strategies for situations where the number of time points at which data can be collected is very restricted.

Except when one of the special conditions (i), (ii), or (iii) is satisfied, it is possible for several branches of $\log \hat{P}(t_1)$ to be in \underline{Q} . This nonuniqueness phenomenon, illustrated in the examples below, has received very little attention in scientific disciplines (physics, engineering, sociology) in which Markov processes are frequently utilized. Nonetheless, the existence of multiple solutions $Q \in \underline{Q}$ to the equation $e^{Qt_1} = \hat{P}(t_1)$ is not at all uncommon.

Example 12. Consider the empirically determined matrix²⁶

$$\hat{P}(t_1) = \begin{bmatrix} .234 & .252 & .264 & .250 \\ .252 & .237 & .245 & .266 \\ .268 & .255 & .230 & .247 \\ .248 & .271 & .248 & .233 \end{bmatrix}.$$

²⁶ The relative closeness of this array to the equilibrium matrix is not a requirement for the existence of multiple branches, except for small order arrays such as 3×3 and 4×4 . See n. 29 below on this point.

This array can be represented in the form e^{Qt_1} , $Q \in \underline{Q}$ and $t_1 = 1$, with either of the matrices

$$Q_1 = \begin{bmatrix} -3.350 & 0.134 & 0.067 & 3.149 \\ 3.132 & -3.306 & 0.144 & 0.030 \\ 0.035 & 3.233 & -3.395 & 0.127 \\ 0.137 & 0.033 & 3.149 & -3.319 \end{bmatrix}$$

or

$$Q_2 = \begin{bmatrix} -3.329 & 3.312 & 0.005 & 0.012 \\ 0.033 & -3.337 & 3.209 & 0.095 \\ 0.016 & 0.023 & -3.334 & 3.295 \\ 3.294 & 0.050 & 0.027 & -3.371 \end{bmatrix}$$

From the perspective of uncovering structural mechanisms, the matter of identifying the "correct" Q for an empirical process must be a central consideration, because the alternative intensity matrices consistent with the mathematical formalism $\dot{P}(t_1) = e^{Qt_1}$ will lead to different substantive conclusions. If only the branch $(1/t_1)\log \dot{P}(t_1) = Q_1$ were recovered, one would assert that the most frequent transitions are $S_1 \rightarrow S_4$, $S_2 \rightarrow S_1$, $S_3 \rightarrow S_2$, and $S_4 \rightarrow S_3$. In contrast, if only the branch $(1/t_1)\log \dot{P}(t_1) = Q_2$ were computed, one would contend that the process evolves principally through the following pattern of movements: $S_1 \rightarrow S_2$, $S_2 \rightarrow S_3$, $S_3 \rightarrow S_4$, and $S_4 \rightarrow S_1$. Since, in applications of continuous-time Markov processes, attention has been directed to the relative magnitudes of the q_{ij} entries and to apportioning these elements among theoretically specified effect parameters (e.g., Coleman 1964a, chap. 6; McDill and Coleman 1963; Bartholomew 1973, chap. 5), identification of the appropriate intensity matrix would appear to be a necessary initial step in this sort of analysis.

This task may be divided into two component issues: (a) recovery of all matrices $Q \in \underline{Q}$ that are compatible with the representation $e^{Qt_1} = \dot{P}(t_1)$ and (b) selection from this list of alternative Q -matrices the correct one for the empirical process at hand. Procedures for accomplishing the first task are presented in the current section. The second issue can be resolved by bringing additional substantive information to bear on the nature of the process to aid in choosing among the alternative Q -matrices, by collecting data at more than two time points, or by sampling the population over a briefer time interval (e.g., within the region of uniqueness). These matters will be considered in Section 5.

4.2 How Multiple Versions of $\log \dot{P}(t) \in \underline{Q}$ Arise

The simplest way to describe how multiple matrices $Q \in \underline{Q}$ originate is to consider the case of a general 3×3 stochastic matrix $P(t)$ which has complex eigenvalues. Expressing this matrix in diagonal form, we have

$P(t) = HD(t)H^{-1}$. For convenience, we write the complex eigenvalues of $P(t)$ as exponentials,

$$D(t) = \begin{bmatrix} 1 & 0 & 0 \\ 0 & \lambda(t) & 0 \\ 0 & 0 & \bar{\lambda}(t) \end{bmatrix} = \begin{bmatrix} 1 & 0 & 0 \\ 0 & e^{t(a+bt)} & 0 \\ 0 & 0 & e^{t(a-bt)} \end{bmatrix}, \quad (4.1)$$

where $\bar{\lambda}(t)$ denotes the complex conjugate of the eigenvalue $\lambda(t)$. Then $\log \dot{P}(t) = H \log D(t)H^{-1}$, in which

$$\log D(t) = \begin{bmatrix} 0 & 0 & 0 \\ 0 & at + i(bt + 2\pi k) & 0 \\ 0 & 0 & at - i(bt + 2\pi k) \end{bmatrix},$$

$$k = 0, \pm 1, \pm 2, \dots \quad (4.2)$$

We specify $b > 0$. Also note, for reference, that because $|\lambda(t)| < 1$ for all t , $at = \log |\lambda(t)| < 0$.

Applying Runnenberg's necessary condition for embeddability (3.24), we have

$$\frac{5\pi}{6} \leq \tan^{-1} \left(\frac{bt + 2\pi k}{at} \right) \leq \frac{7\pi}{6}, \quad k = 0, \pm 1, \pm 2, \dots, \quad (4.3)$$

where the inverse tangent specifies $\arg(\log \lambda(t))$ in (3.24). For a fixed t , we therefore have a series of tests, one for each integer (branch) k . The point to be emphasized here is that, since every branch of $\log P(t)$ whose eigenvalues satisfy (4.3) is a candidate for membership in \underline{Q} , more than one version of the logarithm may, in fact, be in \underline{Q} . It is also the case that, as t increases and $P(t)$ evolves to the equilibrium matrix of the process, the number of branches of $\log P(t)$ that are potentially in \underline{Q} becomes larger. These phenomena are illustrated in figures 4 and 5.

Figure 4 displays the locations of various branches²⁷ of the eigenvalues $\log \lambda(t) = (a \pm bi)t + 2\pi k$, $t = 1$, in relation to Runnenberg's criterion. The wedge-shaped region (solid lines) defines the boundaries of this necessary condition for embeddability—all eigenvalues of $\log P(t)$ must lie in the zone. In this illustration, only the principal branch ($k = 0$) is located in the wedge-shaped region; other branches of the logarithm, which differ by multiples of 2π in their imaginary parts, lie outside the wedge.

Now consider the effect of letting t increase. With respect to the principal branch of $\log \lambda(t)$, $\tan^{-1}(bt/at) = \tan^{-1}(b/a)$, and hence the

²⁷ For brevity in the discussion, we focus on positive branches ($k > 0$). An analogous description can be presented for negative branches of the logarithm.

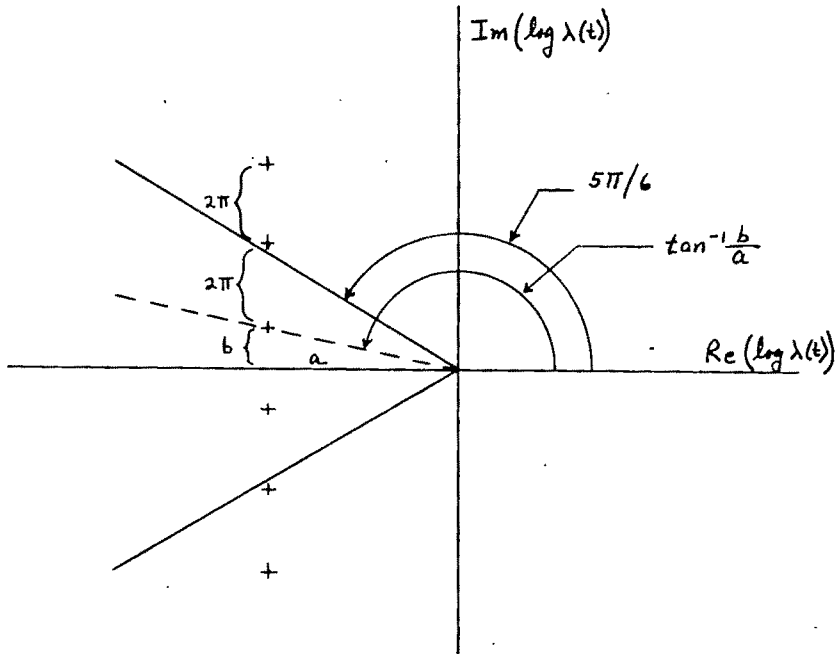


FIG. 4.—Eigenvalues of $\log P(t)$, for $t = 1$. The pair of crosses closest to the negative real axis depicts the principal branch of the logarithm, $\log \lambda = a \pm bi$. The pair next further out represents the branch for $k = 1$, i.e., $\log \lambda = a \pm i(b + 2\pi)$, and so forth.

argument of the logarithm is unchanged. With regard to any other branch $k > 0$, since

$$\frac{bt + 2\pi k}{at} = \frac{b + 2\pi k/t}{a} > \frac{b + 2\pi k}{a}$$

(the inequality follows because $a < 0$) and since $\tan^{-1}x$ is an increasing function of x in the second quadrant, we have

$$\begin{aligned} \arg(at + i[bt + 2\pi k]) &= \tan^{-1}([bt + 2\pi k]/at) \\ &> \tan^{-1}([b + 2\pi k]/a) = \arg(a + i[b + 2\pi k]). \end{aligned}$$

This calculation shows that the angle made by a branch of the logarithm ($k > 0$), with respect to the positive real axis, enlarges with time. As a result, additional branches enter the wedge, and the number of versions of $\log P(t)$ that are candidates for membership in \underline{Q} increases. This phenomenon is illustrated in figure 5.

If we let $L(t) = \{\text{number of branches of } \log P(t) \in \underline{Q} | \text{given } t\}$, the next relevant considerations are: (i) $L(t)$ itself is a monotone nondecreas-

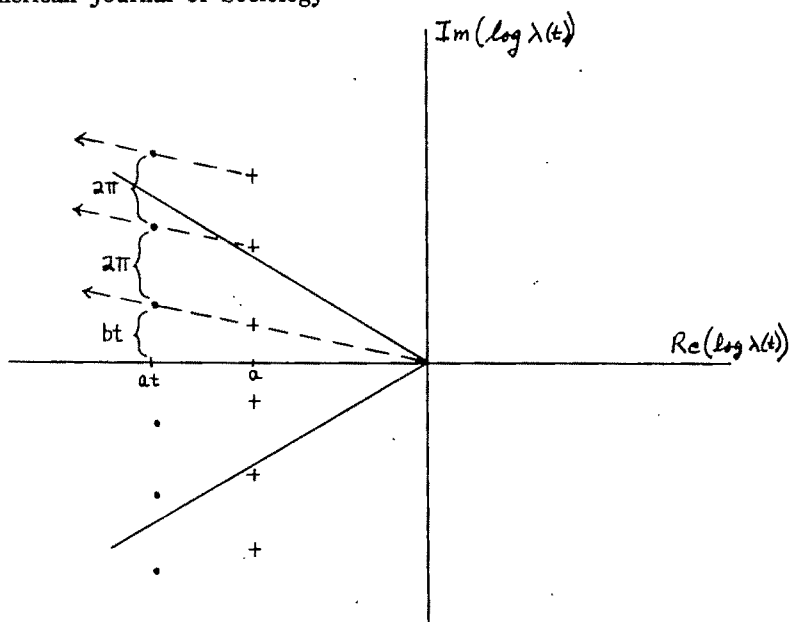


FIG. 5.—Trajectories of the eigenvalues of $\log P(t)$, as a function of time. The dashed lines with arrowheads show the trajectories of the branches of $\log \lambda(t)$, as a function of t .

ing function of t (except, possibly, for isolated time points), and (ii) if $L(t) > 1$ at some time t (other than one of the isolated time points), then $L(t) \rightarrow \infty$ as $t \rightarrow \infty$ (Cuthbert 1972, 1973). The graph of $L(t)$ in figure 6 is the prototype for the evolution of any Markov chain where Q

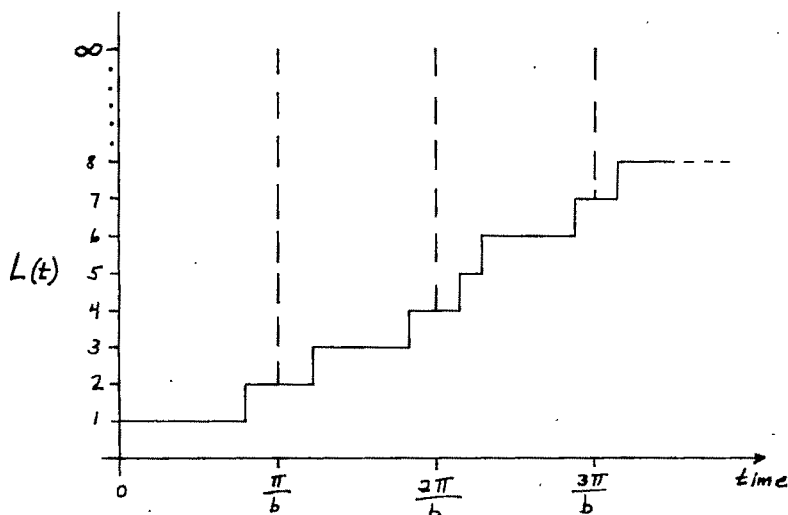


FIG. 6.—Number of branches of $\log P(t)$ in \underline{Q} , as a function of time

has distinct eigenvalues and at least one complex conjugate pair. At times $t = \pi/b, 2\pi/b, 3\pi/b, \dots, n\pi/b, \dots$, the complex conjugate eigenvalues of $P(t)$ will equal $\exp[an\pi/b \pm i(n\pi + 2\pi k)]$, $n = 1, 2, 3, \dots$, and $k = 0, \pm 1, \pm 2, \dots$. This expression reduces to one of the multiple real-root conditions, either $\lambda_2(t) = \lambda_3(t) = \exp(an\pi/b)$ or $\lambda_2(t) = \lambda_3(t) = -\exp(an\pi/b)$, according to whether n is even or odd. The point to be stressed is that, at these times, $P(t) = e^{Qt}$ has repeated eigenvalues with nondistinct elementary divisors, which will give rise to a continuum of branches of $\log P(n\pi/b)$.

From the point of view of model identification—determining the correct $Q \in \underline{Q}$ for a substantive process—these times are a source of difficulty, because their locations are a priori unknown. Knowledge of $\log \hat{P}(t_1)$, where $\hat{P}(t_1)$ has the same structure as $P(n\pi/b)$ in the preceding illustration, can be useless for making statements about the propensity of individuals to move between particular states.²⁸ If many observations in time were to be allowed in a particular study, we could prepare sampling plans for model identification which would be relatively uninfluenced by this phenomenon. With observations at only two, three, or four time points being a constraint in most studies, however, a single uninformative matrix $\hat{P}(t_i)$ can make a considerable difference in the available information for identifying the Q -matrix underlying a substantive process.

With general r -state matrices, the preceding discussion is complicated by the possible presence of more than one pair of complex conjugate eigenvalues. The graph of $L(t)$ (fig. 6) would then be altered in two ways: first, there would be additional isolated time points at which $L(t) = +\infty$. These correspond to the instants at which the added complex eigenvalues have zero imaginary parts and become repeated real roots. Second, the rise in the step function can be much steeper. This is because the wedge-shaped region (fig. 4), which determines the number of branches of $\log \lambda(t)$ that can generate candidates for membership in \underline{Q} , widens as a function of r , the order of the matrix. This phenomenon is illustrated in figures 7, 8, and 9.

Figure 7 displays the wedge-shaped zones for general 3-state and 6-state matrices; the respective angles made with the positive real axis are determined by the inequalities (4.3). From the illustrative representation of an eigenvalue of $\log \hat{P}$ and its complex conjugate, we see that, while only the principal branch lies in the wedge for 3×3 matrices, two additional branches would be candidates for membership in \underline{Q} if this same eigenvalue belonged to the larger array. It is this fact, together with the presence of additional complex conjugate eigenvalues to generate candidates for membership in \underline{Q} , which prompted our remark in Section 3.3 to

²⁸ The same remark holds for a \hat{P} which is considered to be within error distance of $P(n\pi/b)$.

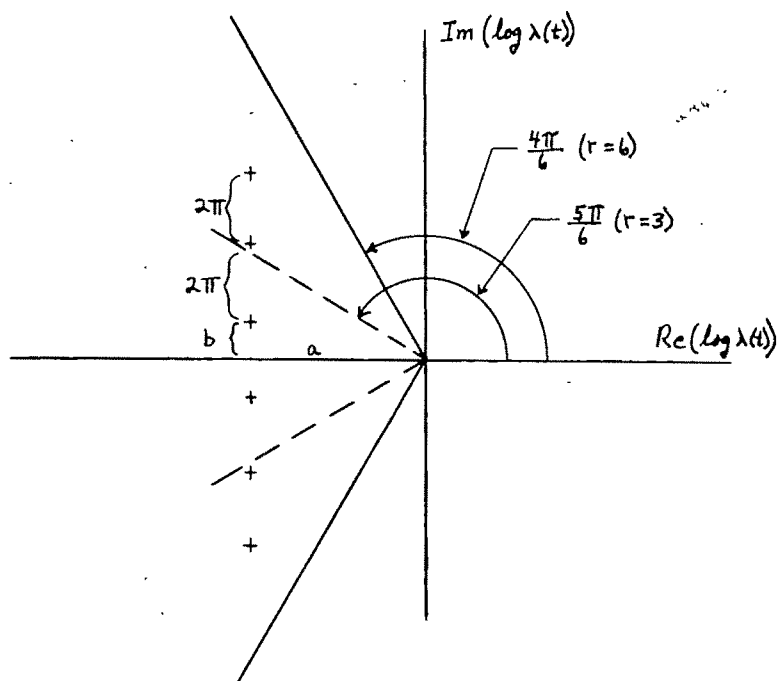


FIG. 7.—Runnenberg's wedge criterion, illustrated for 3×3 and 6×6 matrices, for $t = 1$.

the effect that the number of branches which must be checked for embeddability increases directly with the order of \hat{P} . In the context of the present discussion, we emphasize that the computations are more likely to produce multiple versions of $\log \hat{P} \in \underline{Q}$ in large-order arrays.

Figure 8 presents the same information as figure 7, but from a different perspective. The preceding plot depicted the constraints on the branches of the eigenvalues of $\log \hat{P}$, as they relate to eligibility for membership in \underline{Q} . In figure 8 we display the conditions on the eigenvalues of \hat{P} , in the case of 4×4 , 6×6 , 12×12 , and 20×20 matrices, for it to generate at least two candidates for membership in \underline{Q} . We thereby see in a more direct fashion how the constraints are relaxed as the matrix size is increased.²⁰ Finally, in figure 9 we show the restrictions for different numbers of logarithms to be eligible for membership in \underline{Q} , in the particular instance of a 20×20 array. The outer, heart-shaped region, labeled H_{20} , is a graph of Runnenberg's necessary conditions: all eigenvalues of \hat{P}

²⁰ In connection with this point, we refer the reader to the 4×4 matrix $\hat{P}(t_1)$ in example 12. The reason why it is reasonably similar to the equilibrium matrix for the process can now be appreciated; namely, the complex conjugate eigenvalues are close to zero in magnitude.

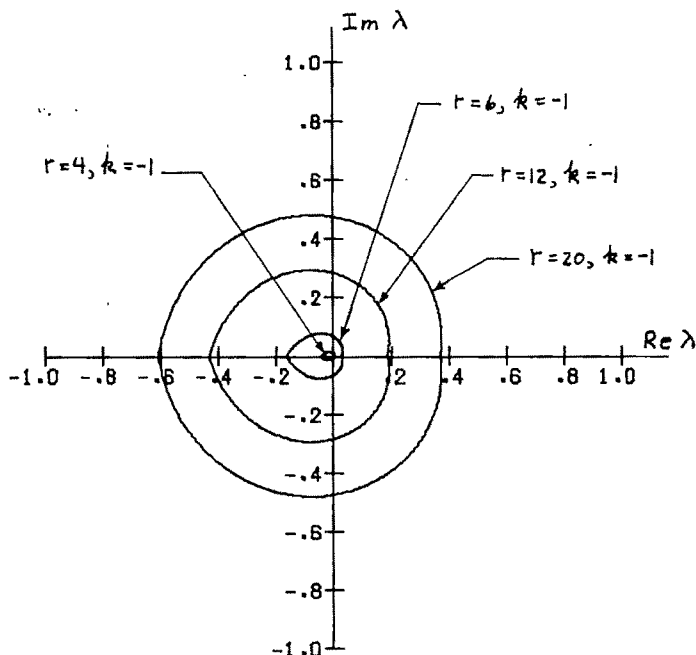


FIG. 8.—Eigenvalue regions of \hat{P} in which two versions of $\log \hat{P}$ are candidates for membership in \underline{Q} , for 4×4 , 6×6 , 12×12 , and 20×20 matrices. Each eigenvalue of \hat{P} interior to the curve relevant to its size will generate at least two logarithm candidates for membership in \underline{Q} .

must lie in this zone for the matrix to be embeddable. The interior curves delineate the regions in which an eigenvalue of \hat{P} will generate multiple branches of $\log \hat{P}$ that can be in \underline{Q} ; for instance, if some eigenvalue λ_j lies interior to the curve labeled " $k = -1$," then each of the two branches of its logarithm,

$$\log \lambda_j = a + bi \quad \text{and} \quad \log \lambda_j = a + i(b - 2\pi),$$

will generate versions of $\log \hat{P}$ which must be examined for membership in \underline{Q} .

The most severe form of nonuniqueness of $\log \hat{P}(t_1)$ occurs for Markov chains $P(t) = e^{Qt}$ having real eigenvalues which remain repeated for *all* $t > 0$, instead of separating into complex conjugates, as was the case in the preceding discussion. The transition mechanisms associated with such chains are by no means pathological from a substantive point of view, and the prototype of this phenomenon is illustrated in the following example.³⁰

³⁰ The remainder of this section is more difficult mathematically and can be skipped at a first reading. Continue with Section 4.3.

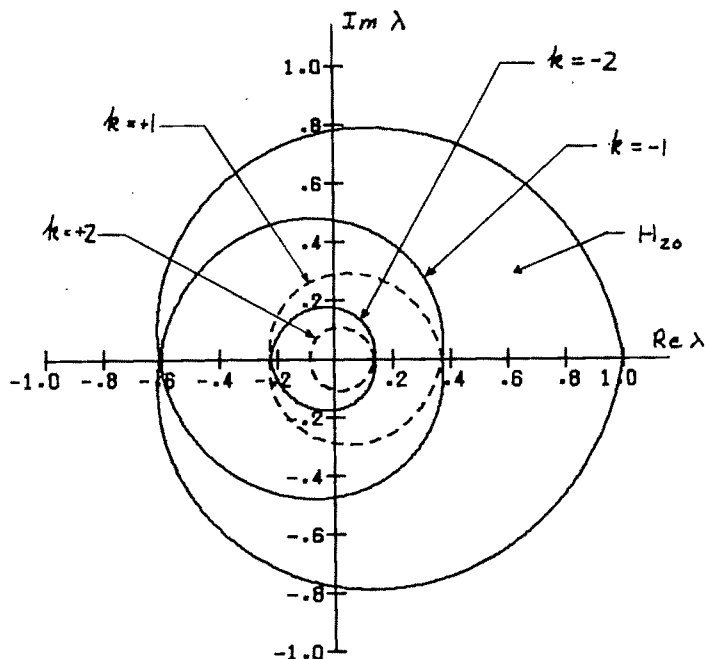


FIG. 9.—Eigenvalue regions of \hat{P} in which multiple versions of $\log \hat{P}$ are candidates for membership in \underline{Q} , for 20×20 matrices. All eigenvalues of \hat{P} must lie in the region H_{20} for \hat{P} to be embeddable. If an eigenvalue is interior to a k -curve, it generates $|k| + 1$ versions of $\log \hat{P}$ which may be in \underline{Q} .

Example 13. Consider the matrix

$$P(t) = \frac{1}{3} \begin{bmatrix} 1 + 2e^{-3t/2} & 1 - e^{-3t/2} & 1 - e^{-3t/2} \\ 1 - e^{-3t/2} & 1 + 2e^{-3t/2} & 1 - e^{-3t/2} \\ 1 - e^{-3t/2} & 1 - e^{-3t/2} & 1 + 2e^{-3t/2} \end{bmatrix}, \quad (4.4)$$

where $t > 0$. $P(t)$ has eigenvalues 1, $e^{-3t/2}$, $e^{-3t/2}$ (which are repeated irrespective of the choice of t) and nondistinct elementary divisors $(\lambda - 1)$, $(\lambda - e^{-3t/2})$, $(\lambda - e^{-3t/2})$. Note that this is the matrix of example 10 with $X = e^{-3t/2}$.

From the discussion of repeated eigenvalues with nondistinct elementary divisors (Section 3.2b), we know that all branches of $(1/t) \log P(t)$ may be computed via

$$\frac{1}{t} \log P(t) = \frac{1}{t} HB \log J(t) B^{-1} H^{-1} \quad (4.5)$$

where H is any similarity transformation that reduces $P(t)$ to diagonal

form (e.g., eq. [3.19]), B is a matrix with complex entries (3.21) which commutes with $J(t)$, and $\log J(t)$ has the form

$$\log J(t) = \begin{bmatrix} 0 & 0 & 0 \\ 0 & \frac{-3t}{2} + 2\pi ki & 0 \\ 0 & 0 & \frac{-3t}{2} - 2\pi ki \end{bmatrix}, \quad (4.6)$$

in which $k = 0, \pm 1, \pm 2, \dots$ specifies branches of the logarithm.

We now describe how a continuum arises in this eigenvalue condition. The first time that the complex eigenvalues in (4.6) satisfy Runnenberg's condition (4.3) with $k \neq 0$ occurs at $t^* = 4\pi/\sqrt{3}$. Before this time only the branch $k = 0$ of $\log J(t)$ will be in the wedge-shaped zone (fig. 4). It can be checked that, when $k = 0$, $B \log J(t) = \log J(t)B$, and therefore equation (4.5) reduces to $(1/t) H \log J(t) H^{-1}$ for every matrix B . This means that at most one version of $(1/t) \log P(t)$ can be in \underline{Q} . Indeed,

$$Q = \frac{1}{t} \log P(t) = \begin{bmatrix} -1 & \frac{1}{2} & \frac{1}{2} \\ \frac{1}{2} & -1 & \frac{1}{2} \\ \frac{1}{2} & \frac{1}{2} & -1 \end{bmatrix} \text{ for } 0 < t < \frac{4\pi}{\sqrt{3}}. \quad (4.7)$$

When $t > \frac{4\pi}{\sqrt{3}}$, a second branch of $\log J(t)$ in (4.6) enters the wedge-shaped zone (see fig. 5). In this circumstance, it is no longer the case that $B \log J(t) = \log J(t) B$, and a continuum of versions of $(1/t) \log P(t)$ will be generated, each version corresponding to a choice of $\{c_{ij}\}$ in B (eq. [3.21]). A bit of computation will show that, if $\{c_{ij}\}$ are restricted according to

- i) $c_{11}c_{12} - c_{21}c_{22} = 0$
- ii) u is real, $u = \frac{c_{11}c_{22} + c_{12}c_{21} + 2c_{21}c_{22}}{c_{11}c_{22} - c_{12}c_{21}}$
 v is real, $v = \frac{c_{11}c_{22} + c_{12}c_{21} - 2c_{21}c_{22}}{c_{11}c_{22} - c_{12}c_{21}}$
- iii) $|ku| < \sqrt{3}t/4\pi$ and $|kv| < \sqrt{3}t/4\pi$

where k is an integer (the branch number), then all choices of $\{c_{ij}\}$ will yield matrices $Q \in \underline{Q}$, and they are summarized by

$$Q = \frac{1}{t} \log P(t) =$$

$$\begin{bmatrix} -1 & \frac{1}{2} + \frac{2k\pi u}{\sqrt{3}t} & \frac{1}{2} - \frac{2k\pi u}{\sqrt{3}t} \\ \frac{1}{2} - \frac{2k\pi v}{\sqrt{3}t} & -1 - \frac{k\pi(u-v)}{\sqrt{3}t} & \frac{1}{2} + \frac{k\pi(u+v)}{\sqrt{3}t} \\ \frac{1}{2} + \frac{2k\pi v}{\sqrt{3}t} & \frac{1}{2} - \frac{k\pi(u+v)}{\sqrt{3}t} & -1 + \frac{k\pi(u-v)}{\sqrt{3}t} \end{bmatrix}$$

for $t > \frac{4\pi}{\sqrt{3}}$. (4.8)

The graph of $L(t)$ versus t for this Markov chain is shown in figure 10. From the point of view of model identification, the second observation t_1 must be taken before $t^* = 4\pi/\sqrt{3}$. After this time, a repeated observation will yield the matrix (4.8), which is completely uninformative about the propensity to move between different states. The fundamental difficulty illustrated by this example is that empirically determined matrices with nondistinct elementary divisors *in which this property is retained through time* may be associated with a continuum of intensity matrices for all times t_1 greater than some threshold t^* . To distinguish this "essential"

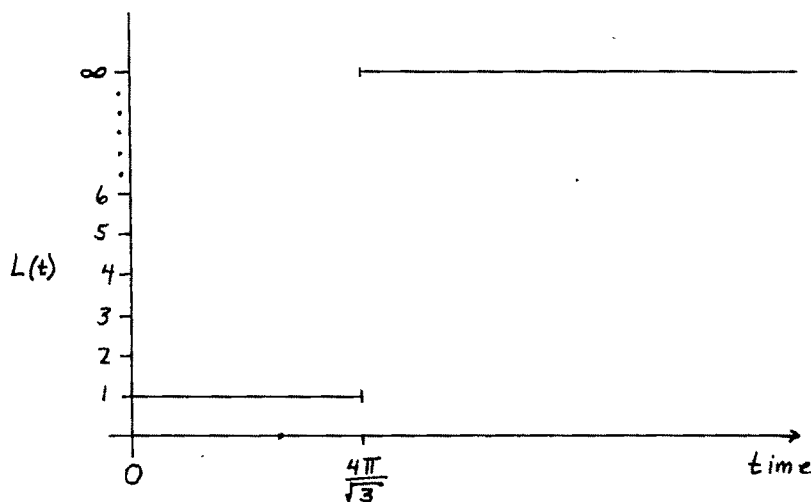


FIG. 10.—Number of branches of $\log \hat{P}(t) \in \underline{Q}$ as a function of time, for $\hat{P}(t)$ in example 13.

continuum case from the chance occurrence of an "isolated" continuum (viz. the points $\pi/b, 2\pi/b, \dots, n\pi/b, \dots$ in Section 4.2), a researcher should check whether the eigenvalues of $\hat{P}(t_1 + \Delta t)$, some $\Delta t > 0$, are repeated with nondistinct elementary divisors when his initial matrix $\hat{P}(t_1)$ has these properties.

4.3 Summary of Correspondence between Eigenvalue Characteristics and Number of Matrices $Q \in \underline{Q}$

The number of versions of $\log \hat{P}$ that can possibly be in \underline{Q} , as this relates to the eigenvalue characteristics of \hat{P} , is summarized in table 1. The left

TABLE 1
EIGENVALUES OF \hat{P} AND THE NUMBER OF MATRICES $Q \in \underline{Q}$

Eigenvalue Characteristics	Embeddable?	How Many Q's?
1. Positive, distinct	Possibly	One
2. Positive, repeated, distinct elementary divisors	Possibly	One
3. Positive, repeated, nondistinct elementary divisors	Possibly	One or continuum
4. Negative, distinct	Never	...
5. Negative, repeated, odd multiplicity	Never	...
6. Negative, repeated, even multiplicity	Possibly	Continuum
7. Complex, distinct, member of a conjugate pair	Possibly	One or multiple
8. Complex conjugate, repeated	Possibly	One, multiple, or continuum
9. Mixture of the types above	Possibly	The most extreme form of nonuniqueness present in any component of the mixture

tab of the table refers to a single eigenvalue of \hat{P} or to a set of eigenvalues sharing a common property (e.g., complex conjugates). The evaluation in the farthest right column assumes that embeddability is met; in other words, that at least one version of $\log \hat{P}$ is in \underline{Q} . In making this evaluation, it is also presumed that the remaining eigenvalues of \hat{P} do not satisfy a condition which is compatible with a greater number of candidates for membership in \underline{Q} ; for instance, all eigenvalues must belong to categories (1) and (2) in order to conclude, on the basis of an examination of eigenvalues alone, that at most one version of $\log \hat{P}$ is in \underline{Q} .

A second point to be noted in connection with the table is that the eigenvalue conditions which rule out embeddability do so by not being compatible with a real-valued version of $\log \hat{P}$. For example, if \hat{P} has a unique negative eigenvalue, $\lambda = -a$ ($a > 0$), its logarithm will be $\log a + ik\pi$, $k = 0, \pm 1, \pm 2, \dots$, which always has a nonzero imaginary part.

The corresponding eigenvector h in the similarity transformation $\hat{P} = H J H^{-1}$ will be *real* valued (since $\lambda = -a$ is distinct and real), and $\log \hat{P} = H \log J H^{-1}$ will have the identical eigenvector corresponding to its *complex* eigenvalue. There is no way in which $\log \hat{P}$ can be real valued in this circumstance. What alters the situation in the case of repeated negative eigenvalues with even multiplicity is that, when the elementary divisors are not distinct, the eigenvectors corresponding to the repeated eigenvalues will be complex conjugates, and real versions of $\log \hat{P}$ can result. In particular, this will occur when different branches of the logarithm of $-a$ are present simultaneously in $\log D$.

Finally, we emphasize that the eigenvalue configurations most commonly found in empirically determined matrices involve combinations of distinct positive and distinct complex conjugates (categories [1] and [7]).

5. TESTING STRATEGIES

5.1 Identification of Structural Parameters

We assume first that the process under observation is time stationary, that the data are free of measurement and classification error, and that the entire population has been surveyed, so sampling variability is not a concern. These assumptions have also been made, though without being noted explicitly, in the preceding sections. In this environment, the identification problem arises when observations are taken at only two time points ($t = 0$, $t = t_1$), and the matrix $\hat{P}(t_1)$ constructed from these observations can be represented in the form $\hat{P}(t_1) = e^{Q t_1}$ for multiple arrays $Q \in \underline{Q}$. A researcher then has the following options:

i) He may bring to bear other information about the substantive process. For instance, if $\hat{P}(t_1)$ were the matrix in example 12, a researcher might have reason to believe that $q_{12} > q_{14}$ and therefore Q_2 , not Q_1 , governs the evolution of the process. Clearly, such a choice can be made only when there is a finite list of intensity matrices, and not when a continuum is present.

ii) If an opportunity exists to collect data at a third time point, it should be selected so as not to be an integer multiple of the initial interval $(0, t_1)$. The reason is that, at multiples of an observation interval, the same list of Q -matrices can reappear; this was the case, for instance, with the times π/b , $2\pi/b$, etc. in figure 6. If, however, the third observation is taken at $t_2 \neq k t_1$, k an integer,⁸¹ then even in the presence of multiple

⁸¹ This recommendation assumes that we have observed the first appearance of a continuum, which will be the most common situation. If we have observed the second

occurrence, the times $t_2 = \frac{k t_1}{2}$ should be avoided. If it is a third occurrence, omit the

branches of $\log \hat{P}(t_1, t_2) \in \underline{Q}$, only one version of the logarithm, Q_0 , will have the property

$$Q_0 = \frac{1}{t_1} \log \hat{P}(0, t_1) = \frac{1}{t_2 - t_1} \hat{P}(t_1, t_2). \quad (5.1)$$

This correspondence will identify the *unique* Q that can be associated with the empirical process.

There is an additional virtue in collecting data at three or more time points. The embeddability problem concerns only the question of compatibility of a *single* stochastic matrix $\hat{P}(t_1)$ —that is, observations at two time points—with a continuous-time Markov process. We have seen that, on the basis of this information alone, it is frequently possible to rule out a Markov structure. However, when data are available from more than two time points, a direct test can also be made of the fundamental dynamic assumption of a first-order Markov process, namely that the future state of the system depends only on current state, not on its history. These additional necessary conditions are specified by tests of the sort

$$\hat{P}(t_i, t_k) = \hat{P}(t_i, t_j) \hat{P}(t_j, t_k), \quad 0 < t_i < t_j < t_k. \quad (5.2)$$

The availability of data at three time points provides the most rudimentary opportunity to check this assumption. Formal statistical tests of the validity of the Markov property are described in Anderson and Goodman (1957) and Billingsley (1961).

Study design considerations.—The potential for nonuniqueness can be minimized at the study design stage. If the use of Markov models is contemplated, the survey times should be chosen close together in time, while still permitting a representative amount of movement to take place. When the number of states is small (say, $r \leq 5$), it should be possible to select t_1 so that $\min_i \{\hat{p}_{ii}(t_1)\} > 1/2$. If $\hat{P}(t_1)$ is embeddable, this condition on the diagonal elements ensures that $\log \hat{P}(t_1) \in \underline{Q}$ will be unique (see Section 4.1). When the number of system states is large, it may not be possible to satisfy this condition and still retain an adequate amount of population movement to estimate $\log \hat{P}(t_1)$ accurately. Even in this circumstance, however, t_1 should be selected reasonably close in time to the initial observation, since the degree of nonuniqueness of $Q \in \underline{Q}$ is a monotone increasing function of time (fig. 6), except for isolated instants such as $\{k\pi/b\}$.

In most data-gathering situations, one has neither a priori information concerning the rate of movement (to assist in selecting the second observation) nor an opportunity to schedule the second wave of a survey accord-

times $t_2 = \frac{kt_1}{3}$, etc. As a practical guide, if a researcher avoids the two sets of time points cited in this footnote, he is unlikely to encounter a second continuum.

ing to these considerations. A more pragmatic suggestion would be to collect detailed retrospective information about the process. Ideally, this should consist of "sample path" data; that is, complete information about a respondent's duration in each system state over the time interval of interest. When such data are deemed too costly to collect, a respondent should be queried regarding his system state at several prechosen time points in the past (e.g., one year ago, two years ago, etc.). Having gathered such information, the researcher may utilize the estimation procedures and model tests that require more than two observations in time.

5.2 Sampling Error and Data Noise

The data available to researchers are commonly contaminated by errors of various sorts. While we may wish to make statements about a *population-level* process, information is usually collected for a *population sample*. Similarly, errors of measurement can result in the misclassification of individuals with respect to system state.

Ordinarily, these are not very serious problems. In many sampling situations, the inference made about a population parameter, using standard statistical procedures, tends to be incorrect to a degree that varies *continuously* with the magnitude of the measurement error. By using distributional statistics, one can put confidence bounds around an estimate and describe the interval in which the population-level parameter lies. However, measurement error and sampling variability carry greater consequence when we seek to identify the non-directly observable structural mechanisms (Q -matrices) that underlie Markov processes. In particular, when an empirically determined matrix $\hat{P}(t_1)$ is in the vicinity of a second stochastic matrix \tilde{P} which can be expressed in the form $\tilde{P} = e^Q$ for multiple versions of $\log \tilde{P} \in \underline{Q}$, then a small error in the estimate of $\hat{P}(t_1)$ can result in the recovery of a matrix $Q \in \underline{Q}$ which, while unique, is *the wrong intensity matrix for the substantive process*.

Example 14. Suppose you observe

$$\hat{P}_1(t_1) = \begin{bmatrix} .232 & .249 & .266 & .253 \\ .254 & .236 & .242 & .268 \\ .270 & .258 & .228 & .244 \\ .245 & .274 & .250 & .231 \end{bmatrix}.$$

This matrix can be written in the form $e^{Q_1 t_1}$, $t_1 = 1$, for a *unique* version of $\log \hat{P}(t_1) \in \underline{Q}$,

$$Q_1 = \begin{bmatrix} -3.216 & 0.129 & 0.064 & 3.023 \\ 3.007 & -3.174 & 0.138 & 0.029 \\ 0.034 & 3.104 & -3.260 & 0.122 \\ 0.132 & 0.032 & 3.023 & -3.186 \end{bmatrix}.$$

If one believed $\hat{P}_1(t_1)$ to be error free, it would be reasonable to conclude that Q_1 describes the evolution of the dynamic process. However, in a fallible environment, a second survey of the same population would produce a slightly different observed matrix. Consider

$$\hat{P}_2(t_1) = \begin{bmatrix} .231 & .255 & .266 & .248 \\ .250 & .234 & .247 & .269 \\ .271 & .252 & .227 & .250 \\ .251 & .275 & .245 & .229 \end{bmatrix}.$$

No element of this matrix differs from its counterpart in $\hat{P}_1(t_1)$ by an amount in excess of .006 in magnitude, so it is not unreasonable to suggest that the two matrices represent different samples from a single parent population. However, while it is the case that $\hat{P}_2(t_1)$ is also compatible with a continuous-time Markov process for a *unique* $Q \in \underline{Q}$, this intensity matrix is given by

$$Q_2 = \begin{bmatrix} -3.164 & 3.148 & 0.005 & 0.011 \\ 0.031 & -3.170 & 3.049 & 0.090 \\ 0.015 & 0.022 & -3.167 & 3.130 \\ 3.130 & 0.048 & 0.026 & -3.204 \end{bmatrix}.$$

Matrices Q_1 and Q_2 represent very different structural mechanisms and would lead to contrary conclusions about the nature of the substantive process. What has happened is that, while $\hat{P}_1(t_1)$ and $\hat{P}_2(t_1)$ are each compatible with the representation e^{Qt_1} and have unique logarithms in \underline{Q} , the two empirically determined P -matrices lie in the vicinity of a third, \tilde{P} , which in turn can be represented as a Markov process for multiple matrices $Q \in \underline{Q}$. Indeed,

$$\tilde{P} = \hat{P}_1(t_1 + .05) = e^{(t_1 + .05)Q_1}$$

$$\tilde{P} = \hat{P}_2(t_1 + .05) = e^{(t_1 + .05)Q_2}$$

and this common P -array is the same one presented in example 12 to illustrate the phenomenon of multiple intensity matrices.³²

Specific error structures.—In the context of sampling variability or measurement error, then, a researcher cannot assume that, because $\hat{P}(t_1) = e^{Qt_1}$ for a unique $Q \in \underline{Q}$, this intensity matrix describes the evolution of the substantive process. He must either remove the error from the observed matrix and use the “purged” array for estimating structural parameters or examine the intensity matrices of other P ’s that are within “error distance” of his empirically determined matrix.

³² The Q -matrices in example 12 are the ones in this illustration multiplied by $t = 1.05$.

Misclassification error can be incorporated formally in a description of observed transition matrices by introducing the representation

$$\hat{P}(t_i, t_j) = \bar{P}(t_i, t_j) \circ E(t_i, t_j) \quad 0 \leq t_i < t_j, \quad (5.3)$$

where $\hat{P}(t_i, t_j)$ is an empirically determined $r \times r$ matrix of transition probabilities based on observations at times t_i and t_j ; $\bar{P}(t_i, t_j)$ is a fitted $r \times r$ matrix of transition probabilities representing the error-free or purged mobility structure; $E(t_i, t_j)$ is an $r \times r$ matrix of residuals interpreted as errors due to misclassification; and the symbol \circ denotes either *addition* or *multiplication*. Motivating the representation (5.3) is the view that matrix \bar{P} , rather than \hat{P} , should be tested for compatibility with a Markov process and Q should be estimated from the equation $\bar{P} = e^Q$.

Calculation of \bar{P} and E must be based on an assumed model of the error structure, together with independent estimates of the parameters. For example, if the states are occupational categories and there is a natural ordering among them (e.g., on the basis of a prestige scale), an individual who actually moves from state i at time t_1 to state j at time t_2 may have probability c_1 of being recorded in state $j-1$ at time t_2 , probability c_2 of being recorded in state $j+1$ at time t_2 , and probability $1-c_1-c_2$ of being recorded correctly. If this kind of measurement error is believed to operate, it implies a representation of the form

$$\hat{P}(t_1, t_2) = \bar{P}(t_1, t_2)E \quad (5.4)$$

where

$$E = \begin{bmatrix} 1-c_1 & c_2 & 0 & 0 & \mathbf{0} \\ c_1 & 1-c_1-c_2 & c_2 & 0 & \\ 0 & c_1 & 1-c_1-c_2 & c_2 & \\ & & \cdot & \cdot & \cdot \\ \mathbf{0} & & & c_1 & 1-c_2 \end{bmatrix}.$$

Given c_1 and c_2 based on independent misclassification estimates, we could solve the matrix equation (5.4) for $\bar{P}(t_1, t_2)$. See Coleman (1964b) for approaches of this sort to the study of change in a fallible environment.

Random error.—In general, a formal model of the error structure will not be available, yet we may wish to make allowance for the effect of “noise” in the data. We recommend a strategy of “exploring” a neighborhood of the observed matrix $\hat{P}(t_1)$, to ascertain whether nearby P -arrays are compatible with intensity matrices that are very different from the initial Q -matrix.

A reasonable procedure for exploring a neighborhood of $\hat{P}(t_1)$ would be to “ride” its associated intensity matrix Q_0 . By this is meant computing

$P(t)$ from the representation $P(t) = e^{Q_0 t}$, using for t the values $t_1 - \Delta t$, $t_1 - 2\Delta t$, ..., $t_1 - h\Delta t$, and $t_1 + \Delta t$, $t_1 + 2\Delta t$, ..., $t_1 + k\Delta t$, where the termination points h and k are the last times that $P(t_1 - j\Delta t)$ and $P(t_1 + l\Delta t)$ can be considered "within sampling or measurement error" of the observed matrix. Next, examine the eigenvalues in the sequence of matrices:

a) If there is a complex conjugate pair $(\lambda, \bar{\lambda}) = (a \pm bi)$ whose imaginary part passes through zero, then $\hat{P}(t_1)$ is in a neighborhood of some matrix \tilde{P} which has repeated real eigenvalues. Associated with this array, a continuum of matrices $Q \in \underline{Q}$ will satisfy the relation $\tilde{P} = e^Q$. Strategies for exploring the structure of a continuum are discussed in the Appendix.

b) If a continuum does not occur within error distance, recover all matrices $Q \in \underline{Q}$ that are compatible with the representation $P(t_1 + k\Delta t) = e^{Q_0(t_1 + k\Delta t)}$, where k was chosen as the forward stopping point of the sequence of P -matrices.³⁸ The complete solution to the problems of determining the number of candidates for membership in \underline{Q} and computing all versions of $\log P \in \underline{Q}$ was presented in Section 3.

If it is the case that $\log \hat{P} \in \underline{Q}$ is unique under the perturbations of $\hat{P}(t_1)$, this intensity matrix can be viewed as the sole mobility structure compatible with a Markov formulation of the substantive process. Stated more transparently, additional samples from the same population can be expected to produce similar Q -matrices. In contrast, if multiple mobility mechanisms $Q \in \underline{Q}$ are found for matrices \tilde{P} within error distance of the observed array $\hat{P}(t_1)$, one of the procedures described in Section 5.1 for selecting among alternative intensity matrices must be utilized.

In an environment containing error, the advantages of collecting data at three or more points in time are especially apparent. We noted earlier (Section 5.1) that three time points are the minimum number for a direct test of the dynamic assumption underlying a first-order Markov process, that is, for checking that

$$\hat{P}(0, t_2) = \hat{P}(0, t_1) \hat{P}(t_1, t_2), \quad 0 < t_1 < t_2. \quad (5.5)$$

In practice, this entails evaluating whether $||\hat{P}(0, t_2) - \hat{P}(0, t_1) \hat{P}(t_1, t_2)|| < \epsilon$, for $\epsilon > 0$, and some suitably chosen norm (e.g., $||A|| = \sqrt{\sum_{i,j} a_{ij}^2}$).

When (5.5) is satisfied and it is also the case that

$$\frac{1}{t_1} \log \hat{P}(0, t_1) \approx \frac{1}{t_2 - t_1} \log \hat{P}(t_1, t_2) \approx \frac{1}{t_2} \log \hat{P}(0, t_2), \quad (5.6)$$

³⁸ Because $L(t) = \{\text{the number of branches of } \log P(t) \in \underline{Q}\}$ is a nondecreasing function of time (except for isolated occurrences of continua), it is not necessary to examine points earlier than t_1 .

we would define Q , the common intensity matrix for the process, as an average of these three estimates. In the presence of sampling or measurement error, then, collection of data at three or more time points permits a test of the fundamental Markov assumption and also facilitates an accurate calculation of Q , through the pooling of several estimates.

In an instance where (5.5) is satisfied but equation (5.6) is not, the process will still be Markovian, though it is no longer time stationary. This leads to the problem of testing observed matrices for compatibility with a time-homogeneous Markov model (the null hypothesis) against special non-time-homogeneous alternatives. We hope to discuss this important issue in a future publication.

As a final comment on analytic strategy in the context of data noise, we emphasize that, while the occurrence of multiple matrices $Q \in \underline{Q}$ may not be very common in an error-free environment, it characterizes the normal work situation when data are fallible. This is because we advise a researcher to examine a neighborhood of an observed $\hat{P}(t_1)$ for the presence of additional intensity matrices and to consider each recovered $Q \in \underline{Q}$ as possibly governing the evolution of the empirical process. Due to data noise, then, we suggest *creating* a multiple $Q \in \underline{Q}$ situation when an observed transition matrix has associated with it a unique logarithm in \underline{Q} . For this reason, collection of data at three or more time points should be a routine requirement when the use of Markov models is contemplated.

6. CONCLUSIONS

The point of departure for this study was the gross misunderstanding among researchers concerning which stochastic matrices are compatible with a continuous-time Markov process having stationary transition probabilities. We noted that the power-series representation of the logarithm of a matrix (eq. [2.5])—the principal formula used in estimating the structural parameters that govern the evolution of a Markov process—permits an intensity matrix to be recovered only for a subset of this class of stochastic models. By resorting, instead, to the spectral-decomposition representation, we were able to estimate intensity matrices for Markov models in instances where (2.5) does not converge; that is, in cases of transition arrays which Coleman and others have considered not to be compatible with this mathematical structure. In the course of the investigation, we also raised new issues which a researcher must consider; these include, principally, the possibility that multiple intensity matrices may be compatible with an empirically determined transition array and the fact that, as a result of data “noise,” recovery of a unique $Q \in \underline{Q}$ does not preclude the possibility that the observed process is governed by an entirely different intensity matrix.

In subsequent papers, we intend to address two additional issues which a researcher desiring to use Markov models in a flexible and creative manner must entertain: (a) How should a priori restrictions be placed on the elements of a Q -matrix, and (b) how can a researcher discriminate among the alternative mathematical models which, on substantive grounds, provide reasonable descriptions of his data? The first topic was mentioned, in passing, in Section 3.3, when we sought to adjust a nonembeddable $\log \hat{P}$ to a neighboring $Q \in \underline{Q}$. More generally, we may wish to estimate the parameters of a sociological theory which specifies that certain instantaneous transitions are prohibited (see Coleman 1964a, chaps. 4 and 5, for examples). The second topic refers to testing data for compatibility with a subset of Markov models (such as birth and death processes) versus general finite-state Markov processes and to comparing the fit of Markov models with that of other mathematical structures, such as mixtures of Markov processes or semi-Markov processes.

As a final point, we emphasize that the problems addressed in this paper cannot be avoided by employing a *discrete-time* Markov framework in place of a continuous-time formulation. In the discrete-time model, the counterpart to the task of estimating $Q \in \underline{Q}$ entails recovering the one-step transition matrix for an empirical process; that is, taking the appropriate root of the observed matrix \hat{P} . Like a logarithm, a root is a multiple-valued function, so the problem of nonuniqueness which we have discussed here arises also in that formulation. Conceptually, the discrete-time model embodies a further difficulty: because most social processes evolve continuously, there usually isn't a compelling reason for preferring one specification of the unit time interval to another. (For instance, in studying *intragenerational* occupational mobility, should the unit time interval be five years or three years or six months?) Yet this is a question of great consequence, because an empirically determined matrix (estimated, let us say for this illustration, from observations ten years apart) may be consistent with a discrete-time Markov structure *for some choices of the unit time interval but not for other choices* (see Singer and Spilerman 1974, pp. 360–63, for an example). Where no substantive meaning can be attached to a particular interval length,⁸⁴ this does not imply that the unit time interval can be specified at the convenience of the researcher or that tests of the sort described here can be ignored. Rather, it suggests that the appropriate mathematical structure is a continuous-time formulation, the procedures for which have been discussed in this paper.

⁸⁴ Examples of instances where a substantive meaning *can* be attached to an interval length and for which discrete-time Markov chains provide an appropriate analytic structure are (i) the popular preference in presidential elections—interval length equals four years—and (ii) school grades for a cohort or an individual—interval length equals one semester.

APPENDIX

Exploring a Continuum⁸⁵

In the case where \hat{P} has repeated eigenvalues and nondistinct elementary divisors, the value of $\log \hat{P}$ depends on the choice of similarity transformation that is used to reduce \hat{P} to Jordan form. A computer-based strategy to test a representative collection of branches of $\log \hat{P}$ for membership in \underline{Q} is the most direct approach we can currently recommend for deciding on compatibility of \hat{P} with a continuous-time Markov model. If a branch of $\log \hat{P}$ which belongs to \underline{Q} is discovered during the computer tests, then it can be shown that there is in fact a *continuum* of branches which are in \underline{Q} . The testing strategy outlined below and illustrated in a simple example is also designed to give some indication of the extent of the continuum of branches which are in \underline{Q} .

Step 1.—Compute one similarity transformation H which reduces \hat{P} to Jordan form. The method of computation is entirely at the discretion of the researcher (see Gantmacher 1960, chap. 6, for suggestions).

Step 2.—Take a random sample of points in an 8-dimensional square region with center at the origin.⁸⁶ For each sample of 8 numbers, use them as the real and imaginary parts of the parameters in the matrices B which commute with $J = H^{-1} \hat{P} H$. Then evaluate

$$\log \hat{P} = HB \log J B^{-1} H^{-1}$$

where

$$B = \begin{bmatrix} 1 & 0 & 0 \\ 0 & c_{11} & c_{12} \\ 0 & c_{21} & c_{22} \end{bmatrix}, \quad \log J = \begin{bmatrix} 0 & 0 & 0 \\ 0 & \log \lambda_2 & 0 \\ 0 & 0 & \overline{\log \lambda_2} \end{bmatrix},$$

the $\{c_{ij}\}$ are given by

$$\begin{aligned} c_{11} &= x_{11} + iy_{11} & c_{12} &= x_{12} + iy_{12} \\ c_{21} &= x_{21} + iy_{21} & c_{22} &= x_{22} + iy_{22} \end{aligned}$$

$\{x_{ij}\}, \{y_{ij}\}$ are the 8 numbers associated with each sample point, and $\overline{\log \lambda_2}$ denotes the complex conjugate of $\log \lambda_2$. Note whether this branch of $\log \hat{P}$ is in \underline{Q} . Several hundred such evaluations may be necessary in order to identify those matrices B , if any, which yield versions of $\log \hat{P} \in \underline{Q}$.

⁸⁵ The Appendix is more difficult mathematically and can be skipped at a first reading.

⁸⁶ We recommend beginning this search in the 2-dimensional subspace defined by the conditions $c_{11} = c_{22}$, $c_{12} = c_{21}$, real. Then extend the search to the 4-dimensional space defined by the restriction that $\{c_{ij}\}$ be real, and finally introduce complex numbers in the full 8-dimensional space. Improved strategies for exploring this kind of continuum are currently in the preliminary development stage.

The preceding computations do not increase in complexity for $r \times r$ matrices having a single pair of repeated real roots, which is the situation most likely to arise. In this general case, B will have the form

$$B = \begin{bmatrix} 1 & 0 & & & & & & 0 \\ 0 & a_{11} & & & & & & \\ & & \ddots & & & & & \\ & & & a_{vv} & 0 & 0 & & \\ & & & 0 & c_{11} & c_{12} & & \\ & & & 0 & c_{21} & c_{22} & & \\ & & & 0 & 0 & 0 & a_{v+3, v+3} & \\ & & & & & & \ddots & \\ & & & & & & & a_{rr} \\ 0 & & & & & & & \end{bmatrix}.$$

However, since only the $\{c_{ij}\}$ generate a continuum, the same simulation as before is involved. In carrying out these computations, the reader is reminded that, if λ_2 is a repeated *negative* root, the simulation must be performed for all branches of $\log \lambda_2 = \log |\lambda_2| + i(\pi \pm 2\pi k)$ which satisfy Runnenberg's necessary condition for embeddability (eq. [4.3]). If λ_2 is a repeated *positive* root, the calculations must be carried out for all branches, except $k=0$, of $\log \lambda_2 = \log |\lambda_2| \pm 2\pi ki$ which satisfy Runnenberg's condition. (The case $k=0$ can produce at most one version of $\log \hat{P} \in \underline{Q}$ —see example 13.)

Example 15. Recall the matrix of example 10,

$$\hat{P} = \frac{1}{3} \begin{bmatrix} 1+2x & 1-x & 1-x \\ 1-x & 1+2x & 1-x \\ 1-x & 1-x & 1+2x \end{bmatrix},$$

with $x = -e^{-2\sqrt{3}\tau}$. This array is reduced to Jordan form by the similarity transformation

$$H = \begin{bmatrix} 1 & 1 & 1 \\ 1 & \frac{1}{2}(-1 + \sqrt{3}i) & \frac{1}{2}(-1 - \sqrt{3}i) \\ 1 & \frac{1}{2}(-1 - \sqrt{3}i) & \frac{1}{2}(-1 + \sqrt{3}i) \end{bmatrix}.$$

Our problem is to indicate how a random sampling scheme of the type mentioned above could give some insight into the variety of branches of

$\log \hat{P}$ which are in \underline{Q} . To illustrate the ideas, we restrict our consideration to the subset of matrices B of the form

$$B = \begin{bmatrix} 1 & 0 & 0 \\ 0 & \beta & \alpha \\ 0 & \alpha & \beta \end{bmatrix},$$

where α and β are arbitrary *real* numbers.

A computing strategy designed to identify matrices B yielding branches of $\log \hat{P} \in \underline{Q}$ would begin by generating uniformly distributed (α, β) values within a square centered at the origin—the boundaries $|\alpha| = |\beta| = 10$ are chosen here for illustrative purposes. Each generated value represents a point on the α - β plane for which $\log \hat{P} = HB \log J B^{-1} H^{-1}$ is to be computed. If the resulting matrix is in \underline{Q} , a “+” is recorded at the point; otherwise, a dot is recorded. In the present example, the flared pattern shown in figure 11, known as the “Iron Cross of the Red Baron (2d class),” would result.

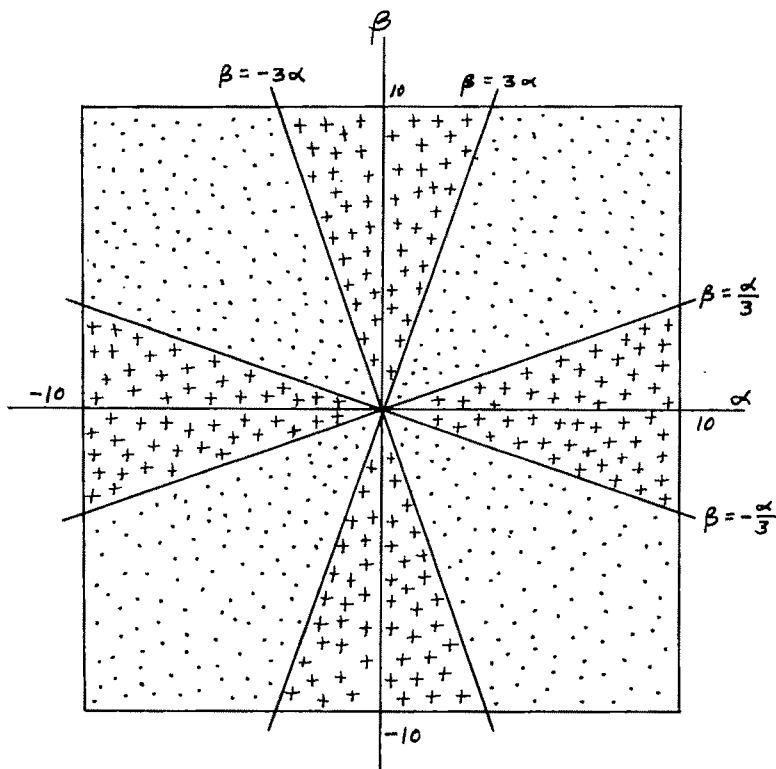


FIG. 11.—Restrictions on matrix B to generate branches of $\log \hat{P} \in \underline{Q}$. The (α, β) values for which $\log \hat{P} \in \underline{Q}$ are indicated by the symbol “+.” The contours of constant values of $\log \hat{P}$ are the straight lines $\beta = \left(\frac{1+x}{1-x} \right) \alpha$, where $1/2 \leq |x| \leq 2$.

The restrictions on α and β can be summarized by the inequality $\frac{1}{2} \leq \left| \frac{\beta - \alpha}{\beta + \alpha} \right| \leq 2$. What is more to the point, the structure of the continuum is identical to the one reported in equation (3.23). (This may be verified by replacing $\{c_{ij}\}$ in [3.21] with the appropriate α and β values and computing the restrictions [3.22].) In general, however, by limiting $\{c_{ij}\}$ to real values, only a portion of the continuum will be produced.

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On the Sociology of National Development: Theories and Issues¹

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This paper reviews the different perspectives from which the problem of national development has been approached by sociology. General characteristics of the problem of development and definitions of the term are discussed in the introductory sections. The perspectives reviewed are labeled "development as social differentiation," "development as enactment of values," and "development as liberation from dependency." The main trend noted is movement from earlier extrapolations of theories based on the unique European experience to more historically grounded analyses. Recent approaches to development are not without limitations, and these are reviewed, as are those of earlier evolutionary and psychosocial perspectives.

The study of national development occupies a paradoxical position within sociology. From classical times to the present, it has had a central place in the minds of theorists concerned with the transition toward more advanced social stages. At the same time, familiarity with the concrete historical experiences of countries in the "underdeveloped" world has remained a tangential preoccupation. This is especially true with regard to the actual dilemmas faced by nations attempting to break away from their past and move toward different models of the future.

A major gap appears to exist between theoretical perspectives chosen by modern sociology and recurrent dilemmas and concrete restrictions faced by the nonindustrialized world. In part, the paradox which makes of "development" both a central and an esoteric concern within sociology stems from the confluence of two different major themes. One is the century-and-a-half-old recapitulation of major processes of change which occurred in Europe beginning in the 16th century. The other is the more recent comparison between countries that are "developed"—wealthy, industrialized, technologically advanced, militarily powerful, politically stable, etc.—and those that are at different stages of "underdevelopment."

The first theme has generated a vast literature around the question, What were the forces which impelled Europe to evolve such drastically

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new social forms out of a rather conventional feudal order? The second has been concerned with the issue, What forces, present in some societies, have been absent in others and hence prevented their rapid advance toward industrialism? The issue of European transformation has given rise to what is, without doubt, the core of classic and of most modern sociological theory. As will be seen, however, extrapolation of concepts thus generated to answer questions of contemporary national development has yielded more questionable results. It is here that origins of the gap between social theory and social reality in comparative international development begin to be apparent.

The goal of this paper is to further explore relationships between the experiences of underdeveloped countries, as they attempt to bridge the gap separating them from advanced nations, and contributions made by modern sociology to an understanding of these phenomena.

DEVELOPMENT: PRELIMINARY CONSIDERATIONS

When scholars or leaders of countries in Africa, Asia, or Latin America refer to "development," they consistently sound one or more of the following major themes, which, taken together, form a working definition of the concept:

1. Economic transformation, in the direction of sustained and rapid increases in the national product and the conquest of "decision centers" in manufacturing, which give the country a measure of autonomy for guiding its future growth (Furtado 1964).
2. Social transformation, in the direction of a more egalitarian distribution of income and widespread access of the population to "social goods" such as education, health services, adequate housing, recreational facilities, and participation in political decision making (Weiner 1966).
3. Cultural transformation, in the direction of reaffirmation of national identity and traditions. Emergence, in elites and masses alike, of a new self-image which dispels feelings of second-rate nationality and external subordination (Lagos-Matus 1963).

There are three important aspects of the relationships between wealthy and impoverished nations on which there seems to be growing consensus. They are introduced here because they serve as appropriate background against which to discuss contributions of sociology to the problem of development.

Growing inequality.—Little more than a decade ago the line between economically advanced and underdeveloped countries was set by the U.N. at U.S. \$500–\$600 of GNP per capita. Table 1 presents some recently compiled data on GNP per capita and annual growth rates for the decade

TABLE 1

GROSS NATIONAL PRODUCT PER CAPITA (IN 1972 DOLLARS), 1963-73

Year	Developed Nations*	Underdeveloped Nations*	World
1963	2,381	185	821
1964	2,509	194	857
1965	2,619	202	887
1966	2,742	207	917
1967	2,832	208	934
1968	2,967	212	967
1969	3,074	223	996
1970	3,164	236	1,020
1971	3,256	244	1,043
1972	3,378	251	1,071
1973	3,561	252	1,112
Average annual growth	3.9	3.2	2.9

SOURCE.—Adapted from U.S. Department of State (1975).

* "Developed nations" include: United States; Canada; all European NATO countries except Greece and Turkey; all Warsaw Pact countries except Bulgaria; and Austria, Finland, Ireland, Sweden, Switzerland, Australia, New Zealand, Japan, and South Africa. Twenty-eight countries were classified as developed. "Underdeveloped nations" include: all those in Latin America; in the Near East including Egypt; in East Asia except Japan; in South Asia; in Africa except South Africa; and also Albania, Bulgaria, Greece, Malta, Spain, Turkey, and Yugoslavia. One hundred eight countries were classified as underdeveloped.

from 1963 to 1973.² According to the compiler (U.S. Department of State 1975), the demarcation line now stands at roughly \$1,500 (1972 dollars). Apart from the effects of inflation, the tripling of the figure stems from the growing dynamism of economic, especially industrial, production all over the world.

Such growth has not been distributed equally, however, nor has it contributed to narrowing the gap between economically advanced and underdeveloped societies. Figures from the State Department can hardly be suspected of exaggerating that gap. Yet those appearing in table 1 indicate that the average GNP per capita of developed countries increased from 12.88 times that of underdeveloped nations to 14.13 times that amount in 10 years. Average annual growth rate in developed countries during this period stood at 3.9% as compared with 3.2% in underdeveloped ones. The latter figure is inflated by inclusion of the oil-producing countries, which experienced an extraordinary growth during the decade. The OPEC countries as a whole were estimated to grow at an average rate of 4.7% per year (U.S. Department of State 1975).

The situation may be further clarified by considering individual countries. They are cross classified in table 2 by 1963 per capita GNP and by average annual growth rates from 1963 to 1973. There is obviously a great

² Despite criticisms leveled against Gross National Product per capita, it has remained the single most important measure of economic development because it reflects differences among countries in the average productivity of the labor force. On this point, see Emmanuel (1974).

TABLE 2

GROSS NATIONAL PRODUCT PER CAPITA AND GROWTH RATES, 1963-73

AVERAGE ANNUAL GROWTH RATE 1963-73 (%)	GNP PER CAPITA, 1963 (IN 1972 DOLLAR EQUIVALENTS)							
	Less than 100	100-199	200-299	300-499	500-999	1,000-1,999	2,000-2,999	3,000 or More
Less than 1.00 . . .	Afghanistan, Burma, Chad, India, Nepal, Somalia, Upper Volta	Cambodia, Haiti, Niger, Sudan, North Vietnam, South Vietnam, Yemen	Egypt, Ghana, Jordan, Senegal	Zambia	Cuba, Lebanon, Mongolia, Uruguay	Kuwait
1.00-1.99	Burundi, Dahomey, Ethiopia, Mali, Rwanda	Central African Republic, Guinea, Malagasy Republic, Sierra Leone	El Salvador, Honduras, Morocco	Albania, Congo, Nicaragua, Peru	Chile	Venezuela
2.00-2.99	Ceylon	Bolivia, Uganda	Colombia, Iraq, Paraguay, Philippines, Syria	...	South Africa	...	Iceland, Luxembourg, United Kingdom, New Zealand	Sweden, Switzerland, United States
3.00-3.99	Indonesia, Laos, Tanzania	Cameroon, China (People's Republic), Kenya, Liberia, Nigeria, Togo, Zaire	Ecuador, Ivory Coast, North Korea, Tunisia	Algeria, Costa Rica, Dominican Republic, Guatemala, Malaysia	Argentina, Mexico, Trinidad-Tobago	German Demo- cratic Republic, Ireland	Australia, Denmark, Norway	Canada, Federal German Republic
4.00-4.99	Mauritania, Pakistan	Thailand	...	Brazil, Turkey	Panama	Austria, Czechoslovakia, Finland, Italy, Hungary, Poland, Soviet Union	Belgium, France, Netherlands	...
5.00-5.99	Spain, Yugoslavia	Israel
6.00 or more	South Korea	China (Taiwan), Iran	Gabon, Saudi Arabia	Bulgaria, Greece, Cyprus, Rumania, Lybia, Portugal	Japan

SOURCE.—Adapted from U.S. Department of State (1975).

deal of dispersion in these data. Nevertheless, dominant trends exist which may be summarized as follows: Among countries considered "developed," none failed to grow at annual rates of at least 2.00%. Among the least developed countries (those with per capita GNP of less than \$300 in 1963), more than half failed to reach the 2.00% annual growth figure, and only 10% exceeded growth rates of 4.00%. In the intermediate categories (per capita GNPs between \$300 and \$999 in 1963), slightly more than a third of the countries had growth rates of 4.00% or higher; a third, however, fell below the minimal criterion of 2.00% per year.

These figures obviously do not take into account the form of growth in those underdeveloped countries which performed most satisfactorily. The fragile, artificial nature of much of this growth has been noted recently by several authors (Cardoso 1974; Emmanuel 1974). These considerations and the above figures confirm the insight of earlier experts that, left to themselves, economic mechanisms would tend to increase disparities between the wealthiest and the poorest nations (List 1856; Myrdal 1957).

State intervention.—A second, related point of consensus is the need for decisive state intervention in initiating and sustaining a process of development. This applies to economic growth as well as to social redistribution and cultural transformation. Examples of "late development"—countries which crossed the threshold after the dichotomy development-underdevelopment became part of international reality—furnish perhaps the most useful point of reference for analysis of the process. Japan and the Soviet Union are generally regarded as the most remarkable examples of late development. Countries like Argentina, Greece, Portugal, Spain, Yugoslavia, and Poland experienced, at some point or another, rapid processes of social or economic change and, hence, partially attained developmental goals (O'Donnell 1972; Linz 1973). Finally, nations ranging from Cuba to Brazil and from China to Peru are currently engaged, under differing political regimes, in sustained efforts at economic and/or social transformation (Dumont 1965; Lowenthal 1974; Cardoso 1974).

The main lesson emerging from these experiences is that development does not just happen. Instances of accelerated economic growth or social transformation among modern nations have generally been preceded by deliberate policies initiated and sustained by national governments. This process, in turn, has been dependent on arrival to positions of power of new ruling groups. Such "developmental elites" have had the most varied social origins—from radical intellectual groups to middle classes and the military. Their common theme, expressed under different ideological forms, has been attainment of national autonomy by somehow bridging the gap between their countries and advanced nations. State resources are then employed to implement the image of the national future held by the new elites.

Early industrial development in northern Europe and the United States differed from late or contemporary development, for the former lacked the definite teleological component of the latter. Ever since the international division between technologically advanced powerful nations and technologically backward poor ones became fact, rapid development has generally been the result of a consciously guided process. The presence of development-oriented ruling groups, their effective control of the state, and their deliberate attempts to transform the social structure have been necessary, albeit not sufficient, conditions for national development (Apter 1971). Governments under the control of such groups have aimed at reversing those "natural" processes of the international economy which, left to themselves, would increase disparities between their countries and developed ones.

Emergent realities.—Despite the goal of bridging the gap and the analogy of movement from a backward to a modern situation, there is agreement that contemporary development cannot be a mere repetition of earlier models. The international context in which national transformations occur has been altered drastically in the course of a century. International trading alliances—the growth of common markets and producer organizations—and especially the rise of a "transnational" corporate economy in recent years have produced an immensely more complex situation (Sunkel 1974).

In addition, advanced countries do not simply furnish a static end point for the process of development but are themselves rapidly evolving. Both technologically advanced and backward nations are integral parts of a changing "world-system." Within it, societal development does not occur as a simple linear advancement toward a static goal but rather as a dialectic exchange between national and transnational actors (Wallerstein 1974).

At least one author has argued persuasively that nations oriented toward development should explicitly abandon the model offered by already advanced societies (Illich 1969). Increasing doubts about the quality of life offered by these societies to their own populations, their permanent inequalities, and growing alienation suggest the need to search for a different course. Illich goes on to outline a process of nonimitative development in which simple, original solutions to human problems are locally evolved in lieu of imported, expensive, and unnecessarily complex devices. Utopian or not, this point of view serves to underline the importance of new and innovative paths for development. Both in terms of goals guiding the process and in terms of structural constraints affecting it, contemporary development transcends the mere reenactment of experiences of currently industrialized nations.

The preliminary remarks above are intended as introduction to the

general topic of this paper and as generally accepted points of reference for guiding the ensuing discussion. Experts in the field should not expect to find much that is new in the following sections. They are not intended as an alternative perspective on development but as a survey and critique of major contributions to this area. The purpose is to assess the standing and limitations of current sociologies of development. The strategy consists of presenting the major perspectives on the problem in rough chronological order. A brief outline of each is followed by comments. I believe that the three viewpoints to be examined exhaust major ones advanced in sociology. Thus, discussion of them should clarify current limitations of the field and, I hope, point in alternative and more promising directions.

DEVELOPMENT AS SOCIAL DIFFERENTIATION

Theory

Nineteenth-century scholarly efforts to apprehend processes leading to industrialism culminated in the various theories of social evolution. Borrowing from the biological sciences, the concept of "development" was applied to the continuous transformations experienced by human societies. The latter were envisioned as growing organisms which passed through a series of ordered and inevitable stages. Such stages culminated in the highest levels of societal complexity, represented by advanced European nations. The theories of Morgan, Comte, Hobhouse, Spencer, Kidd, and Ward represented this viewpoint in the 19th and early 20th centuries (Bogardus 1940). Leslie A. White (1959) and J. Steward (1955) are modern anthropological proponents of the evolutionary perspective on cultures.

The status of evolutionism as the dominant perspective in the social sciences did not suffer because of its Europe-centered character. Instead, its popularity floundered on two formal shortcomings: first, the assumption that development of human societies was unilinear and its major "stages" were universal, and second, the failure to specify fully the causal mechanisms and processes of change which led to the transition from one "stage" to another (Eisenstadt 1964).

The demise of evolutionary theory was not total, for it left behind a series of notions and hypotheses influential to our days. Three such sets of ideas can be mentioned: first, a wide assortment of bipolar theories which compress the former evolutionary stages into ideal-typical extremes; second, the notion of "social differentiation" as a continuous and largely irreversible process; and third, the more recent listing of "evolutionary universals." Each idea eventually acquired a double role: as part of general social theory, each continued the tradition of classic European thought, preoccupied with the historical transformation of the West. Each was also

made to apply, eventually, to comparisons between contemporary "developed" and "underdeveloped" societies and the processes of change occurring within the latter.

Bipolar theories of society trace their origins to classic sources of modern social thought. Theorists in this tradition were less concerned with encompassing the entire history of mankind than with apprehending that moment of European transition from a feudal-agricultural to a capitalist-industrial order. Their theories necessarily adopted a "before-and-after" model, in which beginning and end stages were described in different terms and with different value tonalities (Bendix 1967). Bringing these theories together should not be construed as neglect of the many particular points of value in each but rather as a means of highlighting a formal feature present, to a greater or lesser extent, in all of them.

Ferdinand Toennies's "community" and "society" and Émile Durkheim's "mechanic" and "organic" solidarities are perhaps the best-known classic examples. Toennies (1957) came to deplore the absorption of the spontaneity and intimacy of "community" into the impersonal automatism of "society." Making systematic use of the notions of social differentiation and integration, Durkheim (1933) viewed the process more optimistically as a transformation toward higher social forms. Charles H. Cooley (1962) opposed "primary" to "secondary" social attachments in a manner similar to Toennies's. Henry Maine (1907) distinguished between societies based on status and those based on contract. Robert Redfield (1965) came to oppose "folk" to "urban" cultures on the basis of his anthropological research. Howard Becker theorized on differences between "sacred" and "secular" social orders. In this context, the contemporary distinction between "tradition" and "modernity" (see Lerner 1965; Levy 1966) can be easily conceptualized as a latter-day counterpart of a theme present since the beginnings of sociological thought.

One of Talcott Parsons's best-known contributions is the attempt to break down this time-honored polarity into a series of more specific and exhaustive "value orientations." The resulting pattern variables (Parsons 1964a) were originally intended as a contribution to general social theory. It did not take long, however, for writers of the functionalist school to transform the scheme into a tool for the comparative diagnoses of societies.

For Hoselitz (1960), the pattern variables constitute a paradigm of the sociological aspects of economic growth: underdeveloped (traditional) societies are those in which roles are ascribed, functionally diffuse, and oriented toward narrow particularistic goals. Developed (modern) societies are characterized, on the other hand, by clearly delineated specific roles, which are acquired through achievement criteria and oriented toward universal norms (Chodak 1973). The transformation of classic bipolar theories into the pattern variables scheme and of the latter, in turn, into a theory

of societal development is perhaps the clearest instance of the application of notions emerging from European history to the contemporary situation of underdeveloped societies.

Among residues left by evolutionism, none receives a greater degree of acceptance at present than the concept of "social differentiation." The theory of society based on this notion is one in which pressures faced at a given point are eliminated through increasing specialization and differentiation, which, in turn, give rise to problems of integration, which are solved through emerging networks of interdependence. The whole process results in an ever-growing societal "complexity" or "systemness." Social differentiation is another concept emerging from general social theory and made to apply to contemporary comparisons of development and underdevelopment. For Smelser (1968, p. 138), "Development proceeds as a contrapuntal interplay between differentiation (which is divisive of established society) and integration (which unites differentiated structures on a new basis)." For Eisenstadt (1964, p. 376), "Such continuous differentiation has been usually conceived as a continuous development from the 'ideal' type of the primitive society. . . . Development proceeds through various stages of specialization and differentiation." Finally, the notion of "evolutionary universal" is Parsons's major late contribution to social theory. The concept is defined as "any organizational development sufficiently important to further evolution that, rather than emerging only once, it is likely to be 'hit upon' by various systems operating under different conditions" (Parsons 1964*b*, p. 339).

A series of such organizational developments is identified, from the appearance of a scale of stratification and patterns of cultural legitimation independent of social structure, to bureaucratic organization, money and markets, a universalistic legal system, and the emergence of the democratic association. While the major intent of the listing is to foster the revival of evolutionary theory under a revised form, it has implications also for a theory of development. This was already noted in the original formulation, where the author called attention to the "extremely important" consequences that the list of universals had for modernization in present "underdeveloped" societies (Parsons 1964*b*, p. 357). The last two or three universals analyzed appeared to be especially relevant in this regard.

In sum, the sociology of development which emerged as a by-product of classic theory is built on polarities provided by ideal-typical forms of social organization. Development is conceptualized as gradual, qualitative passage from less to more differentiated social forms. This occurs through processes of ever more complex specialization and functional interdependence. Through them, social roles are transformed to approach modern standards of universalism, specificity, and achievement. As societal development proceeds, certain "adaptive features" are incorporated which in-

crease the capacity of the system to survive in its environment. A money economy, formal rationality in the administration of justice, and, finally, the democratic association are among such structural features.

Discussion

Critiques of the definition of development as evolutionary transformation have been made before (Frank 1969; Bendix 1967). They have not dealt with attempts at operationalizing the concept of differentiation (through measurements of division of labor, etc.) but rather with the theoretical statements reviewed above. The discussion below aims less at raising new points than at systematizing those already made. In this regard, four major aspects must be considered:

Abstractness.—Notions derived from general evolutionary theory may prove useful as a comprehensive tool for the analysis of human societies. They could also be accepted as a broad perspective on social history. Their utility, however, breaks down when they are applied to the concrete "middle-range" problem of national development.

Students of developing societies have had difficulty in applying concepts stemming from the general evolutionary perspective to the analysis of specific historical instances. Such a gap is caused less by the "falsehood" of propositions based on the theory than by their excessive abstractness. Treatises of development based on this perspective are constructed in terms so general that they are vacuously applicable to any and all societies. The "truth" contained in such propositions is trivial, for it defies empirical negation.

To make the diagnosis, for example, that social transformations occur in the passage from underdevelopment (backwardness, undifferentiation, ruralism, etc.) toward modernization (industrialism, social complexity, urbanization, etc.) is to beg the entire question. Such descriptions (assuming for the moment their accuracy) are not the end point of scientific inquiry into the problem but their beginning. The question is why such transformations occur in some societies and not others, why they take place at different rates and in different forms, under which conditions such processes are paralyzed or even reversed, and under which they successfully overcome structural obstacles.

These and other issues, fundamental for the analysis of social change in underdeveloped societies, cannot begin to be handled in terms of classic bipolar theories. These typologies provide no theoretical guidance for inquiring into determinants and constraints of developmental processes. Still, many discussions in this area continue to restate such descriptions as if they constituted an adequate response to very complex questions:

When we employ the term [development] we usually have at least four distinct but interrelated processes in mind: 1) In the realm of technology,

a developing society is changing *from* simple and traditionalized techniques *toward* the application of scientific knowledge. 2) In agriculture, the developing society evolves *from* subsistence farming *toward* the commercial production of agricultural goods. . . . 3) In industry, the developing society undergoes a transition *from* the use of human and animal power *toward* industrialization proper. . . . 4) In ecological arrangements, the developing society moves *from* the farm and village *toward* urban concentration. [Smelser 1966, pp. 110-11]

Similarly, the statement that development proceeds through the "counterpoint of differentiation and integration" or that it constitutes a process of increasing "social systemness" can be readily accepted without advancing in any way our understanding of the phenomenon. The abstract character of these assertions makes them difficult to operationalize. More important, they provide insight neither into origins of developmental efforts nor into their many variants. Inquiries, for example, into the composition of elites oriented toward development, their struggles for control of the state, and the dilemmas they face in allocation of scarce national resources do not flow naturally from this paradigm nor can they be readily integrated into it (Kerr et al. 1960; Furtado 1973).

Abstractness, in sum, means that the effort to encompass processes of change in all societies has rendered the evolution-differentiation perspective virtually useless for close-range analysis of individual ones. Theorists of this persuasion can pursue their discussions, secure in the knowledge that no empirical fact will ever disturb their views. For the serious student of Japanese, Indian, or Mexican development, such "truths" are both readily evident and trivial.

Gradualism.—The imagery conveyed by the concepts of differentiation and integration is one of a relatively smooth, gradualistic, and irreversible societal process. Problems to be solved by greater functional specialization are part of the natural internal evolution of the social system in its march toward greater complexity. Such a view may again be applicable in the abstract to all societies in "evolutionary time." It may also provide a reasonable framework for describing that portion of social change made of "adjustments" of an ongoing system. Yet it can hardly accommodate the many contingencies of contemporary social change in the underdeveloped world.

Those contingencies involve the composition and orientations of different political and economic elites, the situation and distribution of rural and urban masses, the uncertain outcomes of struggles for control of the state, the sudden thrusts, convulsions, and reversals suffered by the social fabric during processes of national transformation. Descriptions of development as automatic, growing societal systemness offer few conceptual tools with which to understand the structural constraints and failures

that form an intrinsic part of any developmental effort (Malloy 1971; Seers 1969).

How are we to understand, for example, the dismantling of redistributive and participatory institutions in contemporary Uruguay and Chile? How should we explain the persistent economic stagnation of India and the sudden growth of Thailand? What accounts for advances in social redistribution under the military in Peru and reverses of a similar trend under the military in Bolivia? What are the limits of social development without rapid economic growth in Cuba and of economic development without social equality in Mexico?

Above all, the gradualistic imagery of development as social differentiation provides no insight into the role of social actors in social change. In underdeveloped countries, that role involves the commitment of strategic groups—elites and masses—to overcoming past structural barriers (Horowitz 1966). Developmental efforts are aimed precisely at reversing, not encouraging, the “natural” evolution of the preceding social structure (see Scott 1973). As seen above, natural processes of differentiation in the international economy have tended to increase inequalities between wealthy and impoverished nations. Development, as experienced in the latter, is not a gradual, automatic, and impersonal process but a consciously guided, difficult, and often drastic alteration of existing social trends.

External factors.—A criticism leveled more frequently at this approach is its neglect of interrelationships among nations and the impact of such factors in the internal structures of each. Societies are conceived as autonomous units which change according to internal forces. Room is made for some intersocietal exchanges and consequent processes of “diffusion.” However, the introjection of the economy and polity of some societies into the internal structure of others and the supranational dynamism of an organized world-economy are not comprehended.

It is perhaps for the above reason that this perspective on development is able to conceptualize the phenomenon as movement from an initial (backward) state to a final (modern) one. Within the analogy, autonomous societies occupy more or less advanced positions, with the “winners” coming to resemble the United States and the industrialized western European nations. As seen above, this imagery neglects the fact that the internal configuration of each competing unit is profoundly affected by its interactions with the others, especially the more powerful ones.

Though abstract and partial, the evolutionary viewpoint provides at least a statement on the internal structure and change of societies. It fails completely, however, to provide a framework for understanding the insertion of individual countries in an evolving international system. Distinctions between core and peripheral economic regions are foreign to the

theory. Nor does it grasp the possibility that "autonomous competitors" in the developmental race may be integral parts of transnational units in which weaker states are kept in place by a context of overwhelming political and economic forces (Sunkel 1972). Contemporary development is not a matter of autonomous change but one composed largely of exchange and confrontation in an integrated world-system, in order to alter the position of individual nations (Wallerstein 1974).

Conceptual misplacement.—Limitations of the passage from a Europe-centered theory of change into a theory of contemporary development are reflected in identification of the "wrong" dimensions as indicative of or strategic for development. Theoretical writings may thus contain extensive discussions of aspects which have not been central to historical experiences of change in the Third World. Deflection of attention from important problems occurs because the theory is based, not on extensive knowledge of underdeveloped societies, but on reflection on the European past.

Thus, for example, development may be conceptualized as an attempt to attain missing "evolutionary universals." Minimal familiarity with Third World nations would indicate that, by these standards, most of them are already "developed." Such features as money and markets, extensive bureaucratic regulation, and formal legal systems have been long known and present in underdeveloped countries. It is perhaps for this reason that when the abstract discussion of "universals" reaches for concrete examples, it selects primitive tribal societies as poles of contrast to modern Western nations (see Parsons 1964*b*). Such comparisons are, of course, entirely irrelevant to the problem of national development. Third World nations, regardless of stereotypes dear to theorists, are not in the tribal stage.

Here again, the problem lies in conceptualizing underdevelopment as a feature intrinsic to autonomous societies rather than as part of an international system. Structural characteristics like currency and markets, formal legal regulation, and bureaucracies exist in both developed and underdeveloped nations, for both kinds are part of an integral world-order. The functions of these traits vary, however, between the two types of societies because their "adaptive" potential is employed in accordance with the particular insertion of the country into the international economy.

Since colonial times, extensive legal and bureaucratic regulation of dependent territories has been employed by metropolitan centers as a means of insuring their hegemony. The historical dialectics by which "modern" structural features serve to perpetuate weak and stagnant societies are not understood by proponents of the evolutionary perspective. The paradox of modernization for underdevelopment is further illustrated by the recent role of the final evolutionary universal—formal democracy.

Following western European models, many underdeveloped countries

began their independent existence as democratic republics, and some managed to maintain a working democratic system for extended periods of time. In Latin America, Costa Rica and, until recently, Uruguay and Chile were examples (Valenzuela 1972). Democratic politics in these societies did not greatly increase the "adaptive" capacity of their social systems. As a recent history of economic stagnation and political turmoil shows, formally democratic politics often function to legitimize and perpetuate the contradictions created by social inequality and a dependent economy (Zeitlin 1968). In these circumstances, democratic systems have been replaced by traditional repressive oligarchies or by different "mobilization" systems (Apter 1967).

DEVELOPMENT AS ENACTMENT OF VALUES³

Theory

Myron Weiner (1966) notes that, for many scholars, the starting point of any definition of development is not the character of the society but the character of individuals. The same author observes that "although there are differences among social scientists as to how values and attitudes can be changed, it is possible to speak of one school of thought that believes that attitudinal and value changes are prerequisites to creating a modern society, economy, and political system" (Weiner 1966, p. 9).

For Chodak (1973, p. 11), writers of this school do not ask, What is development? or What happens in its course? but rather why it happened and what specifically caused the breakthrough from traditional into modern societies. Where such factors are present, development happens; where they are absent, stagnation prevails. The distinctive factor is then sought in the sphere of value orientation.

Unlike the first perspective, whose emphasis was on describing evolutionary trends, this one is essentially explanatory. The search in this case is for those "mental viruses" (McClelland 1967) changing the "spirit" (Inkeles 1969) of men so that they come to adapt and promote a modern society. This perspective derives its impetus from the general emphasis in United States sociology on value-normative complexes (Parsons 1964a) as opposed to the structure of material interests in society (Mills 1956). More specifically, the value approach to the problem of development lays claim to, and often labels itself as a direct continuation of, the thesis Weber developed in *The Protestant Ethic and the Spirit of Capitalism* (1958).

Weber's argument was, however, securely embedded in a body of re-

³ This section draws substantially from an earlier article (Portes 1974). The redundancy is justified by the effort to bring together, in a single piece, presentations and critiques of all major perspectives in this field.

search which clearly brought forth the importance of structural forms and the politico-economic interests of groups and classes. Emergence of an urban burgher class out of the feudal "oikos" and the relative vulnerability of feudalism, as opposed for example to the "prebendary" system of China, are subjects examined at length in his work (Weber 1951). The combined effects of the political assault by the central state and the economic assault by rising urban classes on the weakened feudal order meant an increasingly "open" structure for capitalist expansion (Weber 1958; Bendix 1962). Only because of the growing predicament of a lordly class incapable of defending its position by enforcing old prerogatives could the Protestant "spirit" of capitalism, or any other spirit for that matter, transform the economic order to its own advantage (Wallerstein 1974).

Psychological theories of development, such as those proposed by McClelland (1967) and Hagen (1962), have chosen to ignore the Weberian treatment of historicostructural issues and concentrate on the primacy of ideas in society: "This is just one more piece of evidence to support the growing conviction among social scientists that it is values, motives, or psychological forces that determine ultimately the rate of economic and social development. . . . *The Achieving Society* suggests that ideas are in fact more important in shaping history than purely materialistic arrangements" (McClelland 1963, p. 17).

Since ideas inhere in individuals, these theories result in an "additive" image of societal development in which the larger the number of people "infected" by the strategic psychological ingredient, the greater the economic growth of the country. This arithmetic approach is concerned neither with differences in positions in the stratification system nor with existing arrangements of economic and political power. Theorists of this persuasion subscribe to the proverb, Where there is a will, there is a way; their voluntarism is, in turn, predicated on the creation of sufficiently high levels of motivation. In the best known of these theories, the factor responsible for this result is labeled "*n*-achievement": "The mental virus received the odd name of *n*-Ach (short for 'need for Achievement') because it was identified in a sample of a person's thoughts by whether the thoughts had to do with 'doing something well' or 'doing something better' than it had been done before: more efficiently, more quickly, with less labor, with a better result and so on" (McClelland 1966, p. 29). Extrapolating the result to national development, McClelland (1966, p. 30) reports: ". . . a country that was high in *n*-Ach level in its children's texts around 1925 was more likely to develop rapidly from 1929 to 1950 than one that was low in *n*-Ach in 1925. The same result was obtained when 1950 *n*-Ach levels were related to rates of economic development in the late nineteen-fifties." Apart from "*n*-Ach," McClelland prescribes a series of additional psychological ingredients making for soci-

etal development. Sense of collective responsibility and feelings of superiority over others are the most important ones.

Hagen's (1962) theory of "withdrawal of status respect" is more complex, in a psychoanalytic sense, than McClelland's. It comes, however, to the same formal conclusion. Again, a psychological motor, present in sufficiently large numbers of people, provides the strategic impulse for economic development. In this case, however, the "virus" is not transmitted by children's texts but instead has a gestation period of several generations. Humiliations resulting from status withdrawal among parents have certain psychic consequences for their sons who, in turn, transmit them to their own children. After a complicated evolution of complexes and stages, the "virus" finally matures and is ready to do its work in society. Whether the society is ready for the actions of the new entrepreneurial group is not of major importance to the theory.

More recent, and perhaps more accepted among sociologists, is the theory of "modernity" as a psychosocial complex of values. "Modern man" is characterized, internally, by a certain mental flexibility in dealing with new situations and, externally, by similarity to the value orientations dominant in industrial Western societies (see Lerner 1965). The spirit of modernity is regarded by these writers both as a precondition for societal modernization and as a major consequence of it: "Indeed, in the end, the ideal of development requires the transformation of the nature of man—a transformation that is both a *means* to the end of yet greater growth and at the same time one of the great *ends* itself of the development process" (Inkeles 1966, p. 138).

Inkeles identifies nine major attitudes and values distinguishing modern man: (1) readiness for new experience and openness to innovation, (2) disposition to form and hold opinions, (3) democratic orientation, (4) planning habits, (5) belief in human and personal efficacy, (6) belief that the world is calculable, (7) stress on personal and human dignity, (8) faith in science and technology, and (9) belief in distributive justice.

This list is not complemented by a similar description of "traditional man." The latter tends to be defined by default: whatever is not properly modern must be traditional. It is difficult to understand, however, why "traditional man" does not stress "personal and human dignity" or believe in distributive justice.

The list of what goes on to make "modern man" tends to vary from author to author. This is not surprising, since much of what enters into the definitions appears to come from introspection. Theorists of this persuasion vie with each other in developing ever more elaborate descriptions of what modern man is like. In doing so, they tend to contradict one another. While, for some, individualism and self-reliance are clearly modern traits, for others the ability to subordinate personal goals to the welfare

of the collectivity and the ability to work with others toward its common pursuits constitute the mark of modernity (see Kahl 1968). As seen above, dichotomies between "traditional" and "modern" man are then built by extrapolation, through attributing to the former the reverse of characteristics assigned to the latter (Gusfield 1967).

In addition to the dimensions quoted above, the following are frequently encountered in characterizations of modern man: (1) participation: motivation and ability to take part in organizations and electoral processes; (2) ambition: high mobility aspirations for self and children and willingness to take risks; (3) secularism: limited religious attachments and low receptivity to religious and ideological appeals; (4) information: frequent contact with news media and knowledge of national and international affairs; (5) consumption orientation: desire to own new goods and technologically advanced recreation and labor saving appliances; (6) urban preference: desire to move to or remain in urban areas; (7) geographic mobility: experience of moving and/or willingness to move from original residence in search of better opportunities (Lerner 1965; Kahl 1968; Schnaiberg 1970; Horowitz 1970; Portes 1974).

Discussion

Some critical observations have already been made in the course of outlining this second perspective on development. The value-enactment viewpoint has probably been the most controversial of all three perspectives. Critics have noted, for example, its neglect of international economic and political linkages. As Frank (1969, p. 47) states: "Those engaging in [the value-enactment] mode of analysis resolutely avoid the study of the international structure of development and underdevelopment of which the domestic structure of underdevelopment is only a part."

Discussion of this second sociology of development must consider, however, several important aspects. A systematic presentation of these will cover three major points:

Structural constraints.—No matter how compelling the image of highly motivated entrepreneurs racing to break the barriers of stagnation, the fact remains that individual action is highly conditioned by external social arrangements. Despite the frequent application of "tribal" imagery to underdeveloped nations, the reality of such societies is not one of an open frontier awaiting conquest by an entrepreneurial elite. Indeed, a complex structure of economic and political interests penetrates every aspect of them. To think that more modernity, achievement motivation, or status withdrawal will automatically transform these structures is, at best, naive. Regardless of what psychologists may think, societies are not the simple "additive" sum of individual members.

An active set of individuals, motivated by whatever psychological mechanism one may wish to posit, must still cope with existing economic and political arrangements. One way of doing so is to attempt to transform them, in which case "entrepreneurs" must organize themselves and enter the political arena in conflict with entrenched interest groups. The transformation of "modernity" or "achievement" into potential rebellion and ideologically committed elites is a possibility seldom contemplated in these theories.

A second alternative is that "entrepreneurs" may attempt to work through established channels. This alternative, the most likely one in view of the costs involved, may explain the embracement by established power groups of this perspective as their sociology of development. Highly motivated modern individuals may be extremely functional for maintenance of existing power structures. They may be hired, for example, as highly paid managers of foreign corporations, as has been increasingly the practice of multinational companies (see Blair 1974). They may also be brought into preexisting civilian and military bureaucracies. There are indeed many opportunities for entrepreneurial fulfillment within the existing social order. Altruistic motivations of "moderns" may even be employed in melioristic welfare activities, irrelevant for the structural task of development but functional for legitimizing the existing politico-economic system.

Individual motivations for achievement can be absorbed, fulfilled, and utilized without changing a basic situation of economic subordination and social maldistribution. The issue is not how much individual motivation there is, or what its sources are, but rather to what goals it is directed. The fundamental individualism apparent in theories of achievement and entrepreneurial motivation may be either irrelevant or inimical to struggles for national transformation. Elites committed to the task of development are not formed by "moderns" but by "modernizers"—individuals committed to achievement of collective economic and social change (Kerr et al. 1960).

Consumption-oriented values.—A second, related aspect has to do with some of the values defined as "modern." That most modern of traits—"empathy"—is usually described as ability to comprehend and place oneself symbolically in the midst of urban-industrial life (see Lerner 1965). This, in turn, is directly linked with a "demonstration effect" which raises demands for consumption beyond what a poor country can realistically afford. Excessive, media-promoted demand is a problem faced by both status-quo and development-oriented governments in the Third World. For the latter, however, it presents a major difficulty, for it exercises pressure on scarce resources required for long-term investment. The dilemma of choosing between "political" and "economic" strategies (im-

mediate consumption and resulting mass political loyalty versus consumption restrictions and long-term planning) emerges here as a major developmental issue (Malloy 1971).

Communications experts agree that modern values are diffused through the mass media. Some argue that advertisements in the commercial media are effective carriers of the "modern" message: "Advertising itself may also be a powerful instrument of development. It is a way of facilitating the distribution of commodities, broadening the market, and making people aware of possibilities with which they would not otherwise be familiar" (Pool 1966, p. 108).

The market is certainly broadened, often for the benefit of multinational enterprises, strains are placed on the country's capacity to import, and new "possibilities" are taught which often bear no relationship to local conditions. Such modern values—premature wants, imported needs and tastes, excessive consumption—are not the values of development. Historical experiences of national development in this century show consistently the necessity for restriction of consumption and for an orientation which places as much emphasis on achievement of national goals as on personal gratification. "Mobilization systems" in the Third World have evolved precisely as attempts to diffuse these "nonmodern" values (Apter 1967).

Finally, much-derided "traditional" cultures have often furnished value legitimations necessary in periods of rapid national change. Secular modernity lacks sufficient cultural depth to match the force of great national traditions. Japanese ideology during the Meiji period furnishes perhaps the best known, but not the only, example of the uses of tradition for development (Bellah 1965; Gusfield 1967; Walton 1976).

Historical fiction.—Theories of modernity share with those of evolutionary differentiation the belief that development proceeds from an early traditional stage toward a terminal "advanced" one. It is proper at this time to complete analysis of the character of this analogy.

As seen above, tradition is described in terms which are only logical counterparts to those embodied in modernity. There is no existing nation in the Third World which can be labeled "traditional" in this sense. The fictional character of the initial stage of the process is due to the fact that it is not based on observation of actual societies but on reflection on the features of the "terminal" stage. Modernity creates tradition very much as, in Owen Lattimore's (1962) words, "Civilization gave birth to barbarism."

At the other extreme, as seen above, the current stage of development in industrial societies is unlikely to be replicated in underdeveloped countries. While providing points of reference for developmental efforts, features of currently industrialized nations are products of unique historical

processes which already belong to mankind's past. The concrete features of advanced societies of today cannot be reproduced exactly in the future, nor is this the goal of most Third World nations (Illich 1969).

Sociologies of development dominant in the West thus come to posit a transition from a fictional stage to an impossible one. By concentrating on current characteristics of industrial societies, they neglect the fact that these traits, as well as those of underdeveloped societies, are themselves evolving and that social change in each type of society occurs in interaction with the other type. As a contemporary Latin American sociologist states: "A science of development is only science when it abandons the assumption of a formal goal to be reached and attempts instead to comprehend development as a historical process. . . . The object of [such] a theory cannot be to describe the passage from a society that is not really known to one that is not going to exist" (Dos Santos 1970a, p. 174).

DEVELOPMENT AS LIBERATION FROM DEPENDENCY

Theory

The third and newest perspective on the sociology of development is one not original to the United States or western Europe but to countries of the Third World. In a sense, the theory of "dependency" is the counterpart of earlier theories of imperialism (Lenin 1939; Hobson 1965), seen from the standpoint of the subordinate nations.

This perspective views development and underdevelopment not as two different stages in the history of mankind but as integral parts of the same "world-economy." The forms that social poverty and economic stagnation take in the Third World at present are largely creations of the process of capitalist world expansion. It is argued that rapid industrial growth in the West could not have occurred without the conditioning of a "periphery" from which an economic surplus is extracted and necessary raw materials secured (see Wallerstein 1974).

Thus underdevelopment is not a "backward" state prior to capitalism but a variant of the latter and a necessary consequence of its evolution. Some dependency writings recognize the possibility of economic growth in the periphery but view it as nonautonomous "induced" development, limited by the interests of international centers (Dos Santos 1970b). This interpretation of causes of underdevelopment took its cue from earlier efforts in Latin America to implement an autonomous nationalist policy. The failure of the import-substitution strategy of industrialization prompted a search for the true mechanisms of the international economy in which these countries were embedded.

Earlier writings, such as those of Baran (1957) and the initiators of

the U.N. Economic Commission for Latin America (Prebisch 1950), addressed a situation in which international forces kept peripheral economies stagnant and restricted their role to that of producers of raw materials. Partly in response to these analyses, country after country, in Latin America and elsewhere, attempted to promote autonomous growth by raising tariff barriers and encouraging national industrial production for the domestic market. Import-substitution industrialization was expected to lead the way toward greater economic autonomy, less reliance on raw export commodities, progressive weakening of the power of the landowning aristocracy, and the diffusion of economic benefits to the impoverished masses (O'Brien 1975).

Writings in English, such as those of Gunder Frank (1967), have created the erroneous impression that "dependency" analyses continue to be concerned with the same quasi-colonial situation of economic stagnation and foreign control of export enclaves. On the contrary, contemporary dependency studies address a situation in which domestic industrialization has occurred along with increasing economic denationalization; in which sustained economic growth has been accompanied by rising social inequalities; and in which rapid urbanization and the spread of literacy have converged with the ever more evident marginalization of the masses (Sunkel and Paz 1970).

The complex dialectics of economic successes that lead to social failures and of strategies for national autonomy which produce external subordination lie at the core of the situation analyzed by proponents of this third perspective. Efforts to untangle the dialectics have centered on the structure of foreign-controlled multinational corporations and their role in underdeveloped societies.

The presence of multinational enterprises is credited with the bulk of the dismal outcomes of earlier import-substitution industrialization. In a sense, multinational corporations stood earlier goals of national autonomy on their heads by employing protectionist barriers as instruments for the more effective control of domestic markets and internal economic activity. Their role within national industrialization was justified by their control of two key resources which domestic enterprises often lacked: investment capital and commercial technology (O'Brien 1975). Use of these resources did not follow the blueprints of national development but rather those of corporate profitability, with consequences which led to increasing contradictions.

Without entering into the complexities and variants of the argument, one can say that dependency analyses generally identify two major consequences of the activities of multinational enterprises within import-substitution industrialization: externally, from international centers, these enterprises have maintained and expanded the economic dependence of

underdeveloped nations; internally, they have led to emergence of new privileged groups and the acceleration of social inequality.

External subordination comprises, in turn, two aspects. First, use of complex imported technology by local corporate affiliates and the inputs they require ties the dependent economy ever more closely to that of the centers. Second, the costs of capital intensive technology, added to profit remittances abroad, intercompany transfers, and so forth, amount to a constant drain of scarce foreign exchange resources.

In terms of net effect on the international balance of payments, the impact of multinational-sponsored industrialization is often negative, for the initial investment is offset by later capital goods imports, royalty payments, and profit remittances (Frank 1969). To finance continued industrial expansion, national governments rely increasingly on foreign loans. These, however, accentuate further the loss of internal autonomy by giving a voice on policymaking to international lending agencies and forcing compliance with their conditions. The current balance-of-payments situation of some of the most industrialized countries in the underdeveloped world—such as Argentina, Brazil, and Mexico—offers vivid proof of the recurrent crises induced by this mode of economic development (Dos Santos 1970a).

The internal impact of multinational corporations has been described as a virtual restructuring of the domestic social order. At one extreme, superior resources of foreign subsidiaries create "instant" obsolescence and progressive elimination of preexisting national enterprises. Those which survive must accept a satellized role, and the small number of domestic enterprises created to satisfy input demands of the multinationals are entirely dependent on the interests and policies of the latter. Thus, a progressively increasing portion of elite economic activity comes to revolve around the interests and priorities of foreign subsidiaries, in turn obedient to their metropolitan offices.

At the other extreme, a capital intensive technology not only is costly to import but also fails to absorb labor surpluses and, in some areas, increases them. While the working sector associated with the multinationals may evolve into a veritable labor aristocracy, the masses remain in a subsistence situation where unemployment, scarcity of bare essentials, and lack of access to expanding social and economic benefits are the norm. Industrialization under the aegis of foreign-owned corporations promotes a structure of increasing inequality where those working in the "modern" sector and able to purchase its products are a self-contained minority, progressively divorced from the plight of the majority (Furtado 1973). The basic cleavage in these new quasi-industrial societies does not occur between traditional peasant and landowning classes or even between domestic bourgeoisie and urban proletariat but between groups linked with

the foreign-owned sector (workers and employers alike) and the rest of the population (Sunkel 1972).

A situation of contemporary dependency is thus quite different from one of foreign colonialism. For underdeveloped societies, dependency is a condition not only external but also internal. Foreign domination is not imposed by an "army of occupation," as naive versions have asserted; instead, it is initially implemented by a willing local bourgeoisie (Quijano 1967). As Torres Rivas (1974, p. 195) has asserted concerning Central America: "[Such] dependent power is not only that which confronts and submits itself to imperialist power, but the behavior of dominant classes which, even prior to that confrontation, are already submitted to it. This is the consequence of anticipatory socialization, product of the very weakness of these groups."

At later stages, multinational enterprises are able to evolve their "own" bourgeoisie and proletariat, both allied in the defense of a privileged situation against the majority. This remolding of social structure serves well the short-term interests of the companies by creating artificially high demand in a docile market. It runs counter, however, to the interests of the nation as a whole. In this context, development consists logically of liberation from external control and from the internal structure of inequality which it promotes.

Discussion

Rhetoric.—The theory of dependency does not represent a system of logically interrelated propositions. Rather, it refers to a historical "vision" (Sunkel and Paz 1970), a sociohistorical "model" (Roth 1975) meant to apply to a plurality of situations. Such theoretical strategy helps define the scope of a field of study, sets priorities for empirical investigations, and provides an abstract framework under which more concrete hypotheses can be articulated. Like all historical models, however, it can also be applied so mechanistically as to suggest that there is nothing new under the sun.

In the hands of some writers, dependency has become a *deus ex machina* explanation for everything that is wrong with Third World societies (O'Brien 1975). This is accompanied by a denunciatory emphasis which obscures, more than it clarifies, the complexities and variants present in different situations. This lapse into rhetoric is not limited to the dependency perspective. The field of national development is one in which values are particularly close to the surface of theories. In the past, United States writers and others have also extended their own models and waxed romantic about the advantages of modernization and the virtues of "modern man."

Raising the issue of rhetoric in regard to dependency is justified by the threat which the rhetorical tendency poses to a promising approach. At the same time, comments must be qualified, for there are in reality not one but several varieties of dependency theory. The rhetoric of dependency characterizes primarily the earliest and crudest versions. Their excesses are largely responsible for the two criticisms leveled most frequently against the perspective as a whole: First, some writings have equated national development with liberation from foreign domination (see Frank 1967). Popularized versions of this view convey the impression that elimination of imperialist influence will automatically bring about national welfare and a higher standard of living for the masses. This is clearly not so. The same writers do not make explicit just how the newly "independent" states should proceed for attainment of national goals. They come to suggest, by default, a naive path of autarkic development (see McMichael, Petras, and Rhodes 1974).

Second, denunciations of imperialism and dependency as global phenomena leave no room for analysis of national variants. Countries like Canada are profoundly "dependent," in the sense of penetration of their economies by foreign-owned subsidiaries, and yet exhibit a much higher per capita income, a better distribution of wealth, and more efficient health and educational services than most "Third World" countries.

Though there are serious problems in Canada, and though many are susceptible to analysis within a dependency framework, they belong to a different genre from the situation of massive misery portrayed by early writings (O'Brien 1975). If dependency is the conditioning structure of poverty (Dos Santos 1970a), situations like those of Canada, Australia and New Zealand, small countries in western Europe, and even some eastern European satellites must be adequately accounted for. While the theoretical framework may encompass comparisons across a wide variety of national situations, the fervor of earlier writers managed to limit them to those documenting only a particular viewpoint (Cardoso 1974).

Internal dynamics.—A focus on the international economy in the study of underdevelopment has certainly been the major contribution of dependency studies. At the same time, it has led to neglect of the analysis of internal dynamics. This is not to say that interest has been limited to the international order, since, as seen above, the major topic of study at present lies in the penetration of external institutions into underdeveloped societies. The rather extensive literature on "internal colonialism" (Walton 1974), national centers and regional peripheries (Quijano 1967), and so forth documents this concern with the internal projections of international relationships.

The limitation—which is shared by all types of dependency theory—

lies in a deductive approach to internal conditions, making them conform to what is logically expected on the basis of external forces. The data base for dependency studies is usually limited to aggregate economic figures on production and distribution, exports and imports, capital flows, inflation, and the like. From these, inferences are made concerning the impact of the situation on different classes and sectors and about their subsequent behavior.

The empirical weakness of dependency studies, as pointed out frequently by critics, is based precisely on the absence of autonomous data for each country to validate these deductive inferences. Without them, assertions derived from an abstract framework are always suspect. While available empirical research has supported some propositions, others have been rendered more questionable. Thus, Frank's thesis of the progressive elimination of precapitalist forms in dependent countries runs counter to recent anthropological evidence on the preservation of tribal and other precapitalist production forms and their articulation with the "centers" (Wolpe 1975). Similarly, views of regional subordination and exploitation by the national metropolis are contradicted by evidence of the dynamism of regional elites and the autonomous economic growth of several "peripheral" areas (Balan 1974). Finally, descriptions of a labor sector divided between a privileged "internationalized" elite and disenfranchised masses are called into question by recent data on the preference of many workers to abandon secure factory jobs in order to join the "informal" sector as self-employed entrepreneurs (Peattie 1974).

A final point is that a view of social structure centered on the international economy fails to take account of autonomous internal forces and their potential role in processes of change. All historical evidence points to the existence of certain "degrees of freedom" for national governments and their ability to carry out, under certain circumstances, fairly drastic policies of internal and external transformation. While analyses of dependency often end by calling for precisely this kind of action, the forces which could promote it and the dilemmas and options that it would confront are not explicitly integrated into the theory.

While the dependency approach has been useful in clarifying the historical origins of a situation of underdevelopment, it has not been equally helpful for understanding those processes which could lead away from it. At present, we must look elsewhere for studies of development-oriented elites, conditions under which they come to control the state, and circumstances which permit them to mobilize the masses into a national developmental effort. These and other topics, outlined below, constitute an agenda for future research and for expansion of a still useful theoretical perspective.

CONCLUSION

Reviews of a field customarily end with an appraisal of the future and its likely trends. These appraisals are either prescriptive (calls to action and appeals to pursue certain lines of research) or descriptive (speculative assessments of the directions which research and theory will take). Here, I will choose the second alternative, for prescriptions are seldom heeded, serving only to legitimize initiatives already taken.

One thing seems certain—the almost complete exhaustion of earlier modernization theorizing as a source of insights into the situation of underdeveloped nations or of significant questions for research. Newer perspectives—elaborated at closer range to the realities of underdevelopment—represent a potentially more useful guide for future investigation.

Still, much remains to be done before the sociological approach to the problem can acquire a clearer sense of priorities. The passage from earlier evolutionary theories to modernization theories and thence to dependency studies has mostly clarified what a sociology of development should not be concerned with. It should avoid, for example, a social philosophical perspective in which processes of change are described in such abstract terms as to be of little use for the analysis of concrete situations. The concept of evolutionary stages and a theory of development limited to abstract bipolarities are examples.

The intellectual thrust toward new middle-range perspectives has come from sociologists from the countries directly affected and from those in the United States and Europe disenchanted with the general and uninformed assertions of the past. Students of development have become less concerned with translating theories suggested by the European past into theories about the underdeveloped world and more with elaborating an autonomous framework for dealing with underdeveloped societies. A point to be stressed is that rejection of classic theories extends both to non-Marxist evolutionary and value-normative approaches and to orthodox Marxism (Cardoso 1974).

What then are likely trends to be followed by this field in the future? A first and predictable concern will be the continuing analysis of external linkages and their projection into the social structures of underdeveloped societies. Much of the dependency literature has been written from the perspective of political economy rather than sociology proper. While analyses of external and internal economic transactions are likely to continue as a central focus, there is an increasingly diversified concern with the internal manifestations of dependency.

Such studies will include ones of trends in spatial distribution of the population, urban growth and rural-urban migration, the changing ecology of cities, partition of the labor force into an "internationalized" sector

and its "domestic" counterparts, the growth of public sector enterprises and their relationships with multinational firms, emergence of a "national bourgeoisie" of entrepreneurs and civil servants and their efforts to cope with or resist external penetration, the insertion of foreign-based corporate bureaucracies into local power structures, the extent to which cultural themes and patterns of consumption are affected by diffusion from international centers, and so forth.

Much of the existing literature on such subjects is essentially descriptive of ongoing trends. Its most valuable future products are not likely to be those which attempt to apply earlier principles to "explain" each and every situation but rather those which try to cope afresh with the rapidly evolving characteristics of external transactions and domestic dependence (Frank 1974).

A second likely trend stems from limitations of past dependency studies, noted above. It will have to do with conditions and constraints affecting national development efforts. An abundant literature on many of these topics already exists, but it has not been successfully integrated into a coherent framework. Analysis of past and current efforts and strategies of development in countries of Latin America, Africa, and Asia furnished the major historical source for these studies.

Topics will include the origins and socialization of political elites; the uses of ideology for mass legitimation; the demographic, educational, economic, and cultural constraints which different development strategies face; the interplay of coercion and charisma in achieving acceptance of consumption sacrifices; the limitations of democratic, totalitarian, and authoritarian political systems at different stages of development; allocative decisions in "balanced" and "unbalanced" strategies of economic growth; choices between "pure" economic development and "social capital" investment; identification of those sectors tapped as sources of support for development and of those "made to pay" its costs; consequences of "growth poles" versus decentralized investment decisions; processes of exchange among the state, transnational finance, and national entrepreneurial groups; the role of organized labor as source of redistributive demands or instrument for increasing productivity; and so forth.

These topics form a partial and crudely outlined agenda of matters which development scholars are likely to undertake in the future. Many have precedents in the past, but the marked shift in direction represents a significant departure from earlier experience. Students of Third World societies tended in the past to enter this field in a somewhat casual manner. Underdeveloped societies appeared to be appropriate settings in which to test theories evolved in more advanced contexts or on which to try explanatory frameworks based on different historical experiences. At other points, they have appeared to be ideal recipients of charitable inten-

tions, and the sociology of development has been limited to analysis of the welfare aspects of the process.

Colonies first and weak nations second had to accept "tribal" comparisons and stereotypes in lieu of analysis of their own situations. Times have changed, but fulfillment of the current promise offered by this field is contingent on abandonment by sociologists of the naive notions and extrapolations of the past. For the sociology of development to contribute in proportion to current expectations, it must revert to those who, in one way or another, have acquired close familiarity with the internal problems of "peripheral" societies and the external constraints under which they must survive.

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Size and the Density of Interaction in Human Aggregates¹

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A baseline model is developed to show the conduciveness of population size to the density of interaction in human aggregates. This model permits deduction and, therefore, explanation of a wide variety of social phenomena. It explains the positive relation between rates of crimes of violence and city size indicated by Webb's (1972) data. And it provides a formal rationale for Kasarda's (1974) hypothesis relating size and communication in macrostructures, as well as an alternative to some aspects of the theory formulated by Hamblin, Jacobsen, and Miller (1973). Our model accounts for the "transitory" and "superficial" nature of urban social relations commented upon by Simmel (1903), Wirth (1938), and Milgram (1970) and explains why inhabitants of large cities are unlikely to intervene in crises. In conjunction with considerations noted by Mayhew et al. (1972), the model explains why role strain may be expected to increase with organizational size, as illustrated in Snoek's (1966) national study. The wide variety of phenomena illuminated by this model suggests that it is a serious mistake to view the effects of population size as "obvious." Rather, taking population size explicitly into account should enhance the explanatory power of sociological theories.

An extensive literature, both classical and contemporary, has called attention to the relationship between the size of human aggregates and a variety of sociological phenomena presumed to be direct consequences of size itself (e.g., Simmel 1903; Wirth 1938; Meier 1962; Milgram 1970; Kasarda 1974). Milgram has summarized a variety of evidence suggesting that the size of urban aggregates has exactly those effects previously claimed by Simmel and Wirth: most notably, decreased interaction time and increased anonymity. Similarly, Kasarda has provided indirect evidence of structural growth as a consequence of the proliferation of communication networks indicated by Meier.

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While the work of both Milgram and Kasarda surpasses earlier efforts in bringing more systematic data to bear on the questions they address, they follow the example of their predecessors in assuming that the potential effects of size are sufficiently obvious that they require no explicit formulation. That this is not the case will become clear from the fact that an explicit statement of the structural conduciveness of size to increased density of interaction reveals far more in the way of explanation than may be obtained through the implicit assumption.

The objective of the present inquiry is to develop a model of the conduciveness of population size to the density of interaction in human aggregates. Having done this, we shall indicate a wide variety of social phenomena that are explained by deductions from the model, including such variables as abbreviated interaction time in urban settings, role strain, violent crime, and growth rates of federal bureaucracies. The rationale for this undertaking is that it should show how sociologists can increase the scope and rigor of their explanations by taking population size explicitly into account in theory construction.

A BASELINE MODEL OF EXPECTED CONTACT

In order to establish an expectation for the number of contacts (encounters, links, interactions, etc.) that may occur among the elements of a population aggregate, we must have maps of all the logical possibilities. Such maps are supplied by the mathematical theory of graphs. To paraphrase Harary (1967, p. 3) a graph consists of a finite set of points and a set of lines, each of which joins two distinct points. The number of points in a graph is here interpreted as the size of a population aggregate. Similarly, the number of lines indicates the number of contacts among population elements. All distinct (nonisomorphic) graphs of order four (population size four) are shown in figure 1. These graphs map the number of distinct ways in which contacts or interactions of any kind can occur in a population of size four, varying from no contacts at all to contacts between every pair of population elements.

A baseline model for the expected number of contacts in a population of a given size is established by assigning the same probability of occurrence to each distinct graph that can be mapped on that population. This procedure makes the likelihood of any given contact configuration (line configuration) uniform; the way in which contacts occur is random. This permits us to determine how many contacts to expect by chance alone.²

For convenience of exposition, we adopt the following symbols and their

² The type of model described here is the same as those developed by early mathematicians to establish baselines for the study of any natural phenomenon (e.g., Laplace 1812, pp. 349-62).

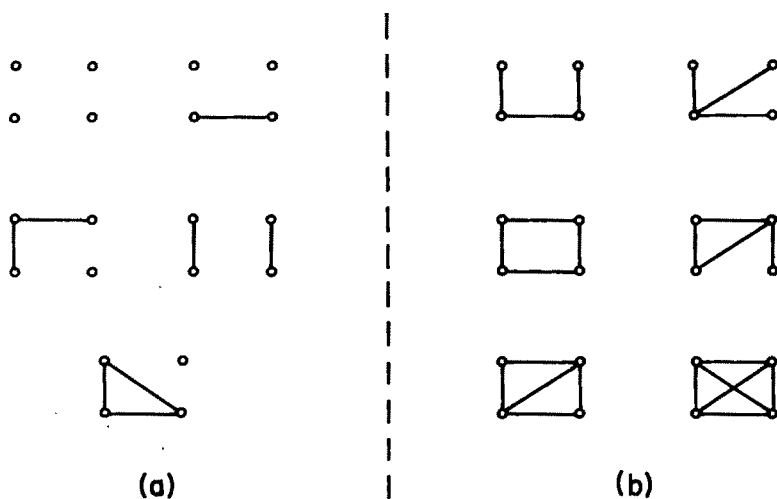


FIG. 1.—All distinct graphs of order four (size four)

definitions: S = the number of population elements, that is, the order of a population's graph; L = the number of contacts in a distinct population of given S , that is, the number of lines in a distinct graph with S points; N = the number of distinct graphs mapping contacts in a population of size S ; and, C = the expected density of interaction, that is, the number of contacts that would be expected to occur in a population of size S by chance alone. Observe that the maximum number of contacts (L_{\max}) which can occur in a population of size S is

$$L_{\max} = \frac{S(S-1)}{2}. \quad (1)$$

Let $g = 1, 2, 3, \dots, N$ be an index for the distinct graphs that can be mapped on a population of size S , and let p_g be the probability of g . As indicated previously, we establish a baseline model for the contacts in a population by assigning the same likelihood of occurrence to each g of S . Accordingly, we define the probability density function

$$p_g = \frac{1}{N}, \quad (2)$$

which generates the underlying line distribution for the g 's that map S . This line distribution is symmetric about its mean (Harary 1955, p. 451), from which it is determined that the expected number of contacts (or lines) in a population of size S is

$$C = \frac{L_{\max}}{2}, \quad (3)$$

that is, the number of contacts, interactions, encounters, etc. that we expect to occur by chance alone in a population of size S .

Equations (2) and (3) permit us to establish basic theorems concerning the conduciveness of aggregate size to the density of interaction in human populations.⁸

Aggregate theorem 1: Expected density of interaction is an increasing function of aggregate size.

That is, the rate of change in C with respect to S is positive; specifically,

$$\frac{dC}{dS} = \frac{S}{2} - \frac{1}{4}. \quad (4)$$

This leads to the next theorem:

Aggregate theorem 2: The rate of increase in the expected density of interaction with respect to aggregate size is an increasing function of aggregate size.

That is, the rate of change in the rate of change is positive; specifically,

$$\frac{d^2C}{dS^2} = \frac{1}{2}, \quad (5)$$

from which it is clear that:

Aggregate theorem 3: As aggregate size increases without bound, expected density of interaction increases without bound.

Specifically,

$$\lim_{S \rightarrow \infty} C = \infty. \quad (6)$$

The implications of these theorems are illustrated in figure 2, which graphs equation (3) as a function of size. As figure 2 clearly illustrates, an additive increase in aggregate size is accompanied by a multiplicative increase in the expected density of interaction. In other words, as the size of a population aggregate increases, its interaction potential increases at an increasing rate.

Equation (3) also permits us to arrive at the interaction potential of individual population elements, that is, the *expected number of contacts*

⁸ Because we are using nonisomorphic graphs, the distinct structural forms illustrated in fig. 1, it might be thought that we have not adequately considered all configurations of events, due to the fact that individual points are not identified. However, the underlying line distribution for labeled graphs (graphs in which each point is identified) is also symmetric about its mean and its mean is identical to equation (3). All our results will be the same for labeled or unlabeled graphs. Also, Kephart's (1950) work might lend the impression that graphs, because they are based upon dyadic relations, do not consider all the possibilities for interaction among subsets in the aggregate. The impression is incorrect, for Kephart was making additional assumptions. There is no configuration of interaction among population elements that is not taken into account by the underlying line distribution of graphs.

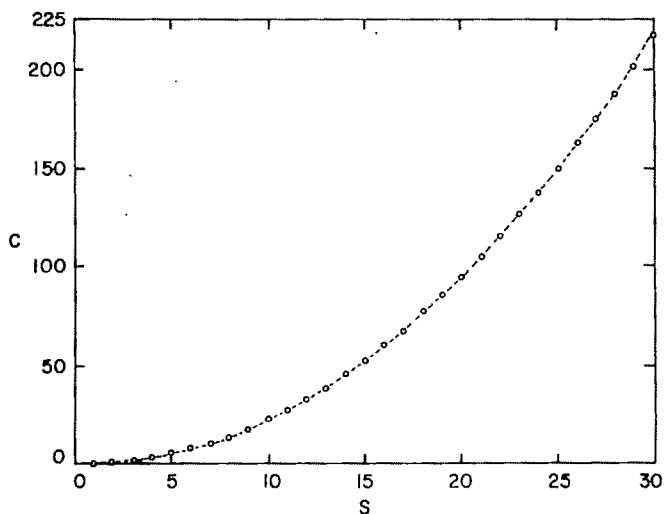


FIG. 2.—Expected interaction density (C) as a function of aggregate size (S)

or interactions per population element. This expected number will be called *expected element density* (symbolized D) and is given by

$$D = \frac{(S-1)}{2}. \quad (7)$$

This leads to the conclusion that:

Aggregate theorem 4: Expected element density is an increasing function of aggregate size.

That is, the rate of change in D with respect to S is positive. Specifically,

$$\frac{dD}{dS} = \frac{1}{2}, \quad (8)$$

which leads to the further conclusion that:

Aggregate theorem 5: The rate of increase in expected element density with respect to size is a constant.

That is, the rate of change in the rate of change is zero:

$$\frac{d^2D}{dS^2} = 0. \quad (9)$$

Accordingly,

Aggregate theorem 6: As aggregate size increases without bound, expected element density increases without bound.

Specifically,

$$\lim_{S \rightarrow \infty} D = \infty. \quad (10)$$

Size and the Density of Interaction

In contrast to the multiplicative effect of aggregate size on the expected density of interaction in the population as a whole, additive increases in aggregate size yield additive increases in expected element density, the expected number of contacts per population element.

However, since finite population elements, such as humans, are by definition entities with a finite amount of time to devote to the total stream of incoming contacts specified in equation (7), it is necessarily the case that the expected proportion of time (symbolized A) an element can devote to each contact is a decreasing function of aggregate size, according to the equation

$$A = \frac{2}{(S - 1)}. \quad (11)$$

Thus:

Aggregate theorem 7: Expected time per contact is a decreasing function of aggregate size.

That is, the rate of change with respect to aggregate size is negative:

$$\frac{dA}{dS} = - \frac{2}{(S - 1)^2}. \quad (12)$$

Furthermore:

Aggregate theorem 8: The rate of decline in expected time per contact is an increasing function of aggregate size.

That is, the rate of change in the rate of decline is shifting toward zero:

$$\frac{d^2A}{dS^2} = \frac{4}{(S - 1)^3}. \quad (13)$$

Finally:

Aggregate theorem 9: As aggregate size increases without bound, expected time per contact approaches zero.

That is,

$$\lim_{S \rightarrow \infty} A = 0. \quad (14)$$

The general implications of these trends are illustrated in figure 3, which graphs equation (11) as a function of aggregate size. As figure 3 clearly shows, an additive increase in aggregate size generates a precipitous decay in the expected proportion of time a population element has for each potential contact. As aggregate size increases, the expected proportion of time per contact rapidly approaches zero.

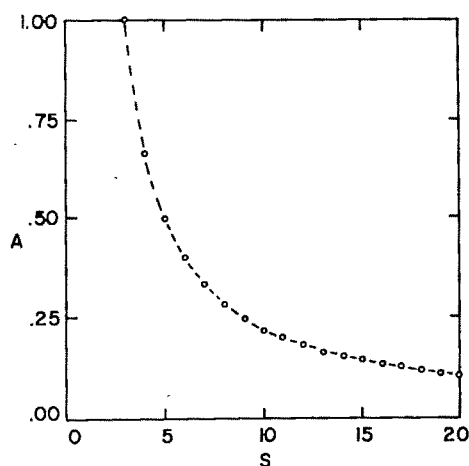


FIG. 3.—Expected proportion of time per contact (*A*) as a function of aggregate size (*S*).

TRANSITION

Our model is stated in abstract theoretical terms and we must effect a transition to the “observables” of concrete social life. Doing so requires that we first clarify several points about the nature of the model itself.

Maximum and Potential

Our “expected density of interaction” given in equation (3) is a statistical expectation based upon two critical items of information: (*a*) the maximum possible number of direct contacts is $[S(S-1)]/2$, and (*b*) the underlying line (contact) distribution is symmetric about its mean (again, see Harary 1955, p. 451). Knowledge of the maximum possible number of contacts is necessary but not sufficient to derive a statistical expectation for the number of contacts (relations, interactions, etc.) in a population aggregate. Without knowledge of the underlying line (contact) distribution, we cannot arrive at such an expectation. Accordingly, our model is not merely a reaffirmation of the well-known fact that the maximum possible number of contacts in a population of size *S* is $[S(S-1)]/2$.

Rather, our model corrects what appears to be a common misconception in the social science literature (e.g., Graicunas 1933; Caplow 1956; Mott 1965): the maximum possible number of contacts is *not* an estimate of the “potential” number of contacts in either an aggregate or an organized social system.⁴ The maximum possible number of contacts (or relations)

⁴ Our use of the term “aggregate” is to distinguish between populations in general, illustrated in fig. 1, and populations which are connected social systems, illustrated in fig. 1*b*. The underlying line distribution for connected graphs is not symmetric about

is relevant only to special categories of human groups. For example, Bossard (1945) used it correctly because: (a) he was not discussing a statistical expectation, and (b) he was discussing primary groups.⁵ Our statistical expectation is not confined to special cases. It refers to all human aggregates, whether or not they are organized social systems.

Dynamic Density

We interpret the statistical expectation for density of interaction as a theoretical expectation for what Durkheim called "dynamic density" (1893, p. 283). However, our theoretical expectation is based upon knowledge of aggregate size alone. It is not based upon all the factors Durkheim considered relevant to the production of dynamic density (1893, pp. 283-99). But there is no reason that it should be. We are interested in examining the effects of size, not the effects of other factors.

Nevertheless, other factors are important and require comment. We do not expect that aggregate size alone will have total and deterministic effects. Both physical distance and level of contact technology should have marked effects on (observed) density of interaction (Hawley 1971). The Law of Distance-Interaction states that the likelihood of interaction or contact of any kind between two social elements is a multiplicatively decreasing function of the distance between them, or of the costs of overcoming that distance (Carrothers 1956). This proposition holds over time and cross-culturally.⁶ The effects of distance are not merely present, they are profound, and they are documented by reams of empirical research (see Olsson 1965). The effects of distance pervade all aspects of social life, from marriage to crime (Turner 1969) and from the distribution of

its mean and gives rise to additional theorems not considered here. For present purposes, we need only note that for size 10 or larger, there are no noticeable differences between the expected number of contacts in connected social systems and population aggregates in general.

⁵ Bossard's statement of the Law of Family Interaction is as follows: "With the addition of each person to a family or primary group, the number of persons increases in simple arithmetic progression while the number of personal interrelationships within the group increases in the order of triangular numbers" (1945, p. 292). This is a deterministic relation based upon the definition of a primary group as a system in which *all members stand in the same social relation to one another*. A primary group exists only if all members maintain reciprocal personal relations with one another, as Bossard's statement clearly specifies (cf. Cooley 1909, p. 23). For this reason it is definitionally determined that the number of relations in a primary group is always the maximum number. However, since personal social relations cannot be sustained on this scale in large aggregates, Bossard's law applies to a very restricted size range, and Bossard never claimed otherwise.

⁶ In ancient Mesopotamia (Johnson 1972), in pre-Columbian America (Marcus 1973), in preindustrial Europe (Braudel 1966), and in contemporary societies, both agricultural (Johnson 1970) and industrial (Zipf 1949).

medical doctors (Kühne 1937) to the evolution of empires (Taagepera 1968).⁷

By "level of contact technology" we mean the relative efficiency of the technical means of communication and transport in overcoming the "drag effect" of distance. While increases in the relative efficiency of this technology have never reduced the effects of distance to zero in any society, both over-time and cross-cultural differences in the level of contact technology have sharp ratchet-like (step function) effects in reducing the impact of distance on interaction (Hawley 1971). Consequently, however powerful the effects of aggregate size may appear to be in our subsequent examples, a thorough analysis of interaction density would ultimately involve the variables of distance and level of contact technology (Schnore 1958).

Structural Conduciveness

Our model of interaction potential is a model of structural conduciveness. It says that increases in aggregate size (over time or cross-sectionally) are conducive to increases in the number of contacts or interactions. It says nothing about why people engage in such contacts or interactions in the first place. It predicts, for example, that as aggregate size increases additively, both the number of phone calls and the number of homicidal assaults will increase multiplicatively. But it says nothing about why people make phone calls or commit homicide. Rather, a model of structural conduciveness merely shows that, given a nonzero propensity to sustain contacts, aggregate size multiplies the opportunities for those contacts to be realized. Accordingly, we would predict that they will in fact be realized more often in larger aggregates.

This is not to say that there are no criteria for applying the model to particular aggregates. Durkheim (1893) has already supplied two criteria that specify how well it should work. First, the larger the distance between population elements, the lower is the likelihood that the model will apply. This rules out most arbitrary attempts at gerrymandering. Second, the higher the level of contact technology within an aggregate, the greater the probability that the model applies. However, the principal criterion must always be theoretical relevance. If an aggregate is deemed a meaningful system of interaction to which other sociological theories are applicable, then the model should apply to that aggregate. Thus, if cities are meaningful units for sociological analysis, as sociological theories about communities would claim, then our model applies to cities. Otherwise, it would

⁷ We do not include the effects of distance here because models for estimating the effects of distance already exist (e.g., Beckman 1968; see also von Thünen 1826; Weber 1909; Christaller 1933; and Lösch 1940).

not. The same criterion holds for groups, organizations, associations, and even whole societies. Our model is intended to supplement other theories about interaction systems. It is not proposed as an alternative to them.

Lines of Contact

The lines of contact or communication illustrated in figure 1 may be very transitory or very stable. Transitory contacts could vary from eye contact among sidewalk strollers to homicidal assaults between strangers, or from ticket purchases in an airport to casual conversations at a cocktail party. Stable contacts range across the entire spectrum of social organization, from lines of communication among employees in formal organizations to interaction among family members.

The expectation for lines of contact given in equation (3) refers to all contacts sustained in an aggregate at a given point in time, regardless of whether some are stable and others transitory. On the other hand, this does not mean that the observed number of contacts enumerated in an empirical study will exactly correspond to the expected number in equation (3). It probably would not, partially for reasons given in theorems 7-9 and discussed in the next section. However, the multiplicative increase predicted in equation (3) is used to estimate the number of contacts we expect to occur empirically. This is not because we think that the expected and observed numbers will exactly correspond, but we expect the observed number to grow according to the function given in equation (3). The observed and expected numbers should differ by no more than a linear change of scale. For this reason, it is the form of the function in equation (3)—not the exact number it yields—which serves as a guide to prediction.

Similarly, all subsets of contacts will be estimated by the same function (eq. [3]). For example, we expect the number of pairs mapped by the relation of "kissing" to grow according to the same function which predicts an increase in the number of pairs mapped by the relation "homicidal assault." The reason for this is that—for present purposes—the only difference in types of human contacts is the amount of time and energy required to sustain them, and these considerations are governed primarily by physical distance and level of contact technology.

Prediction

There are limits to growth in any finite system (von Bertalanffy 1950, pp. 144-46). Multiplicative growth will stop short of infinity. The same contingency applies to linear increases in scale. Ultimately, therefore, finite systems cannot maintain even linear increases in their principal components. These restrictions on growth apply to relationships among

finite population elements (Goldsmith 1971). Initially the actual volume of contacts may grow according to the multiplicative increase indicated in equation (3). But the decay function in equation (11) guarantees that such growth cannot be sustained indefinitely. As aggregate size increases, theorems 7-9 rapidly depress time per contact, so that the rate of increase in contacts must attenuate.

The interaction of equations (3) and (11) should therefore have distinct empirical consequences. The observed density of interaction (symbolized C_o) should initially increase at a multiplicative rate, but ultimately diminish to a relatively slow pace. Consequently, the relationship between observed density of interaction and aggregate size will be described by an S-shaped logistic curve, as illustrated in figure 4. Both the observed

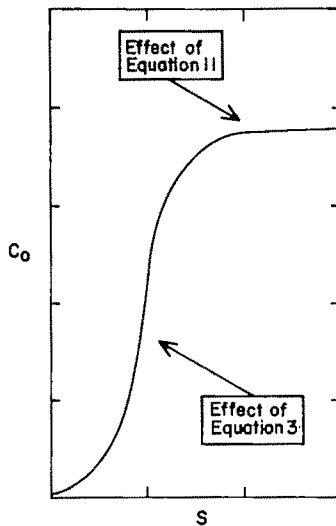


FIG. 4.—Predicted relationship between observed density of interaction (C_o) and aggregate size (S).

volume of kisses and the observed volume of homicidal assaults might then be reasonably⁸ approximated by the function:

$$C_o = a + b(\ln S), \quad (15)$$

⁸ The logistic curve in fig. 4 has an upper bound, while the logarithmic transform in equation (15) does not (the mathematical properties of logistic curves are given in James and James 1968, p. 223). Equation (15) is suggested as an approximation because: (1) in the presence of measurement error and limited variable range, it may be difficult to distinguish between the logistic curve and the simple logarithmic transform; and (2) it is the closest approximation to the logistic curve we are likely to find employed in the studies we wish to cite as examples in subsequent discussion. Note that equations (15) and (16) are not part of our baseline model. They are not mathematically derived from it. They are our estimates of what the data are likely to show as a result of the interaction of equations (3) and (11).

at any given point in time, with the constants a and b determined by such considerations as (1) average distance among population elements and (2) level of contact technology.

The linear increase in element density predicted in equation (7) is subject to the same general restrictions. While the observed number of contacts per population element (i.e., observed element density, symbolized D_o) may initially grow according to equation (7)'s linear function, beyond some point in size the decay function in equation (11) will have its inevitable attenuating effect. The empirical result will be a relationship of the form shown in figure 5, which graphs observed element density as a function of aggregate size. The initial linear increase in observed element density will begin to level off under the impact of equation (11) in the upper range of size.

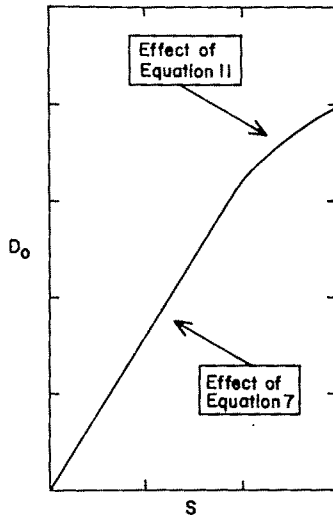


FIG. 5.—Predicted relationship between observed element density (D_o) and aggregate size (S).

However, because the initial increase in observed element density is predicted to be linear, there may be no well defined function which adequately describes the empirical result illustrated in figure 5. On the other hand, it is possible that equation (11) will begin to have its effect relatively low in the size range. Should this occur, the relation between observed element density and aggregate size *might be* of the general form:

$$\ln D_o = a + b(\ln S), \quad (16)$$

with the constants a and b determined by the same considerations suggested for observed density of interaction. However, this can only be empirically determined.

EMPIRICAL ILLUSTRATION

In the examples which follow, we shall see that deductions from our model predict variation in many types of social phenomena. In this sense, our model constitutes an explanation for the variation observed (Hempel and Oppenheim 1948). However, as indicated previously, we are not suggesting that aggregate size provides all the explanation. Our model is no more than an attempted first step toward unraveling the determinants of dynamic density.

Violent Interaction

Violent contacts are a subset of all human contacts and may serve as a beginning example of our model's applicability. Civil authorities keep records on some types of violent contacts and criminologists have suggested that violence may be an emergent property of social interaction (Ball-Rokeach 1973, p. 748). Bearing in mind that the reporting of crimes leaves much to be desired in the way of accuracy (Black 1970), we may consider the homicide potential, robbery potential, and assault potential of population aggregates.

In the United States of North America, violent crimes such as murder, robbery, and aggravated assault are usually reported as rates, ordinarily as the number of such crimes per 100,000 population. The decision to report violent crimes in this fashion directly reflects the assumption that increased population creates increased opportunities for such crimes. Furthermore, the expression of crimes per 100,000 population makes explicit the assumption that the effect of aggregate size is additive. As equation (3) shows, this is a false assumption. The opportunity structure for murder, robbery, and aggravated assault increases at an increasing rate with aggregate size. Accordingly, theorem 2 leads to the deduction that murder rates, robbery rates, and assault rates as usually expressed will be increasing functions of city size because they do not take the multiplicative effect of aggregate size into account.

As illustrated in table 1, where the indicated rates are compared for various size categories of U.S. cities in 1970, with the exception of the reversal in the two lowest size categories under aggravated assault, the trends verify the prediction deduced from theorem 2. The size categories in table 1 are crude, but this does not invalidate our comparison. If the effect of population size were only additive, then no trends would appear regardless of the intervals employed. But they do appear, and in the direction predicted by theorem 2. However, crude categories do not permit us to evaluate the nature of the relationship itself. A recent study by Webb (1972) permits us to examine this question.

TABLE 1

RELATIONSHIP BETWEEN SELECTED VIOLENT CRIME RATES AND CITY SIZE
CATEGORIES IN THE UNITED STATES, 1970

CITY SIZE CATEGORY (POPULATION)	SELECTED VIOLENT CRIME RATES*		
	Murder	Robbery	Aggravated Assault
250,000 or more	17.5	589	334
100,000-249,999	10.0	199	218
50,000-99,999	5.2	110	143
25,000-49,999	4.2	82	117
10,000-24,999	3.3	42	105
Less than 10,000	2.6	24	108

SOURCE.—U.S. Bureau of the Census (1973, table 226).

* Crimes known to the police per 100,000 population.

Webb studied 682 urban places in the United States, all with populations of 25,000 or larger. In spite of this restricted size range, he found (1972, pp. 650-51) that a logarithmic transformation of aggregate size explained more variance in violent crime than any other factor. That is, violent contact—as measured by violent crime—was best predicted by aggregate size through the form of the relation specified in equation (15), the results we expect on the basis of theorems 2 and 7 (from eqs. [3] and [11]). Webb's finding was not particularly strong ($r = .44$), but this may be partially attributed to the limited size range in his data.⁹ A more extended range is required for a full test of the relation. In any case, his data confirm the predictions from our model indicated in equation (15).

Abbreviated Interaction Time

Simmel (1903, pp. 188-89) and Wirth (1938, p. 1) noted the decline in interaction time (per contact) associated with increases in city size. They drew attention to the superficial and transitory nature of urban interaction and to increased anonymity. Milgram has more recently taken up their theme, and it is our intention here to show that our model formalizes the general nature of their observations.

Milgram attempts to account for many regularities in the behavior of urban dwellers by reference to the effects of aggregate size (which he simply assumes) and to the concept of information overload. He summarizes the nature of responses to input overload created by aggregate size as follows: "One adaptive response to overload . . . is the allocation of less time to each input. A second adaptive mechanism is disregard of

⁹ The relatively weak relation might also be due to Webb's use of a composite index of violent crime (1972, p. 646). Aside from the fact that the opportunity structure for forcible rape is different from the other types of crimes, aggregating rates in this fashion could weaken the relation.

low-priority inputs" (1970, p. 1462).¹⁰ In other words, one response to the interaction storm created by aggregate size involves reduced time for any given input. This response is offered in explanation of the superficial and transitory character of urban social relations.

We would not deny that the responses in question describe the behavior of individuals adjusting to overload (see Miller 1956; Miller 1962; Broadbent 1971). However, Milgram is merely describing a process; he is not providing an explanation for its occurrence. Our concern here is to note that the decreased interaction time of urban dwellers is logically implied by our theorems 7-9; our model constitutes an explanation of decreased interaction time per contact. That is, since humans are by definition finite organisms with a finite amount of time to devote to the total stream of incoming signals, it is necessarily the case that the average amount of time they can devote to the increasing volume of contacts specified in equation (7) is a decreasing function of aggregate size according to equation (11). This will occur by chance alone.

Furthermore, it tells us why the vast majority of contacts in large urban centers cannot be based upon "primary" social relations. By definition, primary relations require a great deal of time in interaction to be sustained. This does *not* imply that all primary contacts will disappear with increases in aggregate size. It does imply that *most* contacts will not have the nature of primary social relations.¹¹

And this consequence of our model also explains why anonymity is expected to be an increasing function of aggregate size. As size increases, average time per contact tends toward zero. The finite processing capacity of the human nervous system receives smaller and smaller amounts of information about each additional person encountered, and in very large aggregates many of the potential pair contacts are never even realized. Thus, as aggregate size increases it will ultimately reach a point where attaching labels to unique individuals surpasses the input-and-retrieval

¹⁰ Milgram actually lists a total of six adaptive responses to overload. However, the four others are either (1) special cases or interpretations of the two listed in the text, or (2) responses not of individuals but of social systems (cf. Meier 1962). Furthermore, the second of the two listed in the text is not peculiar to contact overload. Rather, it is a constant feature of the human nervous system acting to filter overloads of all types (see von Foerster 1966).

¹¹ It should be noted that this consequence of our model is in no way inconsistent with the findings of Kasarda and Janowitz (1974). Their work suggests that there is no reason to expect a decline in sociability with increasing aggregate size. This is not the same thing as the relative frequencies of primary and secondary relations in the work of Tönnies (1887) and Wirth (1938). The prediction from our model is that the relative number of primary contacts will decline—not the absolute number. The absolute number of primary contacts may well increase with size. A relative increase in secondary contacts in no way implies a decline in the absolute number of primary contacts. Our model is therefore consistent with Tönnies, Wirth, and Kasarda and Janowitz.

capacity of the human nervous system.¹² Beyond this point, it follows that the proportion of the population any person can identify is a decreasing function of size and, therefore, that anonymity is an increasing function of size. Theorems 7-9 (from eq. [11]) formalize the relation between aggregate size and anonymity.¹³

Returning to Milgram's (1970) discussion, another aspect of urban life which he attributes to reduced time available for any given contact is the failure of urban dwellers to intervene in a crisis. The example he cites is the failure of Kew Gardens residents to render aid in the 1964 Genovese murder in Queens, New York. The question is more general and is answered by the observation that the expected amount of time a person (selected at random) will devote to a contact (selected at random) is a rapidly decreasing function of size. Our theorems 7-9 formalize this prediction; they provide a deductive explanation for failure to intervene in crises in large aggregates. Thus, if we insert the 1964 population of metropolitan New York into equation (11), we discover that even on the basis of a 24-hour day, the average amount of time a New Yorker would be expected to devote to a signal (contact) selected at random is considerably less than it takes to phone the police.

Our primary concern in this section has been to indicate how our model formalizes many existing notions and observations about abbreviated interaction time and its consequences. Actual data are thin. Milgram (1970) has summarized most of the studies in this area, and some of them are relatively inconclusive.¹⁴ However, there is one study which confirms our predictions on the consequences of abbreviated interaction time (Dodd 1957). As aggregate size increases, declining time per contact forces individuals to respond to fewer and fewer of the social signals which impinge upon them. The larger the aggregate, the lower is the probability that a person selected at random will respond to a contact selected at random—as we have argued above in discussing crises. Dodd's (1957) study of leaflet

¹² It has long been recognized that time sequence in the action of the human nervous system is critical to all its operations (von Monakow 1914) and that humans, like computers, take in information in serial order (Simon 1974). The basic problem with respect to anonymity arises not from memory capacity alone but from time sequence patterns in the input of information into primary memory, throughput to secondary memory, and retrieval of information from secondary memory. The frequency and duration of contacts are therefore critical. Time per contact governs anonymity and our model specifies time per contact as a function of aggregate size.

¹³ Anonymity itself may be expected to have structural consequences. The use of uniforms for identifying categories of persons is one information standardization device found in large, complex systems (Joseph and Alex 1972).

¹⁴ This is due, we think, to the relatively small size differences involved. Perhaps the strongest demonstration of the effects of anonymity in a large aggregate is provided by Turner's (1969) study of delinquency. However, his data are from one city and do not permit evaluation of our model.

drops on six cities ranging in size from 1,304 to 325,994 confirms this prediction. He finds that the proportion of persons responding to an extraneous social contact is a monotonically decreasing function of city size.¹⁵ This is a direct confirmation of the consequences implied in theorems 7-9.

Aggregate Size and Role Strain

Goode identifies two basic conditions that contribute to role strain, that is, to "felt difficulty in fulfilling role obligations" (1960, p. 483). One condition has to do with the inherent contradiction contained in diverse expectations that attempt to elicit (logically) mutually exclusive responses (e.g., Field 1953). The second condition has to do with time overloads generated either by many different obligations or by obligations that call for different behaviors simultaneously (e.g., Killian 1952). We shall consider both of these conditions.

From our model we see that as size increases, the number of persons who may be expected, by chance alone, to transmit expectations to any one member of the aggregate increases according to the linear function in equation (7). Furthermore, since each of the expectations will require for its fulfillment some nonzero amount of time, and since the role incumbent is a finite organism, it follows that the amount of time that the expectations will ultimately come to require is greater than the time available to the role incumbent. Similarly, as equations (11)-(14) show, as the number of expectations increases with aggregate size, the average amount of time the role incumbent will devote to each, by chance alone, will steadily shift toward zero, a condition hardly likely to please the transmitters of the expectations. Thus, our model predicts that as size increases, the total amount of time that will be demanded of the role incumbent will ultimately exceed his capacity *and* that the total amount of time he has available to satisfy any given demand steadily declines. We can think of few conditions more likely than these to lead to "felt difficulty in fulfilling role obligations." Our model predicts that role strain due to time pressure will be an increasing function of aggregate size.

Mayhew et al. (1972) developed a baseline model of the relation between aggregate size and structural differentiation. Instances of structural

¹⁵ Although there is insufficient space to give a detailed outline of Dodd's (1957) study design, it should be noted that he began with the assumption that the relevant set of relations is the maximum number, to which he added the idea that people communicate with themselves. Consequently, the expectation he used was the function S^2 . At large values of size, as in Dodd's study, our own model differs from this value by no more than a linear change of scale, because S^2 is the upper limit of our L_{\max} in equation (3). In verifying his own model, Dodd directly verified ours.

differentiation included differentiation of the population into roles, statuses, subgroups, etc. Their model showed that (1) structural differentiation is an increasing function of aggregate size and (2) the rate of increase in structural differentiation is a decreasing function of size. These relations will hold by chance alone. According to both Merton (1957) and Goode (1960), an increase in role differentiation may be expected to lead to an increase in role strain due to increased possibilities for contrary expectations. Thus, the Mayhew et al. (1972) model leads to the prediction that role strain due to contrary expectations will be an increasing function of aggregate size.

To summarize, from our model on the relation between size and expected density of interaction and from the Mayhew et al. (1972) model on the relation between size and structural differentiation, we can logically deduce that role strain due to both time overloads and contradictory expectations will be an increasing function of aggregate size. These two models formalize the relation between aggregate size and role strain.

Snoek's (1966) national study of role strain in work organizations (1) shows a positive relation between role strain and organization size and, (2) presents no analysis which can be interpreted to show effects due to factors other than size.

Relying upon a 1961 sample of persons 18 or older living in private households in the United States, Snoek selects for his analysis the subset of persons who would be classified as (approximately) full-time wage or salary workers (a total of 596 persons). The focus of his analysis is on the role incumbent in the work organization and the expectations impinging upon the incumbent on the job. The variables he considers include (1) role strain, (2) organizational size, (3) role-set diversity (role differentiation among the incumbent's "role-senders"), (4) the incumbent's level of supervisory responsibility, and (5) frequency of interaction between the role incumbent and each category of his role senders. Snoek (1966, p. 365) offers two hypotheses: (1) that role strain will be an increasing function of role-set diversification and (2) that role strain will be an increasing function of organizational size. We have no objection to these hypotheses, but the first hypothesis must be supported independent of size.

Snoek (1966, pp. 367-68) begins with an analysis of the relation between role strain and the frequency of interaction within each of his five classes of role senders. In every role-sender category, there is a positive relation between role strain and interaction frequency, showing statistical significance in four out of five categories. The main point to note from this finding is that increased interaction frequency may reflect nothing more than the increased numbers of persons with whom interaction is

sustained, that is, the size of the interacting aggregate in each category. Thus, the first phase of Snoek's analysis is quite consistent with our size hypothesis and does not permit the interpretation that interaction frequency accounts for the results independent of size.

Snoek next considers the relation between (1) role strain and role-set diversity and (2) role strain and level of supervisory responsibility while controlling for role-set diversity. He finds a significant positive trend in both cases. However, since role-set diversity and number of supervisory levels are both instances of structural differentiation, the Mayhew et al. (1972) model predicts an increase in both as a function of size. The correlation between role strain and role-set diversity and between role strain and level of supervision may reflect nothing more than this aspect of size. Again, it is not clear that any variable other than size is operating.

Snoek also examines the relation between role strain and company size, finding a significant positive trend. This confirms the predictions of the two proposed size models, although it does not permit us to interpret the kinds of effects specified by each model because Snoek's measure of role strain does not distinguish between strain due to time overloads and strain due to contrary expectations.

Snoek then attempts to control for company size by collapsing his original set of size categories (with ranges 1-9; 10-49; 50-499; 500-4,999; and 5,000+) into two, labeled small (less than 500) and large (500+). Within each of these two gross size categories, Snoek examines (1) the relation between role strain and role-set diversity and (2) the relation between role strain and level of supervisory responsibility. In both instances he finds a positive relation within each size category. However, considering the size variability within each of the gross categories, and the predictions of the Mayhew et al. (1972) model for both types of differentiation, these results within size categories may still be due to nothing more than size. And, in the comparison *across* the two gross size categories (within each of the role-set diversity and supervisory level categories), the trend for a positive relation between role strain and size continues to hold for four out of six comparisons—in all three categories of supervision level and in one out of three role-set diversity categories—although statistical significance is achieved only in the comparison within the highest role-set diversity category. The trend continues to hold in the majority of cases, and since much size variation is concealed within gross categories, the extent of the trend may be underestimated. Owing to the mode of analysis, effective control for size is not achieved.

As far as Snoek's analysis goes, (1) it confirms the predictions of the two size models we have discussed, and (2) given his mode of analysis, there are no conclusions that can be drawn about factors which operate independent of size.

Communication and Coordination

Students of industrial society have remarked upon the rapid expansion of coordinative and communicative occupational specialties during periods of demographic and economic growth. Wagner (1892, p. 893) suggested that, under conditions of modern technology, state activity will expand both intensively and extensively. Thus, government spending in England grew by a factor of 6 between 1700 and 1800, while population grew by a factor of 2 and average real output in various economic sectors grew by a factor of 1.47 (Deane and Cole 1967, pp. 65, 76-78). In the United States, the number of federal employees grew by a factor of almost 500 between 1816 and 1961 while population grew by a factor of 20; and between 1940 and 1966 the number of state and local government employees grew more than 150% while population increased by only about 50% (Lenski 1970, pp. 365-366). There has been a corresponding tendency for the rapid expansion of tertiary or service occupations to be associated with intensive industrialization (Sauvy 1949; Deane and Cole 1967; Du Boff 1973).

Explanations for these structural changes have been various and uneven in quality. Sauvy (1949, p. 59) attributes the disproportional growth of the service sector of the economy to technological changes, while Lenski attributes disproportional increases in government to "the rise of the new democratic ideology" (1970, p. 366). Du Boff attributes all structural changes, including these, to an international capitalist conspiracy (1973, pp. 6-9, 19-24). There are other arguments, but we cannot review them here.

To the extent that population increases, multiplication of contacts and interaction of all kinds is predicted by our model in theorems 1-3, and many structural correlates may be expected. Task specialization with respect to communication functions is the clearest expectation, and Kasarda's recent analysis at three levels of system organization is a direct confirmation of this expectation. He finds that the proportion of the labor force in communication specialties is an increasing function of size for formal organizations, communities, and nations (1974, pp. 23-24). Furthermore, his finding (p. 24) that this correlation is with a logarithmic transformation of size directly confirms a structural response consistent with our expectations illustrated in figure 4 and specified in equation (15). And, just as his finding confirms our model, our model formalizes the underlying rationale for his analysis. His assumption (1974, p. 20) that difficulties of communication are likely to be the most important problems faced by expanding aggregates is correct to the extent that communication is predicted to grow multiplicatively and, ultimately, to exceed the information processing capacities of individuals (theorems 7-9). Structural adjust-

ments to overload must be developed if the population is to maintain a communication network. To the extent that the solution takes the form of occupational specialization, it is clearly predicted from our model that task specialization with respect to communication is of primary importance. Our model makes the same prediction for all types of aggregates, and Kasarda finds that his hypothesis on communication specialties holds at three system levels.

The fact that the growth of (observed) interactive density must ultimately attenuate (figure 4) may bear upon Kasarda's finding that coordinative components of occupational structure do not necessarily behave in the same fashion across system levels (1974, pp. 23-25). However, this is a complex problem, partly on account of definitions of coordinative-administrative occupations and especially owing to the fact that social systems can respond to coordination problems in several ways other than by increases in the number of administrative personnel (see Rushing 1966a, 1966b). On the other hand, Kasarda's finding of a reversal of the relations for coordinative-administrative occupational groups at the level of communities and societies may be due to the shift in the definition of his size base for these systems. A fuller test of the question is required.

Hamblin et al. (1973) have developed a powerful diffusion model to explain occupational expansion over time. Their model predicts over-time growth in numerous occupational categories, including those with communication and coordination (e.g., governmental) functions (1973, pp. 96-100). And, their model predicts that these phenomena will approximate a logistic curve (pp. 56-57) over time, for the same (finite system) reasons we have posited in our model.

In some respects our model is an alternative to the theory proposed by Hamblin et al. (1973); in other respects the two models are complementary. For example, our model predicts changes with size both over time and cross-sectionally (as in Kasarda's study), while diffusion models are confined to over-time predictions. Nevertheless, a diffusion model seems quite reasonable for adoption of entrepreneurial occupations (e.g., shopkeeper, attorney). But even in these instances, occupations will not be adopted unless there is demand for their services in the population. Our model specifies the nature of this demand for communicative-coordinative occupations as a function of aggregate size. In many instances it seems more appropriate to say that a system adopts individuals—as in the military draft. More generally, both persons and social systems seem to simultaneously adopt one another, so that the outcome might be described as an emergent property of interaction, rather than as a directional diffusion process. However this may be, a size model like the one presented here indicates pressure on populations to generate task specialization with

respect to both coordinative and communicative functions, independent of the specific nature of the adoption process.

There are yet firmer grounds for the view that our model is complementary to Hamblin et al. (1973). Tarde (1890, p. 19) suggested that the size and density of connections in a social system should affect the rate at which information—or any other item—diffuses in a population. He was correct: both rumors and viruses may be expected to move rapidly toward saturation level in large, densely connected aggregates. This is true on purely *a priori* grounds. The rate at which any diffusion process can occur is directly affected by both (1) the size of the population and (2) the density of connections (contacts) in that population. For this reason, a combination of our model with the Hamblin et al. (1973) diffusion theory should provide a stronger basis for prediction.

OVERVIEW

The interaction potential in a human population is a multiplicatively increasing function of aggregate size. The model we have presented to show this permits deduction and, therefore, explanation of a wide variety of social phenomena. The model predicts that rates for most violent crimes will be an increasing function of aggregate size, a prediction borne out in our illustration for U.S. cities and more specifically in Webb's (1972) study. The model predicts the reduced interaction time remarked by Simmel (1903), Wirth (1938), and Milgram (1970) in their attempts to describe the nature of urban life and is borne out by Dodd's (1957) study of six cities. It provides a formal explanation for anonymity, for the expected relative decline in primary contacts, and helps us understand why urban dwellers are unlikely to intervene in crises. The model predicts that role strain will be an increasing function of aggregate size and, in conjunction with the structural differentiation model of Mayhew et al. (1972), explains the findings in Snoek's (1966) national study. The model is verified by Kasarda's (1974) study of the relation between size and structural expansion of communication-related occupational specialties at three levels of system organization and provides an explicit deductive rationale for his hypothesis. Finally, our model appears to be an alternative to the diffusion model proposed by Hamblin et al. (1973) in some respects and complementary to it in others. A combination of the two models promises an increase in explanatory power. In summary, our model suggests that an explicit formulation of the effects of size provides far more in the way of explanation than does the implicit assumption.

In this paper we have followed the suggestion put forward by Durkheim (1898), Halbwachs (1938), Mackenroth (1953), Schnore (1958), Svalas-

toga (1965), and Kasarda (1974) that taking population structure directly into account is an important and legitimate line of sociological inquiry. As evidence accumulates, it becomes increasingly clear that aggregate size is a powerful explanatory variable, whether at the level of organizations (Blau 1970), communities (Abrahamson 1974), or nations (Sawyer 1967). We suggest that sociologists would gain by dispensing with the notion that the effects of population size are "obvious." Rather, the scope and explanatory power of sociological theories could be greatly enhanced by including size in an explicit fashion.

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Occupational Structure and Alienation¹

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This paper appraises two related hypotheses suggested by Marx's analysis of the occupational sources of alienation—one emphasizing control over the product of one's labor, the other emphasizing control over the work process. Using data from a sample survey of U.S. males employed in civilian occupations, it concludes that, in this large-scale, capitalist system, control over the product of one's labor (ownership and hierarchical position) has only an indirect effect on alienation, whereas control over work process (closeness of supervision, routinization, and substantive complexity) has an appreciable direct effect on powerlessness, self-estrangement, and normlessness.

Despite its ambiguity of meaning, alienation is an appealing concept, standing as it does at the intersection of social-structural conditions and psychological orientation. Certainly it has been the subject of a vast literature (see Geyer 1972). On the structural side, there is the fundamental Marxian analysis, focusing on the meaning for the worker of loss of control over his primary work role. On the psychological side, there is the extensive recent literature on all the ways in which estrangement from self and others can be expressed. But rarely are the social-structural and psychological aspects of alienation juxtaposed, and even more rarely in empirical analysis. This paper examines the relationship between social structure—in particular, occupational structure—and the subjective experience of alienation, under the conditions that exist in a large-scale, technological economy.

Our starting point is Marx's analysis of occupational structure. Marx emphasized ownership of the means of production and division of labor, both especially salient in the early stages of the industrial revolution (Israel 1971, pp. 30–62; Marx 1964, 1971). An exact test of the present-day pertinence of these occupational conditions to feelings of alienation would require cross-national comparisons to see whether workers in societies having capitalist systems feel more alienated than do workers in societies having noncapitalist systems, and whether workers in economies characterized by a highly specialized division of labor feel more alienated than do workers in less specialized economies. Yet, even within the United

¹I am indebted, for advice and essential help, to my associates—Margaret Renfors, Carrie Schoenbach, Carmi Schooler, and Erma Jean Surman; and for carrying out the survey on which the research is based, to Paul Sheatsley, Eve Weinberg, and the staff of the National Opinion Research Center.

States, with its capitalist system and its large-scale, highly specialized economic structure, it is possible to see whether employees feel more alienated than owners, and whether those who work in firms having a specialized division of labor feel more alienated than those who work in firms having less pronounced division of labor. We can also see whether having a position of authority—often thought of as the functional equivalent in large-scale enterprises of ownership in small-scale enterprises—is related to alienation.

As I interpret Marx's discussion, he was concerned with workers' loss of control over the means of production not only for its economic and political import, but also because it signified workers' loss of control over the essential conditions of their own occupational lives. When one sells his labor to an employer instead of working for himself, and when one makes some standardized part instead of an entire entity, he loses control over the "product" of his labor. One can also lose control over the "process" of his labor—by having to do work that does not engage his interests or challenge his abilities. Both a concern with loss of control over product and a concern with loss of control over process are evident in the *Early Writings*:

What constitutes the alienation of labor? First, that the work is *external* to the worker, that it is not part of his nature; and that, consequently, he does not fulfill himself in his work but denies himself, has a feeling of misery rather than well-being, does not develop freely his mental and physical energies but is physically exhausted and mentally debased. . . . His work is not voluntary but imposed, *forced labor*. It is not the satisfaction of a need, but only a *means* for satisfying other needs. . . . Finally, the external character of work for the worker is shown by the fact that it is not his own work but work for someone else, that in work he does not belong to himself but to another person. [1964, pp. 124–25]

Work that is "external" to the worker, in which he cannot "fulfill himself," comes close to being the opposite pole of what Schooler and I have called "self-directed" work—that is, work involving initiative, thought, and independent judgment (Kohn 1969, pp. 139–40; Kohn and Schooler 1973). Although many occupational conditions are either conducive to or deterrent from the exercise of occupational self-direction, we see three in particular as crucial—closeness of supervision, routinization, and substantive complexity. Insofar as workers are free of close supervision, perform a variety of tasks, and do work that is substantively complex, their work is necessarily self-directed. Insofar as workers are closely supervised, are caught up in a repetitive flow of similar tasks, and do work of little substantive complexity, their work does not permit self-direction. Believing that loss of control over the process of work is conducive to alienation, I hypothesize that being closely supervised, doing routinized work, and

doing work of little substantive complexity will result in feelings of alienation. I would further predict that, in a modern capitalist economy, dominated by large-scale enterprise, these occupational conditions will have a greater effect on feelings of alienation than whether one is owner or employee or whether one works in an enterprise having more or less pronounced division of labor.

There is solid empirical reason to make the same predictions. Schooler's and my analyses of the relationships between some 50 separable dimensions of occupation and each of several facets of psychological functioning (including, for example, intellectual flexibility, anxiety, self-esteem, and authoritarian conservatism), led us to conclude that "... occupational self-direction has the most potent and most widespread effects of all the occupational conditions we have examined. In terms of psychological effects, the central fact of occupational life today is not ownership of the means of production; nor is it status, income, or interpersonal relationships. Instead, it is the opportunity to use initiative, thought, and independent judgment in one's work—to direct one's own occupational activities" (Kohn and Schooler 1973, p. 116). This being so, it is probable that the conditions determinative of the degree of occupational self-direction—closeness of supervision, routinization, and the substantive complexity of the work—have an important effect on feelings of alienation as well.

The intent of this analysis, then, is to appraise two related hypotheses suggested by Marx's analysis of the occupational sources of alienation. One hypothesis, emphasizing loss of control over the *products* of one's labor, posits that ownership and hierarchical position are of crucial importance with respect to alienation (and also ascribes an important, if secondary, role to division of labor). The other hypothesis, emphasizing loss of control over the *process* of labor, suggests that (at least within an industrialized, capitalist society) such determinants of occupational self-direction as closeness of supervision, routinization, and substantive complexity overshadow ownership, hierarchical position, and division of labor in their effects on alienation.²

² Little of the empirical literature on the sociology and social psychology of work is directly pertinent to the central issue of this paper. Most occupational studies, of course, do not purport to deal with alienation. Of those that do, some use job dissatisfaction as their index of alienation. But extrapolating from job dissatisfaction or even from a lack of occupational commitment to feelings of alienation is unwarranted. Closer to my purposes are those studies that use as their index of alienation some measure of "alienation from work" (Aiken and Hage 1966; Miller 1967; Pearlin 1962; and Zurcher, Meadow, and Zurcher 1965). Pertinent as these studies are, though, it is unsafe to extrapolate from their findings about people's job orientations to a more general interest in people's orientations toward both occupational and non-occupational social reality. Other studies do not measure actual occupational conditions, but instead treat subjective reactions to the job as if these were the ultimate

The data on which this analysis is based come from a sample survey of 3,101 men, representative of all men employed in civilian occupations in the United States, who were interviewed for Schooler and me by the National Opinion Research Center in the spring and summer of 1964. The interview schedule and detailed information on the sample and the overall research design are given in Kohn 1969 (pp. 235-64). Further information about index construction and statistical methods is given in Kohn (1971), and especially in Kohn and Schooler (1973).

INDICES OF ALIENATION

Before assessing the occupational sources of alienation, one must face the fact that in social-psychological usage, "alienation" is an extraordinarily vague and imprecise term. It refers to people's conceptions of the external world and of self, in other words, to their orientations. Most definitions agree that alienation involves estrangement from (or disillusionment with, or lack of faith in) the larger social world or oneself. But there is little agreement on how large a segment of the orientational domain "alienation" should encompass. Lacking an adequate general definition, I follow Seeman's (1959) example, in his classic analysis of the historical uses of the concept, and use the term to refer generally to the five distinguishable facets of orientation that the term has come to imply: powerlessness, self-estrangement, normlessness, isolation (or cultural estrangement), and meaninglessness.

Seeman's analysis demonstrated that alienation has such disparate meanings that the term cannot be used analytically without specifying which meaning is intended. This conclusion remains valid, even though Seeman (1972*a*, 1972*b*) and others (Allardt 1965; Blauner 1964; Clark 1959; Dean 1961; Finifter 1970, 1972; Horton 1964; Israel 1971; Kon 1967; Nettler 1957; Olsen 1969; Zollschan and Gibeau 1964) have re-

source of alienation, irrespective of the occupational structures that give rise to such reactions. Thus, they measure boredom rather than routinization, interest in the work rather than substantive complexity, and "alienation in work" (or "work alienation") rather than actual working conditions. This approach ignores the possibilities that there can be a gap between the conditions to which a person is exposed and his awareness of those conditions, that the existence or nonexistence of such a gap is itself problematic and may be structurally determined, and that conditions felt by the worker to be benign can have deleterious consequences, while conditions felt to be onerous can have beneficial consequences. A few studies *do* examine the relationship between one or more occupational conditions and one or more aspects of off-the-job alienation (notably, Blauner 1964; Bonjean and Grimes 1970; Goldthorpe et al. 1969; Neal and Seeman 1964; Nelson 1968; and Tudor 1972). But not even these studies answer the major question of this paper, because they do not deal with (or do not directly measure) the dimensions of occupation central to my interests, or because their indices of alienation are inferential. (Tudor's study is an exception; see n. 13 below.)

defined the conceptualization of alienation in many ways. In fact, the principal controversy today revolves around the theoretical utility of retaining as an analytic concept a term with such diverse meanings (see especially Israel 1971).

I am of course interested in two types of alienation that have been central to theoretical discussion of the alienating effects of occupational structure—powerlessness and self-estrangement. But the other types—normlessness, cultural estrangement, and meaninglessness—also might be affected by occupational conditions. So there is utility in examining them all. To this end, Schooler and I attempted to develop indices of all five types of alienation. We actually succeeded in constructing adequate Guttman Scales of four of them,⁸ failing only with meaninglessness, which we could not differentiate empirically from the others. Since the four scales are basic to all my analyses, it is worth examining them carefully (see table 1).

1. Powerlessness. Seeman says that powerlessness "can be conceived as the expectancy or probability held by the individual that his own behavior cannot determine the occurrence of the outcomes . . . he seeks" (p. 784). The concept implies a continuum, from the individual's being at the mercy of external forces, to his having some degree of control over his own fate. Seeman speaks of powerlessness as a measure, not of "the objective conditions in society," but of "the individual's expectancy for control of events." It is not the fact of being powerless but the sense of being powerless that we were attempting to measure; whatever produces a sense of powerlessness, the psychological reality is that to feel powerless is to lack personal efficacy. This is what the index of powerlessness is meant to, and apparently does, reflect. Other indices in common use have more abstract referents, but even those designed to measure the individual's conception of how power is organized in the larger social order may actually reflect his sense of personal efficacy.

2. Self-estrangement. This is difficult to conceptualize and to index. Seeman says that it "refers essentially to the inability of the individual to find self-rewarding—or in Dewey's phrase, self-consummatory—activities that engage him" (p. 790). The concept overlaps with what I earlier (Kohn 1969, pp. 265–69) called self-deprecation, the negative component of self-esteem. But self-estrangement implies something more than a negative evaluation of one's own worth; it also implies a sense of being detached from self, of being adrift—purposeless, bored with everything, merely responding to what life has to offer, rather than setting one's own course. Our index attempts to capture this sense of detachment from self.

⁸ For enumerations of indices of alienation employed by other investigators, see Bonjean, Hill, and McLemore (1967), pp. 27–38; Robinson and Shaver (1969), pp. 161–210; and Seeman (1972a).

TABLE 1
GUTTMAN SCALES OF ALIENATION

Scale	Attribute
1. Powerlessness	<ul style="list-style-type: none"> a) Do you feel that most of the things that happen to you are the result of your own decisions or of things over which you have no control? b) I generally have confidence that when I make plans I will be able to carry them out (agree-disagree). c) There are things I can do that might influence national policy (agree-disagree). d) How often do you feel powerless to get what you want out of life?
2. Self-estrangement	<ul style="list-style-type: none"> a) How often do you feel that there isn't much purpose to being alive? b) How often do you feel bored with everything? c) At times I think I am no good at all (agree-disagree). d) Are you the sort of person who takes life as it comes or are you working toward some definite goal?
3. Normlessness	<ul style="list-style-type: none"> a) It's all right to do anything you want as long as you stay out of trouble (agree-disagree). b) It's all right to get around the law as long as you don't actually break it (agree-disagree). c) If something works, it doesn't matter whether it's right or wrong (agree-disagree). d) Do you believe that it's all right to do whatever the law allows, or are there some things that are wrong even if they are legal?
4. Cultural estrangement	<ul style="list-style-type: none"> a) According to your general impression, how often do your ideas and opinions about important matters differ from those of your relatives? b) How often do your ideas and opinions differ from those of your friends? c) How about from those of other people with your religious background? d) Those of most people in the country?

NOTE.—All four scales have reproducibilities in the .90s, scalabilities in the .70s, and essentially random patterns of error.

3. Normlessness.⁴ By Seeman's definition, this is the situation "in which there is a high expectancy that socially unapproved behaviors are required to achieve given goals" (p. 788). Our index is based on an implicit continuum—from the individual's believing that it is acceptable to do whatever he can get away with to his holding responsible moral standards. The beliefs that it is morally acceptable to do "anything you want

⁴The concept of normlessness is derived from Durkheim's "anomie" and its psychological counterpart, "anomia." Most empirical studies are based on indices derived from Srole's (1956, p. 711) definition of anomia as "self-to-other alienation." For informative discussions of the relationship between alienation and anomie/anomia, see Horton (1964) and Lukes (1972).

as long as you stay out of trouble," that it is legitimate to "get around the law," that "if something works it doesn't matter whether it's right or wrong," and that it's "all right" to take advantage of "whatever the law allows" provide our index of normlessness. (I must acknowledge, though, that believing that "there are some things that are wrong even if they are legal"—which we treat as the internalization of the moral norms of the larger society—may in some instances represent alienation from the society.)

4. Cultural estrangement (which Seeman originally called isolation). This refers, by his definition, to "assign[ing] low reward value to goals or beliefs that are typically highly valued in the given society" (pp. 788–89). Seeman thinks of cultural estrangement in terms of an individual's rejection of such allegedly dominant cultural themes as success and materialism. We have asked instead whether the respondent believes that his "ideas and opinions about important matters" differ from those of his friends, his relatives, other people of his religious background, and his compatriots generally. This seems preferable, in that it does not prejudice the dominant cultural themes or what the respondent assumes them to be. Still, we may have missed the essence of what Seeman and others mean by cultural estrangement. Another possible problem with our index (see Antonovsky and Antonovsky 1974) is that it fails to distinguish between estrangement from primary groups (friends and relatives) and estrangement from secondary groups ("people with your religious background" and "most people in the country"), which are included in the same index. But I do not believe this invalidates the index, for we have found a unidimensionality in such estrangement, regardless of whether primary or secondary groups are involved.

The intercorrelations among the four types of alienation, as thus measured, are generally small to modest,⁵ the only one higher than 0.15 being that between powerlessness and self-estrangement ($r = .25$). This finding buttresses Seeman's argument that the several types of alienation are indeed distinct. Further evidence is provided by my experience in subjecting all of the items from all four Guttman Scales to a factor analysis.⁶ Ordinarily, when the items in a factor analysis partake of the same general domain, they all correlate substantially with the first unrotated factor. This in effect measures how much they have in common. In this factor

⁵ Other studies (Dean 1961; Middleton 1963; and Simmons 1966) have found higher intercorrelations, but those studies are based on sufficiently different dimensions of alienation, or use such special samples, that their findings do not belie this conclusion.

⁶ This was an orthogonal principal component factor analysis. For earlier attempts to factor analyze the alienation domain, see Cartwright (1965); McDill (1961); Neal and Rettig (1963, 1967); and Struening and Richardson (1965). For a discussion of the technical issues involved, see also Anderson (1971), pp. 618–19.

analysis, all the items reflecting self-estrangement correlate highly with the first unrotated factor, but none of the others correlates nearly as highly; there is no general factor. This finding provides further evidence that the various components of alienation are separate and distinct, and should certainly be examined separately. The occupational conditions that underlie any one of them may or may not be pertinent to the others.

OCCUPATIONAL STRUCTURE

Three aspects of occupational structure are focal to my analysis: ownership and position in the supervisory hierarchy, division of labor, and the conditions that determine how much opportunity a person has to exercise self-direction in his work.

Indices of Occupational Structure

Ownership is indexed straightforwardly: the interviewers asked all men employed in profit-making firms, "Are you an owner?" For these analyses, I simply dichotomize owners and nonowners, disregarding the distinction between full and partial ownership. (An index of "degree of ownership" yields altogether consistent findings.)

Position in the supervisory hierarchy, which some consider the equivalent of ownership, is based on the respondent's estimate of how many people are "under" him—either because he supervises them directly or because they are under people he supervises.

I have no direct index of division of labor. The closest equivalent provided in this study is an index of bureaucratization, employed here on the assumption that the division of labor is generally most pronounced where bureaucratization is greatest. My index of bureaucratization (see Kohn [1971] for the complete rationale) is based on one principal dimension of bureaucracy, the hierarchical organization of authority or, operationally, the number of formal levels of supervision.⁷

As mentioned above, Schooler and I conceive three occupational conditions to be of crucial importance in determining the degree of occupa-

⁷ We asked the respondents: "Is this [an organization] where everyone is supervised directly by the same man, where there is one level of supervision between the people at the bottom and the top, or where there are two or more levels of supervision between the people at the bottom and the top?" To distinguish further, we assumed that when an organization reaches at least three levels of formal authority greater differentiation of structure is roughly proportional to size, at least in organizations of about 100-1,000 employees. Thus, the index of bureaucratization is as follows: (1) one level of supervision; (2) two levels of supervision; (3) three or more levels of supervision, fewer than 100 employees; (4) three or more levels of supervision, 100-999 employees; and (5) three or more levels of supervision, 1,000 or more employees.

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tional self-direction—closeness of supervision, routinization, and substantive complexity.

Closeness of supervision is indexed by a Guttman Scale based on five questions about how much latitude the supervisor allows and how supervisory control is exercised (see Kohn 1969, p. 153).

Routinization of the work (which we earlier called the complexity of organization of the work) is measured by the repetitiveness of tasks and the complexity of the "units" of work (see Kohn 1969, pp. 158-60).

The substantive complexity of the work is based on detailed questioning of each respondent about his work with things, with data or ideas, and with people (see Kohn 1969, pp. 153-55, 271-76). The index is a composite of seven ratings: our appraisals of the complexity of a man's work with data, with things, and with people; our appraisal of the overall complexity of his work; and his estimates of the amount of time he spends on each type of activity. A factor analysis of these seven ratings yields a single index of substantive complexity (see Kohn and Schooler 1973, p. 104, n. 15).

Occupation and Alienation

I begin the analysis of occupational structure and alienation by examining the correlations between the indices of occupational structure just enumerated and the four types of alienation (see table 2).

Consider, first, the relationships between occupational structure and the type of alienation most germane to Marx's analysis, powerlessness.

TABLE 2
CORRELATIONS OF ALIENATION SCALES WITH SELECTED OCCUPATIONAL CONDITIONS

Index	Self-		Cultural	
	Powerlessness	Estrangement	Normlessness	Estrangement
1. Ownership and hierarchical position:				
Ownership	-.04	-.02	-.03	.00
Position in the supervisory hierarchy	-.13	-.09	-.09	.07
Combined (multiple correlation) ..	.13	.09	.09	.07
2. Bureaucratization of firm or organization	-.09	-.09	-.11	.06
3. Occupational self-direction:				
Closeness of supervision16	.14	.23	-.11
Routinization of work08	.04	.13	-.06
Substantive complexity of work ..	-.19	-.17	-.26	.17
Combined (multiple correlation) ..	.21	.18	.29	.17

NOTE.—*N* = 3,101. Correlations of approximately $\pm .04$ are statistically significant at the .05 level; correlations of approximately $\pm .05$ are significant at the .01 level. The "positive" end of each variable represents the "higher" or "greater" pole of the dimension (higher position in the supervisory hierarchy, greater degree of bureaucratization, etc.).

Ownership per se is related hardly at all to powerlessness, the correlation being $-.04$. Position in the supervisory hierarchy, though, is more strongly related to powerlessness, this correlation being $-.13$. (In assessing the importance of a correlation of this magnitude, it must be remembered that correlations between social-structural and psychological phenomena are never as great as those among social-structural conditions or those among psychological states. A correlation of $.13$ between a single facet of social structure and any important aspect of psychological orientation is strong enough to be taken seriously, even though it explains only a tiny proportion of the total variance.)

Bureaucratization also is related to powerlessness (the correlation here being $-.09$), but the direction is opposite to what alienation theorists might predict: Employees of highly bureaucratized firms and organizations—where, presumably, the division of labor is generally greatest—are less likely to feel powerless than are employees of nonbureaucratic firms and organizations. This finding casts doubt on the belief that, under modern conditions, division of labor is itself conducive to powerlessness. (On the other hand, the finding is consistent with my earlier analyses of bureaucratization [Kohn 1971], which showed that the occupational conditions attendant on bureaucratization are conducive to intellectual flexibility, openness to new experience, and self-directed values.)

All three of the conditions determinative of the degree of occupational self-direction are related to powerlessness, all of them in the expected direction: close supervision, routinized work, and work of little substantive complexity are all related to feelings of powerlessness. The correlations range from $.08$ for routinization to $-.19$ for substantive complexity.

This pattern of relationships between occupational conditions and feelings of powerlessness is essentially repeated for self-estrangement and normlessness: Ownership per se is of minor importance at most, position in the supervisory hierarchy is of greater importance, division of labor (as inferred from bureaucratization) is negatively related to alienation, and the three conditions that impede the exercise of occupational self-direction are consistently related to feelings of alienation. In each instance, the conditions determinative of occupational self-direction are more strongly related to alienation than are ownership and hierarchical position. (Similar analyses, limited to the profit-making sector of the economy, yield identical conclusions.)

The relationships between occupational conditions and cultural estrangement, however, are altogether different from those between occupational conditions and the other types of alienation. The very conditions that are positively related to powerlessness, self-estrangement, and normlessness—namely, holding a low position in the supervisory hierarchy, working in a nonbureaucratic firm or organization, and not being self-directed in one's

work—are negatively related to cultural estrangement. So this type of alienation is, at least as judged by its occupational concomitants, quite different from the other three;⁸ we shall not consider it further here, its main value for this analysis having been to confirm the view that the various types of alienation must be considered separately. More important for our purposes is that the analysis thus far indicates a pattern of relationships between occupational conditions and the “Durkheimian” type of alienation—normlessness—similar to the pattern for the “Marxian” types of alienation—powerlessness and self-estrangement.

I conclude tentatively that both control over the product of one's labor (i.e., ownership and hierarchical position) and control over the process of one's labor (i.e., closeness of supervision, routinization, and the substantive complexity of the work) are related to feelings of powerlessness, self-estrangement, and normlessness, the latter type of control more strongly so. This, however, is not to say that I have as yet demonstrated that any of these occupational conditions actually results in alienation; there are several other possible interpretations of the findings.

Two logical possibilities are that the relationship between control over the product of one's labor and alienation reflects the greater opportunities of those in high position to be self-directed in their work, or, conversely, that the relationship between occupational self-direction and alienation reflects the disproportionate numbers of owners and of people high in the supervisory structure among those whose work is self-directed. I tested these possibilities by computing multiple-partial correlations⁹ between each set of occupational conditions and each type of alienation, in each instance statistically controlling the other set of occupational conditions. Table 3 shows that, when occupational self-direction is statistically controlled, ownership and hierarchical position lose much of their explanatory power. Occupational self-direction, on the other hand, is related to alienation nearly as strongly when ownership and hierarchical position are statis-

⁸ Analyses of many other dimensions of social structure yield almost entirely consistent confirmation that cultural estrangement bears a different (usually opposite) relationship to social structure from that of the other three types of alienation. (Middleton [1963] reported consonant findings.) A likely explanation of this disparity is that powerlessness, self-estrangement, and normlessness, at least as we have indexed them, all represent a negative judgment of self—in the sense that the individual feels that he lacks personal efficacy, lacks basic worth, or lacks the ability to make his own moral decisions. Cultural estrangement, on the other hand, does not necessarily represent a negative judgment of self, but often means quite the opposite, that the individual is sufficiently secure in his judgments of self to be independent in his values.

⁹ The multiple-partial correlation is a simple extension, to multiple correlations, of the logic of partial correlations (see Blalock 1960, pp. 350–51). It measures the magnitude of the relationship between a set of independent variables and some dependent variable when one or more other variables are statistically controlled.

TABLE 3

MULTIPLE-PARTIAL CORRELATIONS, ALIENATION SCALES, BY OWNERSHIP AND
HIERARCHICAL POSITION, AND BY OCCUPATIONAL SELF-DIRECTION
($N = 3,101$)

Condition	Powerlessness	Self- Estrangement	Normlessness
1. Ownership and hierarchical position:			
Multiple correlation with the specified type of alienation13	.09	.09
Multiple-partial correlation, controlling:			
Occupational self-direction*04	.00	...§
Plus other pertinent occupational conditions†02	.02	...
Plus education03	.02	...
Plus other social characteristics‡04	.04	...
2. Occupational self-direction*:			
Multiple correlation with the specified type of alienation21	.18	.29
Multiple-partial correlation, controlling:			
Ownership and hierarchical position17	.16	.30
Plus other pertinent occupational conditions†12	.14	.25
Plus education08	.10	.15
Plus other social characteristics‡08	.12	.13

* Closeness of supervision, routinization, and substantive complexity.

† Specifically, salary or wages; bureaucratization; pressure of time; and the respondent's assessment of: the risk of his losing his job or falling in his business, the probability of his being held responsible for things outside of his control, and the competitiveness of his occupational situation.

‡ Specifically, age, race, father's education and occupational status, intergenerational occupational mobility, the degree of urbanism of the place where respondent lived longest as a child, the region in which that place was situated, religious background, and national background.

§ When controlled for occupational self-direction, position in the supervisory hierarchy becomes positively related to normlessness, a reversal of direction from the zero-order correlation. This remains true when additional variables are brought into the equation.

tically controlled as when they are not.¹⁰ (Again, similar analyses, limited to the profit-making sector of the economy, yield identical results.) These findings suggest that the pivotal occupational conditions are those determinative of self-direction in one's work. Insofar as ownership and hierarchical position affect alienation, it is mainly because owners and people high in the supervisory structure are able to be self-directed in their work.

Just as ownership and hierarchical position lose much of their explanatory power when the conditions determinative of occupational self-direction are statistically controlled, so might the conditions that make for occupational self-direction lose their explanatory power if *other* occupational conditions were controlled. Six occupational conditions are poten-

¹⁰ Another way of expressing this finding is that when ownership and position in the supervisory hierarchy are added to a multiple-regression equation already containing occupational self-direction, there is essentially no increment to R^2 for any type of alienation. When the conditions determinative of occupational self-direction are added to a multiple-regression equation already containing ownership and hierarchical position, the increment to R^2 is substantial, ranging from .08 for powerlessness to .21 for normlessness.

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tially pertinent:¹¹ salary or wages, bureaucratization, pressure of time, and three types of job uncertainty—the risk of losing one's job or failing in one's business, the probability of being held responsible for things outside one's control, and the competitiveness of one's occupational situation (all of them as appraised by the respondent). Using the same procedure as before, I additionally controlled all these facets of the job. Although the correlations between occupational self-direction and the various types of alienation are reduced, they remain appreciable (table 3).

The next possibility to consider is that the conditions determinative of occupational self-direction might be related to alienation because men who are more self-directed are generally better educated, and education itself might affect alienation. To test this possibility, I added education to the growing package of statistical controls. (This procedure may be overly stringent, for it ignores the possibility that education might affect, say, powerlessness because education influences the substantive complexity of the work; however, it is substantive complexity that actually affects feelings of powerlessness.) The correlations are further reduced (table 3 again). Nevertheless, even using these stringent statistical procedures, which arbitrarily assign all shared variance to the control variables, the correlations between occupational self-direction and alienation are not trivial.

Several other social characteristics—age, race, social class origins, intergenerational occupational mobility, religious and national background, the degree of urbanism of the place where the respondent was raised, and the region in which that place was situated—are related both to the types of job men hold and to alienation. Thus, they might explain why occupational self-direction is related to alienation. However, adding these variables to the package of statistical controls does not appreciably change the picture. The relationships between occupational self-direction and alienation do not simply reflect differences between the social backgrounds of men who hold more self-directed and less self-directed jobs.¹² Moreover,

¹¹ These are the only occupational conditions included in the study that are related both to occupational self-direction and to one or more types of alienation, when education is statistically controlled. They are thus the only occupational conditions treated here that are potentially pertinent to explaining the relationship between occupational self-direction and alienation.

¹² These multiple-regression analyses also permit specification of the relationships between alienation and social-structural conditions other than the occupational conditions on which this paper focuses. Controlling all variables enumerated in the discussion above showed the following to be significantly related to powerlessness: a high risk (as appraised by the respondent) of losing one's job or business, and low salary or wages. Significantly related to self-estrangement are a high risk of losing one's job or business, a high probability of being held responsible for things outside one's control, being older, and being white. Significantly related to normlessness are having a limited education, being young, being black, having predominantly southern

I have now simultaneously controlled virtually every social-structural variable that my own and previous investigators' analyses have found to be pertinent to alienation—and statistically significant, nontrivial relationships still exist between occupational self-direction and the three types of alienation.¹⁸

Two other possible interpretations of the relationships between occupational self-direction and alienation require different methods of analysis, which can be carried out only in part with the data at hand. One interpretation is that alienation results not from occupational conditions as such, but from a discrepancy between workers' needs or values and their actual occupational experiences (see Argyris 1973); a job of considerable substantive complexity, for example, might challenge some people and threaten others, with predictable consequences for their feelings of alienation.

The available data, while pertinent to this interpretation, fall far short of what is needed for a definitive test, because they do not include measures of workers' needs, values, and capacities before they encountered their present occupational conditions. The data at hand concern only their present occupational values, which of course have been influenced by their occupational experiences. It is nevertheless notable that analysis of variance reveals no significant interaction between occupational values and the conditions determinative of occupational self-direction. Substantive complexity, for example, affects alienation regardless of how greatly the worker values intrinsic aspects of his work. This gives a hint that the effects of the occupational conditions in question may be considerable, no matter what the "fit" between job demands and values (but see Lofquist and Dawis 1969). This conclusion implies, of course, not that men's needs and values are unimportant, but that the conditions determinative

or eastern European rather than northern or western European origins, having a "liberal" rather than a "fundamentalist" religious background, and having been upwardly mobile in occupational level, as compared to one's father's occupational level.

¹⁸ This method of statistical analysis does not rule out the possibility that there may be differences, from one segment of the work force to another, in the degree to which the conditions determinative of occupational self-direction bear on one or another type of alienation. Tudor (1972), for example, found a pronounced relationship between the substantive complexity of work and powerlessness among men who have experienced little or no intergenerational occupational mobility and who have moderate to high incomes, but little or no relationship between substantive complexity and powerlessness among other employed men. The data used here do not confirm the existence of this particular pattern, nor do they reveal differences in the relationship of occupational self-direction and alienation between, e.g., employees of bureaucratic and nonbureaucratic firms and organizations, between white-collar and blue-collar workers, or between employees of profit-making firms and employees of government and nonprofit organizations. Nevertheless, there may admittedly be other differences between one segment of the work force and another that my relatively broad-gauge analyses have failed to detect (see also Bonjean and Grimes 1970).

of occupational self-direction affect such basic needs and values as to apply to most workers, at least within the population studied.

The final substantive possibility¹⁴ that can be addressed with these data is that the correlations between occupational self-direction and alienation result, not from the effects of the job on feelings of alienation, but from selective recruitment or from men molding their jobs to match their feelings. Self-estranged men, for example, might be unlikely to seek or to be selected for jobs of great substantive complexity; or if holding such jobs, they might perform at the lowest level of complexity the jobs allow. Here, too, a definitive test requires longitudinal data. By borrowing from the methods of the econometricians, though, it is possible to make a tentative assessment of the degree to which my findings reflect the effects of job conditions on feelings of alienation and the degree to which they reflect the effects of alienation on job recruitment and performance. The particular method I employ is called "two-stage least squares"; it is described in detail in Kohn and Schooler (1973) and the references cited therein. In essence, this method attempts to "purge" each variable of the effects of all others with which it is reciprocally related by estimating from other pertinent data what each individual's score on that variable would have been if the other variables had not had an opportunity to affect it. These estimated scores are then used as independent variables in multiple-regression equations.

Only for substantive complexity do I have the job-history data required for this type of analysis. Fortunately, though, substantive complexity is the most important of the three conditions determinative of occupational self-

¹⁴ Several technical possibilities must also be considered: (1) The findings might be an artifact of some deficiency in our indices, either of occupational conditions or of alienation. But there is evidence that our occupational indices are adequate (see Kohn and Schooler 1973, p. 106). I have discussed most of the issues pertaining to the adequacy of our indices of alienation earlier in this paper. (2) Respondents' tendencies to agree with "agree/disagree"-type questions, regardless of their content, and to answer in extremes (agreeing or disagreeing "strongly") might account for the relationships between occupational conditions and alienation. But statistically controlling "agree set" and an "extreme answer score" has only slight effect; these response tendencies do not explain my findings. (3) I might not have met all the statistical assumptions of multiple-regression analysis. But analysis of variance has shown all the relationships between occupational conditions and alienation to be essentially linear. In extensive tests of possible interactions between education and the various occupational conditions, and among the occupational conditions themselves, I have found no interaction of any appreciable magnitude. Finally, the findings are not an artifact of treating the Guttman Scales as interval scales: I replicated the essential analyses, substituting factor scores—a much closer approximation to interval scales—for the Guttman Scales. (The factor scores are based on the factor analysis discussed earlier in this paper, rotating the factors to simple structure through the varimax procedure, and extracting four orthogonal factors.) Thus, the essential conditions of multiple-regression analysis—linearity and additivity of the independent variables, linearity of the dependent variables—appear to have been met.

direction. There is strong *prima facie* evidence that the substantive complexity of the work has a causal impact on powerlessness, self-estrangement, and normlessness (see table 4). For powerlessness and self-estrangement, the effects appear to be essentially unidirectional: Working at jobs of little substantive complexity leads people to feel powerless and self-estranged, but persons who feel powerless or self-estranged are not especially likely to be recruited into jobs of little substantive complexity. For normlessness, the relationship to job complexity appears to be reciprocal: Working at jobs of little substantive complexity is conducive to normlessness; but it is also true that persons who do not take responsibility for their actions are more likely to be recruited into jobs of little substantive complexity. Although both processes occur, job complexity appears to have a greater impact on normlessness than does normlessness on job complexity.

TABLE 4
ESTIMATED RECIPROCAL EFFECTS OF SUBSTANTIVE COMPLEXITY OF JOB AND ALIENATION

Type of Alienation	Effect of Specified Type of Alienation on Substantive Complexity*	Effect of Substantive Complexity on the Specified Type of Alienation	Exogenous Variables Included in Equation (All Others Nonsignificant)
Powerlessness03 (N.S.)	.25	Education (— .06)†, region (.04), age (.04)
Self-estrangement	— .02 (N.S.)	.17	Race (.09), age (.05)
Normlessness08	.16	Education (.18), religious background (— .11)†, national background (.10), race (.09), age (.07), region of origin (.04)

NOTE.—Figures represent standardized β coefficients from the second stage of two-stage least squares regression equations.

* Substantive complexity of prior jobs, education, father's occupational level, maternal grandfather's occupational level, and religious background are always included in the equations.

† A minus sign signifies a reversal of direction from the zero-order correlation.

In sum, there is substantial evidence that doing work of little substantive complexity is not only associated with, but actually results in, feelings of alienation. It seems a fair presumption that closeness of supervision and routinization also have a causal impact on alienation. In their principal thrust, then, the findings are entirely consonant with the basic Marxian analysis of the alienating effects of workers' loss of control over their essential job conditions. In this large-scale, capitalist economy, the type of control that is most important for alienation, though, is control, not over the product, but over the process, of one's work. Ownership, hierarchical position, and division of labor have less effect on workers' feel-

ings of alienation than do closeness of supervision, routinization, and substantive complexity.

DISCUSSION

The analysis presented above clearly indicates that the conditions of work determinative of occupational self-direction—closeness of supervision, routinization, and substantive complexity—bear meaningfully on three major types of alienation—powerlessness, self-estrangement, and normlessness. The findings provide evidence not only for an interconnection between these conditions of occupational life and men's orientations to nonoccupational social reality, but even for a causal effect. I emphasize this because of the often-stated and widely accepted argument (see particularly Seeman 1967, 1971, and 1972*b*) that there is little carryover from occupational experience to nonoccupational alienation. My results indicate, to the contrary, that there *is* carryover from occupational experience to alienation in nonoccupational realms, and that this carryover is of the logically simplest type (see Breer and Locke 1965)—the lessons of the job are directly generalized to nonoccupational realities. Occupational experiences that limit workers' opportunities to exercise self-direction in their work are conducive to feelings of powerlessness, to self-estrangement, and even to normlessness.

It must be emphasized that these findings, although derived from a large and representative sample, cannot safely be generalized beyond the United States or even beyond the time the data were collected. I do not know whether these occupational conditions would exert the same alienating effects in different cultural contexts or under different economic conditions. This research was limited to the civilian population. I do not know whether, for example, closeness of supervision would have the same alienating effect in the presumably more authoritarian system of the military. Moreover, and more important, I do not know whether the conditions determinative of occupational self-direction would matter as much in other countries and cultures. In addition, the relative importance of the various occupational conditions may be affected by economic conditions: 1964 was a time of relatively great economic security in the United States; occupational self-direction might be less important and, say, job security more important at times of greater economic uncertainty.

Much of the theoretical literature attributes alienation to conditions stemming directly from capitalism or bureaucracy or both. The data used here are far from adequate for dealing with these issues; the research was limited to one country, capitalist and heavily bureaucratized. Yet the data do bear on these themes, and insofar as they do, they consistently imply that neither capitalism nor bureaucracy is the primary source of

alienation in this industrial society. Within the United States, I have found that being or not being an owner is of at most minor importance for feelings of powerlessness, self-estrangement, and normlessness. Moreover, admittedly within capitalist society, people employed in profit-making enterprises are no more alienated than are those who work for government or for non-profit enterprises. And as for bureaucratization, as already noted, its effects are opposite to what would be expected by those who see bureaucratization as a source of alienation. My findings point, instead, to occupational conditions that impinge more directly and immediately on the worker, in particular, to his opportunities to exercise self-direction in his work. Nevertheless, these very conditions are built into the class structure of this capitalist society. It is an open question, and one well worth investigating, whether these occupational conditions have the same effects in different social and economic structures.

Finally, a question of great practical importance, of direct pertinence to the many "experiments" in the restructuring of work currently being conducted in several countries: Would alienation be alleviated by occupational rearrangements, by worker participation in "management" decisions, or by the organization of work teams? My findings imply that those experiments that fail to give the worker meaningful control over the conditions that impinge directly on his opportunities to exercise initiative, thought, and independent judgment in his work will not affect alienation. On the other hand, I should certainly expect experiments that do increase workers' control over the essential conditions of their work to decrease alienation (Kahn 1975). One of the most interesting questions is how large a measure of individual control, as against a share in group decision making, is required for a worker to feel in sufficient command of his essential occupational conditions that he not feel alienated. The answer, of course, may not be the same in all cultures or in all political systems.

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Industrial Violence in Italy, 1878–1903¹

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We use a unique source of data on each of over 6,000 strikes (many of them violent) occurring in Italy from 1878 to 1903 to address two major issues in the study of collective violence: the determinants of violence (the conditions under which conflicts become violent) and its consequences (whether violence promotes successful collective action). We also suggest strategic advantages of individual event data over the aggregate analyses which dominate empirical studies of these issues. Our results indicate that work stoppages are more likely to become violent when they are large in size and long in duration and occur over multiple issues; some of our negative findings on other issue dimensions challenge long-standing assumptions in conflict theory. We then analyze the relationship between violence and strike outcomes and find that, net of other determinants, violent strikes are less likely to end in success for workers.

Disputes between labor and management have been a frequent focal point for analysts of several types of conflict, including bargaining processes (Ashenfelter and Johnson 1969), class conflict (Dahrendorf 1959), collective protest (Shorter and Tilly 1974), and social conflict (Coser 1956). Despite this diversity of substantive focus, empirical studies of industrial conflict have commonly employed aggregate analyses of fluctuations in strike activity, especially in its frequency (Ashenfelter and Johnson 1969; Goetz-Girey 1965; Griffin 1939; Knowles 1952; Rees 1952; Skeels 1971). However, recent work has begun to consider, in addition to frequency, the *form* of strike activity as a useful indicator of the strategy, “meaning,” and institutionalization of conflict. Analyses of the form of industrial conflict have particularly emphasized the “shape of strikes”² (Ross and Hartman 1960; and more rigorously, Shorter and Tilly 1971*b*; Britt and Galle 1972, 1974; Hibbs 1974) and industrial violence (Shorter and Tilly 1971*a*; Taft 1966; Taft and Ross 1969).

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² Shorter and Tilly use this phrase to refer to their decomposition of the “volume” of industrial conflict (total number of man-days lost) into three dimensions: frequency, size, and duration. They make international and intertemporal comparisons with the aid of geometric figures which show relative contributions of each dimension to total volume. See Britt and Galle (1972, 1974) for extensions of this work.

Studies of long-run shifts in the form of strikes characterize 19th- and early 20th-century conflicts as "tests of economic strength" (Hiller 1928; Ross and Hartman 1960). Profiles of these early strikes show them to be infrequent, small in size, and long in duration (Shorter and Tilly 1971*b*). Conversely, "modern" stoppages are frequent, large, and short; these strikes are viewed as brief demonstrations of protest (Hibbs 1974; Ross and Hartman 1960; Shorter and Tilly 1971*b*). Corresponding evidence on changes in industrial violence also indicates a secular decline of the violent strike (Shorter and Tilly 1971*a*; Taft and Ross 1969).

Most observers explain these trends—particularly the decline of violent strikes—in terms of long-run changes in industrial relations systems. Specifically, shifts in the form of industrial conflict are usually attributed to increasingly sophisticated and disciplined labor organizations, their (often grudging) acceptance by employers and national governments, and the institutionalization of collective bargaining (Dahrendorf 1959; Kerr et al. 1960; Ross and Hartman 1960; Taft and Ross 1969).⁸ These changes in form have occurred even in countries (such as France) where routine labor-management relations have not been very well established (Goetz-Girey 1965; Shorter and Tilly 1971*a*).

Although recent evidence on the shape of strikes and industrial violence has been used mostly to illustrate and explain long-run shifts in labor-management conflicts, data on individual violent strikes are also appropriate for analysis of two major issues in the study of violence: (1) the determinants of violence (i.e., the conditions under which conflict becomes violent); and (2) the relationship between violence and its outcome (i.e., whether violent action is an effective protest strategy). In this paper, we utilize evidence from a large number of late 19th- and early 20th-century Italian labor disputes (many of them violent) to address these two issues.

The suitability of individual violent strike data to consideration of these issues is underscored by several methodological and strategic problems in most quantitative studies of collective protest and/or violence. First, these studies (e.g., Feierabend, Feierabend, and Nesvold 1969; Gurr 1968; Snyder and Tilly 1972) use highly aggregated analyses because information on hypothesized determinants is not available at the level of the individual event. Such analyses are subject to various methodological

⁸ At least one influential study (Ross and Hartman 1960) attributes an alleged decline in strike activity itself to long-run institutional changes. However, recent work (Shorter and Tilly 1971*b*; Hibbs 1974, for post-World War II data) indicates that, if anything, industrialized nations have experienced a secular increase in the frequency, size, and volume of industrial conflict. The failure of the standard frequency measure of strike activity to reflect institutionalization of labor-management conflicts generated the interest in other dimensions of industrial conflict (e.g., declines in duration and violence) as indicators of such institutionalization.

and inferential biases (Hannan 1970; Robinson 1950). For example, one widely held argument specifies relative deprivation (the gap between expected and achieved welfare) as the most important determinant of collective violence (Davies 1962; Feierabend et al. 1969; Gurr 1968). Empirical tests of this argument examine the relationship between aggregate measures of deprivation in some (generally national) population and the extent of violence in that population (Feierabend et al. 1969; Gurr 1968). Although it is assumed that estimated results accurately reflect microlevel relationships (e.g., that violent action is undertaken by the most deprived groups), such assumptions are never examined. In this study, information concerning relative deprivation for each work stoppage allows direct assessment of this assumption at the micro (individual event) level.

A second deficiency of empirical analyses of collective violence is their almost complete inattention to the outcome of violence. This neglect is due to conceptual and methodological problems in assessing the relationship between violence and social change (Bienen 1968; Skolnick 1969; however, see Gamson [1975] for recent work on this issue).⁴ These problems include theoretical and ideological disagreements concerning relevant outcomes (partly because of confusion about most protest objectives) and difficulties in measuring consequences of violence. However, even where outcomes are conceptually clear and it is feasible to measure them, inferences concerning the effects of violence may be spurious in that determinants of outcomes other than violence itself are not included as controls.

Strikes avoid many of these difficulties because they have fairly clear spatial, temporal, and conceptual boundaries (unlike such phenomena as "civil strife" and "political instability"). Moreover, strike outcomes (in terms of success, compromise, or failure for workers) are regularly recorded in most countries and have generally been used to evaluate the immediate results of industrial conflict (Forchheimer 1948; Goetz-Girey 1965; Hiller 1928; Knowles 1952; Peterson 1938; Shorter and Tilly 1974). Additionally, previous studies of the results of work stoppages provide a model of strike outcomes (Forchheimer 1948; Griffin 1939; Knowles 1952; Peterson 1938). With information on industrial violence, we can therefore assess whether violent action is successful for workers net of other known determinants of favorable outcomes.

This last point suggests one further advantage of individual strike

⁴ The literature on violence identifies a variety of outcomes or "social changes," which range from large-scale structural transformations (in many treatments of revolution) to psychological consequences for small groups. We consider only a single dimension of change: whether violence results in increased material benefits for the workers involved.

data. We view the preconditions, occurrence, and consequences of collective violence as a temporal process in which the consequences of violence depend on the preconditions as well as the violence itself. Yet analyses of collective violence continue to treat the determinants of violence and the relationship between violence and its results as unrelated issues. In this paper, however, available information on characteristics of strikes before and after the occurrence of violence enables us to bring our empirical treatment of these issues into line with our conceptual view.

Although data on industrial violence are particularly appropriate for analyses of the determinants and outcomes of collective violence, the violent strike has received little systematic attention beyond noting its passing. This neglect is due to the limitations of most published data on labor disputes. The regularly available official strike statistics which facilitated the development of aggregate models also, by not reporting which conflicts were violent, made standard information on industrial violence difficult to obtain. For this reason, investigations of violent strikes rely almost exclusively on case studies, with two exceptions: (1) In their inquiry into "the causes, character and outcome" of labor violence in the United States, Taft and Ross (1969) compile a large number of violent strike case histories and base their conclusions on that sample. However, the disputes which produced detailed accounts were uncharacteristically large and bloody or marked watersheds in industrial relations. Moreover, by considering only violent stoppages, Taft and Ross preclude comparisons of the characteristics of violent *and* nonviolent strikes. (2) Shorter and Tilly's (1971a) study avoids the second hazard. Having searched French newspapers (four random months per year) from 1890 to 1935 for accounts of industrial violence, they attempt to match those accounts to individual strikes listed in the *Statistique des grèves* series (France 1892-1939).⁵ Shorter and Tilly's sample is the best to date, and we rely on their findings for comparisons with our investigation of violent strikes in Italy. However, Shorter and Tilly's sources limit their sample and analyses (though less so than do Taft and Ross's). As they admit, small conflicts, particularly those in which the authorities did not intervene, were unlikely to have been reported. Furthermore, their procedure uncovered only 88 violent strikes in France during the period they consider. They compare characteristics of violent and peaceful stoppages separately but, because of their small sample size, are unable to control for other characteristics of strikes.

In this paper, our investigation of industrial violence in Italy from 1878 to 1903 is based on the discovery of a unique source of information

⁵ This method was used to make certain that the violent event reported in the press was actually a strike and to augment the often incomplete newspaper information with data on the individual strike from the *Statistique des grèves*.

on more than 6,000 labor disputes during this period. These data provide a sample which is not heavily biased in favor of large stoppages and is large enough to permit multivariate analyses. As indicated above, these analyses test arguments concerning the determinants and outcomes of violent action; they are divided into two major parts which correspond to these issues. We stress that our analyses do not consider the relationship between long-run institutional changes and industrial violence. Instead, we focus on the determinants and results of work stoppages solely during a period when they frequently became violent.

INDUSTRIAL VIOLENCE AND ITS OUTCOME

Determinants of Industrial Violence

No single comprehensive theory identifies the conditions under which industrial conflicts become violent. However, partial explanations derive from a variety of theoretical perspectives on social conflict and violence. These include theories of collective violence, explanations of (not necessarily violent) labor militancy and protest, and general propositions concerning social conflict and its regulation.⁶

Current theories of collective violence include relative deprivation (Davies 1962; Feierabend et al. 1969; Gurr 1970) and "mobilization" or "power struggle" (Tilly 1969, 1975; Snyder and Tilly 1972; Oberschall 1973). While deprivation theories emphasize individuals' psychological states as sources of frustration and aggression, mobilization theories focus on the organizational capacity of groups for collective protest and violence. Although these theories are generally employed in a national political context, proponents of each have applied them directly to industrial violence. Davies (1974, pp. 608-9) argues that sudden drops in the objective welfare of workers are most likely to precipitate violent action. Conversely, Shorter and Tilly (1971a) explain French violent strikes partly in terms of the degree of organization and coordination among workers from several industrial establishments.

Explanations of aggregate fluctuations in strike activity as an indicator of labor protest closely parallel these theories of collective violence (Snyder 1975). However, disaggregated analyses suggest other determinants of differentials in industrial militancy. For example, Kerr and Siegel's (1954) well-known argument accounts for interindustry differences in strike propensity in terms of both "the location of the worker in society" (the degree of isolation of industries and the occupational differentiation

⁶ The discussion below is intended to be illustrative rather than exhaustive. Given the lack of an integrated theory of industrial violence, we postpone fuller consideration of current hypotheses to our section on findings.

within them) and "the character of the job and the worker" (the selection process through which physically difficult and unpleasant jobs are more likely to "... draw tough, inconstant, combative and virile workers who will be inclined to strike" [Kerr and Siegel 1954, p. 195]). Our analysis extends this theory of the importance of "industry effects" on militancy to the investigation of differentials in industrial violence.

Disparate propositions concerning social conflict and its regulation also focus explicitly on labor-management struggles (e.g., Coser 1956; Mack and Snyder 1957; Dahrendorf 1959). However, many such propositions in conflict theory rely more on long-standing assumptions than on well-established empirical regularities. For example, Coser (1956, p. 118) and Aubert (1963) argue that "supraindividual" or "value" conflicts (e.g., union recognition) are more likely to be intense and/or violent than "interest" conflicts which involve only the short-run material welfare of the participants (e.g., wage demands). Because our source includes information on issues over which the strike occurred, we can test such propositions and incorporate them in our model of industrial violence.

Despite their varied origins, the major difference among all these hypotheses is their relative emphasis on structural and organizational (e.g., Shorter and Tilly 1971a) versus psychological and symbolic (e.g., Davies 1962, 1974; Coser 1956) determinants of violence. These alternatives reflect competing types of explanations which persist across theories of collective violence, strike activity, and other forms of conflict. While our data on industrial violence do not always permit unambiguous inferences, they provide an infrequent opportunity to test hypotheses derived from these different perspectives at the level of the individual event.

Violence and Strike Results

The lack of empirical work on the effects of violence is accompanied by an absence of coherent theoretical statements on the issue (Bienen 1968). The relationship between violence and social change is not adequately addressed by either of the lines of theorizing on the determinants of violence.⁷ Social psychological theories generally view the outcome of violence merely as a "tension release" mechanism for participants. Organizational theories imply that success of violent action depends on the power and strategies of antagonists, but this suggestion has not been empirically specified or tested. Therefore, whether violence promotes successful collective action is left to speculative arguments shaped by ideological differences and a priori assumptions about social conflict (Nardin 1971).

⁷ Coser's (1956) propositions on the consequences of violence probably remain the most coherent set of statements. However, he does not consider the issue examined here: whether violence is an effective mechanism for a group to gain its objectives.

Within the limited body of empirical work on the effect of violence, there is also substantial disagreement. Taft and Ross (1969, pp. 361-62) conclude that "the effect of labor violence was almost always harmful to the union. There is little evidence that violence succeeded in gaining advantages for strikers." Conversely, Shorter and Tilly (1971a, p. 112) report that 60% of their French violent strikes ended in compromise (versus 28% of all strikes between 1890 and 1914 and 38% from 1915 to 1935). They argue that, given the context of labor relations, violence was a form of (quoting Hobsbawm) "collective bargaining by riot"—in other words, a realistic means of showing serious purpose and forcing at least partially favorable outcomes from employers. Gamson's (1975, pp. 79-82) analysis of this issue in a national political arena also indicates a positive relationship between violence and success. However, all these conclusions are based on (generally bivariate) cross-tabular analyses. Therefore, it is quite plausible that they are spurious to the extent that violence is related to other determinants of success, for example, size and duration.

Given these substantial theoretical and empirical limitations of previous work, our consideration of the effects of violence has modest aims. Since we recognize that conflict processes may be dependent on their form and historical timing (Snyder 1975, p. 275), we do not hypothesize any general relationship between violence and its consequences. Instead, we limit our discussion to industrial violence and present empirical findings on its relationship to strike outcomes which are much less likely to be spurious than those of previous work.

Our assessment of the net effect of violence requires first a model of strike outcomes. Most analyses of successful strikes agree that work stoppages are more likely to end favorably for workers when they are frequent, well organized, large in size, and short in duration (Forchheimer 1948; Griffin 1939; Knowles 1952; Peterson 1938).⁸ These analyses of outcomes are all consistent with a single (if implicit) conceptual model in which various factors increase the probability of success to the extent that they improve the power or bargaining strength of workers relative to employers. This model of strike results, however, is of no direct help in forming expectations concerning the effects of violent action. We hypothesize that the success of violence depends on intervening mechanisms. Specifically, violence by workers may decrease employers' willingness to compromise on any demands and/or spur the intervention of other parties

⁸ Hibbs (1974, p. 6) suggests that, net of long-run institutional determinants, the secular shift in strike shapes (i.e., toward greater frequency, shorter duration, etc.) "... may partly reflect a working class learning process informed by the historical experience of labor-capital struggles." However, we emphasize that available data on strike results permit us to consider only short-run outcomes in this paper.

(most likely agents of the state) on the side of employers, thereby diminishing the probability of success. Alternatively, labor violence may be a convincing threat or a demonstration of workers' resolve. In this case, violence may intimidate employers and/or induce marginally committed workers to participate in the strike, thereby increasing chances of success. Our empirical investigation of these alternatives examines whether workers' violent actions (and, in a supplementary analysis, different targets of violence) promote their objectives *net of* other established determinants of successful strikes.

DATA AND HISTORICAL BACKGROUND

The data pertain to all work stoppages occurring in Italy during the years 1878 (July) through 1903 which were recorded in the *Statistica degli scioperi* series (Italy 1892–1904).⁹ The Italian government's interest in accumulating data on economic life included this compilation of information on labor disputes. Local administrative officials were responsible (in addition to regular duties) for reporting characteristics of work stoppages in their localities. When strikes began, these officials interviewed the parties involved (probably focusing more heavily on employers, given grudging government tolerance of worker organizations through 1900); several interviews took place during lengthy strikes. They recorded characteristics of the strike on a four-page questionnaire which included standard items (e.g., location, issues, outcome) and space for a written account of events. Upon completion, questionnaires were forwarded to the national ministry, which published the data in tabular form. For each stoppage, the *Statistica* lists its location, the name of the establishment in which it occurred, beginning and ending dates, detailed industrial and occupational categories of workers involved, the number of participants, the (stated) issues over which the strike occurred, duration, outcome, *and whether or not* the strike was violent. This last piece of information makes the *Statistica* a unique and valuable source. Although several nations have published characteristics of individual work stoppages, Italy is the only country which ever regularly recorded evidence of industrial violence. Data on violence in labor disputes for the *entire* period covered by the source (1878–1903) is limited to a yes/no dichotomy in the tabular listings.

⁹ Although this series purports to list all work stoppages which involve at least five workers and last at least one-half day, it is almost certainly an undercount. Shorter and Tilly (1974, p. 352) estimate that in some years French strike-reporting procedures (similar to Italy's) missed close to 10% of all disputes. Three additional notes: (1) we do not treat stoppages initiated by employers separately (these lockouts account for about 2% of all stoppages); (2) we coded half-day strikes (fewer than 0.2% of the sample) as lasting a single day; and (3) our source considers "intermittent" strikes as separate work stoppages.

However, beginning with the tabulations for 1894 (and continuing through 1903), the *Statistica* included an appendix with the account of each strike from the original questionnaires. These accounts generally indicate the intensity and targets (e.g., police, strikebreakers, establishment property) of the violence. We utilize this more detailed information in our examination of strike outcomes.

The *Statistica degli scioperi* series ended in 1903, when responsibility for reporting characteristics of strikes shifted to the bulletin of the Ministry of Labor. This later source (Italy 1904–20) reported strike information irregularly and incompletely. The available data therefore limit our investigation to the period from 1878 through 1903. We coded information on all 6,627 work stoppages listed for those years.¹⁰ Over 11% of these disputes were violent.

This large proportion of violent strikes (e.g., compared with one in 60 French strikes in the Shorter-Tilly sample) necessitates some consideration of the historical context of industrial conflict in Italy as a basis for (1) comparing our results with those for other countries (particularly France) and (2) evaluating possible biases in the source itself. The early development of regular strike reporting in Italy is surprising in view of the lack of industrial development and the infrequency and illegality of strikes around 1880. Gerschenkron (1955, p. 76) characterizes Italy's periods of industrial growth as follows: preindustrial before 1881; "modern growth" from 1881 to 1888 (4.6% average growth per year); "stagnation" from 1888 to 1896 (0.3% per year); and "very rapid growth" from 1896 to 1908 (6.7% annual average). Table 1, which reports selected economic and strike indicators, reflects this periodization in the yearly figures for steel output. It also shows that the frequency (number of strikes) and magnitude (number of participants) of industrial conflict rose slowly, though more regularly, until the rapid upsurge which occurred around 1900.

These fluctuations in strike activity were largely a function of changes in the form and scale of labor organization and government actions toward workers and their associations. Trade unions were forbidden until 1890 (although hundreds of locally based mutual aid societies existed). After slow and irregular growth of unions in the 1890s, victory of the liberal Zanardelli-Giolitti government in 1900 brought in the "Golden Age of Italian Labor" (Gualtieri 1946). The laissez-faire labor policy of this regime facilitated the rapid development of "modern" unions which were organized nationally on the basis of industrial categories.

¹⁰ Our analyses are based on 6,232 cases for which there are data on all variables. However, examination of the excluded cases (on which missing data are generally limited to one or two variables) indicates only small differences from those included in the analyses.

TABLE 1

SELECTED ECONOMIC AND STRIKE INDICATORS, BY YEAR (ITALY, 1878-1903)

Year	Steel Output (Thousand Tons)	Index of Real Wages (1900 = 100)	Work Stoppages (N)	Participants (N)	Violent (%)	Falling (%)	Missing Data (%)
1878	0	74.3	22	2,992	22.7	54.5	9.0
1879	0	74.5	32	4,011	18.8	46.9	12.5
1880	0	75.6	29	5,931	6.9	44.8	6.8
1881	3,630	81.0	49	8,776	26.5	44.9	20.4
1882	3,450	86.2	51	9,789	24.5	54.9	9.8
1883	2,965	89.1	77	13,007	23.7	39.0	14.3
1884	4,645	90.8	95	24,277	21.1	41.8	10.5
1885	3,382	93.7	154	43,613	17.3	28.3	18.8
1886	23,760	93.8	114	20,867	17.5	45.5	5.3
1887	73,262	94.0	79	27,762	24.7	43.0	6.3
1888	117,785	92.8	107	30,410	15.2	40.0	14.0
1889	151,889	94.6	133	24,499	10.9	39.7	17.3
1890	107,676	91.3	152	43,462	20.3	39.5	17.8
1891	75,925	91.6	164	43,678	14.6	45.0	6.7
1892	56,543	92.4	129	34,304	8.9	48.0	7.0
1893	71,380	94.5	168	45,246	15.6	38.3	14.1
1894	54,614	94.9	128	33,716	16.3	37.7	5.5
1895	50,314	95.4	139	24,662	10.8	37.2	1.4
1896	65,955	95.9	217	98,402	12.4	38.7	0.9
1897	63,940	99.5	244	104,297	7.4	42.6	2.0
1898	87,467	98.8	310	48,142	11.9	46.6	2.3
1899	108,501	100.5	279	45,852	5.1	43.0	6.4
1900	115,887	100.0	424	104,210	6.4	33.6	2.6
1901	129,229	105.4	1,661	426,462	9.8	25.4	4.6
1902	134,989	103.6	1,053	352,148	11.1	34.9	3.8
1903	187,361	102.0	617	136,092	6.7	42.7	5.5

SOURCES.—Italy (1958), p. 129, for steel output; real wage index calculated from money-wage and price data in Vannutelli (1961), pp. 570-71. All strike measures calculated from tabular listings of individual strikes in Italy (1892-1904).

The new government also accorded *de facto* legitimacy to the strike. Before late 1900, agreements among workers for the purpose of ceasing, hindering, or increasing the costs of work without "reasonable cause" were prohibited (Clough 1964, p. 151). But, despite the illegality of the strike over most of the period covered by our data, government attempts to suppress industrial conflict were sporadic. Heavy repressive activity was concentrated in a few years, generally in direct response to particularly militant and/or frequent labor and political protests.¹¹

Italy's industrial growth and removal of legal restrictions on workers therefore began late in comparison with most Western nations. Although some arguments (e.g., Kerr et al. 1960) view industrial violence as a

¹¹ Such protests include those of the International in the 1870s, agricultural strikes in the Po Valley in the mid-1880s, the activities of the Sicilian *Fasci* in the early 1890s, and widespread urban and rural food riots in 1898. Heavily repressive years were 1878-80, 1886, 1894-95, and 1898 (May)-1899. Our classification of repressive years is based on accounts in secondary sources, especially Candeloro (1950), Gradilone (1959), Horowitz (1963), Neufeld (1961), and Rigola (1947).

function of the pace of industrialization, it would be erroneous to attribute differences in violence (11% for Italy versus 2% in France) and its determinants to differences in industrial development. Despite the late start, by the mid-1890s Italian industrial relations—including the form and scale of labor organization and the frequency and magnitude of strike activity—were very similar to those in France. Additionally, there is no close temporal relationship between “level of development” and the decline of the violent strike elsewhere. Shorter and Tilly (1971a) point to a sharp decline in France around World War I, while the corresponding decrease in industrial violence occurred a quarter century later in the United States (Taft and Ross 1969).

However, the discrepancy between French and Italian violence raises issues of data quality. Although we cannot answer questions of reliability and validity conclusively, table 1 reports evidence which allows consideration of these issues. The percentages of violent and failing strikes are most likely to be sensitive to the political climate. However, there is no apparent relationship between repressive years and either of these variables.¹² Nor is there clear indication that the new legitimacy of the strike in 1900 affected reporting. Although failure rates dropped precipitously,¹³ levels of violence increased in 1900, 1901, and 1902. However, table 1 does indicate a temporal decline in violence (particularly after 1890), which we are unwilling to attribute to “institutionalization” within this period. We therefore suspect a reporting change due to the legalization of trade unions in 1890. The discontinuous increase in the completeness of strike reporting indicated by the missing data figures (as well as evidence from our analyses) also raises questions of validity due to haphazard reporting before the early 1890s. Although this probable deficiency in data quality will bias reported results, we expect any such bias to be small, because (a) well over 80% of the strikes in the sample occurred after 1890, and (b) the evidence discussed above does not indicate other plausible biases—that is, it is not consistent with year-to-year shifts due to political climate or with later reporting changes associated with the new legal status of strikes in 1900.

It is therefore unlikely that the difference between the French and

¹² Descriptions of small-scale violence in the source accounts and the direction of the comparative difference concern us with over- rather than underreporting in repressive years. However, many such years (e.g., 1879–80, 1895, 1899) show decreases in both violence and failures; others (e.g., 1886, 1898) indicate increases in these variables. These results do not suggest regular year-to-year effects of political climate on strike reporting.

¹³ Fluctuations in failure rates after 1900 (and over the entire period) correspond closely to changes in Italian labor and employer organization (Neufeld 1961). This effect of organization is suggested in table 1 by the strong inverse relationship between short-run changes in failure rates and in the level of strike activity itself.

Italian figures reflects the illegality of Italian strikes and the apprehension of officials responsible for forwarding strike information to the national ministry. Instead, we attribute this difference to the following: (1) Almost half of the Italian violent strikes involved fewer than 100 workers. Events of this size were less likely to be reported in the national newspapers on which Shorter and Tilly rely. Even the larger violent strikes were probably underreported to the extent that they occurred far from Paris. (2) Shorter and Tilly include in their violent-strike category only strikes in which violence could clearly be attributed to the *collective* actions of workers. The Italian source makes no such distinction. Its descriptions of stoppages indicate that these strikes meet conventional definitions of violence (i.e., they involved physical damage to persons or property), but were not necessarily "collective" (e.g., sometimes as few as one or two workers broke windows in the establishment or beat a fellow employee who refused to strike). Since the source does not generally indicate how many workers participated in a violent act, our definition of industrial violence is necessarily broader than Shorter and Tilly's. Consequently, our results could differ from theirs to the extent that certain strikes classified as violent by the *Statistica* but not by the Shorter-Tilly criterion differ from those which are included by both definitions. Although we cannot conclusively reject that possibility, two pieces of evidence give us reasonable confidence that the source reports mostly incidents of collective violence: (1) We utilized information from the supplementary accounts which accompanied strike tabulations beginning in 1894.¹⁴ Seventy-six percent of the violent strike accounts report at least five arrests. Another 3% list three or four arrests in stoppages with 20 or fewer participants. Although these findings may reflect a systematic overreaction of Italian police to individual violence, we doubt that alternative because accounts of nonviolent stoppages which report police intervention only rarely mention any arrests. Moreover, the indication in the arrest data that almost 80% of reported violence was collective is a conservative estimate, since our criterion would classify the following as noncollective incidents: (a) several arrests occurred but were not discussed in the local official's account, (b) participants dispersed before arrival of police, or (c) there were strategic arrests of only one or two leaders. (2) Evidence from our analyses does not suggest different findings as a consequence of definitional differences. Where our independent variables parallel Shorter and Tilly's

¹⁴ We coded these accounts for evidence of prior organization of workers and employers, type of intervention by authorities, number of arrests and convictions, and type of violence. We are reluctant to compare violent with peaceful strikes along these dimensions (because of the much greater space allotted to accounts of violent events). However, this additional information is used here only to make inferences about the characteristics of violent strikes.

measures, the findings are similar to theirs, except for one result which is explained in terms of variables not controlled in their analyses.

METHOD AND MODEL

We use a dummy variable linear probability model to investigate differentials in industrial violence and strike outcomes. Since we calculate coefficients for each category of each variable, our procedure is identical with multiple classification analysis (see Andrews, Morgan, and Sonquist [1967]; and especially Sweet [1973], pp. 52–54, for a cogent discussion of the method and its uses). In tables 2–4, the first column shows the unadjusted or crude deviation of the (dichotomous) dependent variable from the total sample mean.¹⁵ The second column presents net (after controlling additively for all other variables in the table) deviations from the grand mean (see Bumpass and Sweet 1972, p. 755). Comparisons of unadjusted and adjusted (i.e., gross and net) effects “indicate the extent to which the other included variables modify the effect on the dependent variable of the variable in question” (Sweet 1973, p. 53).

Although dummy variable analyses are generally employed with cross-sectional data, the strikes in our samples are distributed through time. There are dangers in pooling strikes from the entire period in a single model, especially if important exogenous variables (those which are not characteristics of individual strikes) are time dependent. For example, if “offensive” issues (defined as those in which workers ask for increased benefits) occur more frequently when real wages are rising, then omitting a measure of wages from the model will overstate the true effect of offensive strikes on the dependent variable. The analyses therefore include measures of several “contextual” variables such as rates of wage change, industrial growth, and repressive activity by the state. Each strike is assigned a value on these variables depending on its temporal location in more or less prosperous, repressive, etc., years.¹⁶ However, given the crude

¹⁵ Use of a dichotomous dependent variable violates assumptions of homoscedasticity of disturbances. Standard errors (and significance estimates) of the coefficients would therefore be biased. We follow conventional practice in this type of analysis (e.g., Bumpass and Sweet 1972; Sweet 1973) and do not report significance tests. Particularly given the sizable sample, we focus on category differences large enough to be of substantive interest (Frideres and Taylor 1972). However, for consistency in our interpretation of substantively important category differences, we use a criterion of 3% (0.03) throughout the analyses. We also note that, even with heteroscedasticity, the coefficients (category effects) themselves are unbiased estimates (Ashenfelter 1969, p. 645), although they may still produce out-of-range predicted values.

¹⁶ The equations also include a series of dummy variables representing the year in which the strike occurred. This procedure attributes to “year” the effect of variables which regularly influence the incidence of industrial violence but are not explicitly included in the model. Since the inclusion of such variables is merely a proxy for several unmeasured, time-dependent factors, we do not offer substantive interpretations of their effects.

categorization schemes for many of these variables (where the data do not permit interval scale measurement), we treat their inclusion more as heuristic controls than as tests of the theories which specify them as important.

Although we do not employ formal causal analysis (e.g., Duncan 1966), figure 1 shows the model underlying the analyses. We treat the political

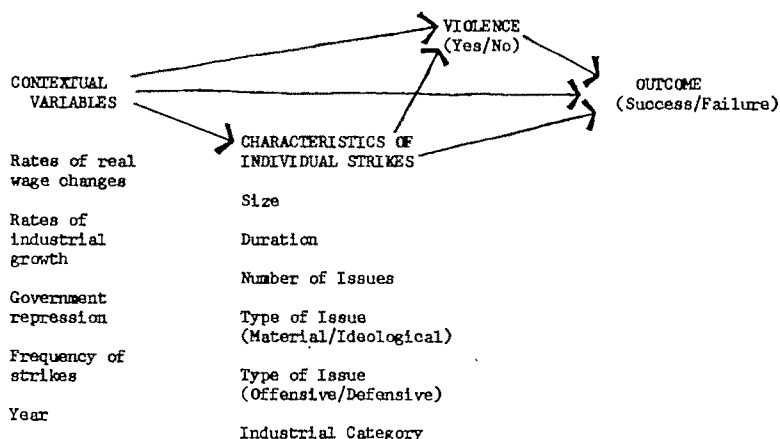


FIG. 1.—Ordering of variables in analyses of industrial violence and strike outcomes. Definition and measurement of variables are discussed in text.

and economic context of strikes occurring in particular years as exogenous variables which influence characteristics of individual strikes, including the dependent variables of interest (violence and outcome). We do not expect large effects of these contextual variables (except for the proxy variable "year") on violence and outcome: first, measurement error due to the crude categorizations may attenuate relationships; second and more important, we hypothesize that temporal shifts in wages, repression, etc., will be mediated through the characteristics of the individual strike at its outset. Such characteristics of strikes include size, industrial category, and the stated issues (from which we construct several dimensions based on hypotheses previously outlined); these variables temporally precede violence when it occurs. Although the temporal precedence of duration is less clear, we can treat duration empirically only as an independent variable.¹⁷ Finally, all of these variables (including violence) occur prior to the outcome of the strike, which was recorded at its termination.

¹⁷ While we recognize that violence probably affected duration, the source does not indicate precisely when violence occurred during the strike. However, the accompanying descriptions indicate that violence usually precipitated official (often police) intervention which quickly ended the stoppage. Therefore, the likely effect of violence was to shorten duration. If this is the case, our analysis of differentials between violent and peaceful stoppages will underestimate any positive effects of duration on violence.

FINDINGS: DETERMINANTS OF INDUSTRIAL VIOLENCE

Table 2 shows unadjusted and adjusted effects of these characteristics of strikes and their temporal context on the probability of industrial violence.

TABLE 2

EFFECTS OF SELECTED CHARACTERISTICS OF STRIKES AND THEIR TEMPORAL CONTEXT
ON PROBABILITY OF VIOLENCE* (ITALY, 1878-1903): UNADJUSTED AND
ADJUSTED DEVIATIONS FROM GRAND MEAN

CHARACTERISTIC	DEVIATION FROM GRAND MEAN		CASES (N) (3)
	Unadjusted (1)	Net of Other Variables in Model†	
		(2)	
Temporal Context			
Rate of industrial growth:			
Slow0327	...	950
Moderate0892	...‡	580
Rapid	-.0175	...	4,702
Rate of real wage change0001	...‡	6,232
Government repression:			
Heavy	-.0230	...	570
Moderate0389	...‡	1,715
Light0417	...	3,947
Year:			
18781115	.1015	20
18791042	.1161	28
1880	-.0360	-.0456	27
18811764	.1883	38
18821399	.1704	46
18831323	.1486	66
18840867	.0924	85
18850437	.0319	125
18860726	.0470	108
18871061	.0996	74
18880638	.0796	92
1889	-.0293	-.0101	110
18900965	.1110	125
18910288	.0682	153
1892	-.0161	-.0006	120
18930505	.0585	144
18940982	.0627	121
1895	-.0006	-.0050	137
18960159	.0418	215
1897	-.0365	.0127	239
18980136	.0211	303
1899	-.0620	-.0327	261
1900	-.0447	-.0256	413
1901	-.0130	-.0358	1,586
19020020	-.0172	1,013
1903	-.0437	-.0397	583
Individual Strikes			
N participants (size):			
5-50	-.0295	-.0230	2,248
51-100	-.0268	-.0213	1,327

TABLE 2 (Continued)

CHARACTERISTIC	DEVIATION FROM GRAND MEAN		CASES (N) (3)
	Unadjusted (1)	Net of Other Variables in Model† (2)	
101-250	-.0106	-.0108	1,342
251-4000280	.0151	583
401-6000764	.0677	275
601-1,0001116	.0927	214
1,001-1,5001269	.1130	96
1,501+2887	.2478	147
Duration (days):			
1	-.0094	-.0076	1,666
2	-.0244	-.0288	907
3-4	-.0201	-.0194	1,059
5-8	-.0120	-.0089	1,212
9-150066	.0062	587
16-250261	.0259	382
26-351003	.0808	155
36-501656	.1647	133
50+1698	.1675	131
Offensive-defensive:			
Offensive0065	.0011	4,463
Defensive	-.0267	-.0064	1,175
Both	-.0095	.0042	594
Type of issue:			
Wages, hours, working conditions ..	.0014	.0013	5,172
Union related	-.0231	-.0028	737
Both0297	-.0139	323
N issues (Complexity):			
1	-.0090	-.0063	4,930
20273	.0176	1,150
3 or 40847	.0707	152
Industry:			
Mining	-.0367	-.0599	463
Mechanical/metallurgical	-.0226	-.0124	422
Clerical and services0049	.0018	321
Construction	-.0067	.0117	1,240
Chemical	-.0449	-.0594	59
Wood§	-.0391	-.0239	136
Paper	-.0593	-.0346	75
Printing	-.0260	-.0134	127
Textiles	-.0433	-.0439	1,254
Leather0387	.0144	152
Clothing	-.0341	-.0306	240
Food0185	.0092	343
Transportation0123	.0087	328
Agriculture0730	.0779	1,072
Grand mean = .1126	6,232

* Proportion violent.

† All variables in the table were included in this model.

‡ Coefficients for these variables could not be calculated because of high collinearity with other contextual variables in the model.

§ Although this category is composed mostly of strikes in craft occupations, it contains 17 stoppages by sawmill operatives.

Contextual Variables

Unadjusted coefficients of the contextual variables are neither large (except for year) nor generally consistent with previous explanations of violence. The rate of industrial growth was coded according to Gerschenkron's (1955) periodization. However, there is no indication that strikes occurring in years of rapid growth were more likely to be violent (see Kerr et al. [1960] and Feierabend et al. [1969] for expectations of findings opposite to these). The wage measure represents the percentage rate of real wage change from the previous year. Although this measure makes the simplistic assumption that workers' current expectations are based only on the previous year's wages, it follows relative deprivation theories (Davies 1962; Gurr 1970) which argue that decreases in objective welfare produce a gap between expectations and achievements, and therefore violent action. The results in table 2, however, do not support that argument; the wage variable has no effect on the probability of violence.¹⁸ On the basis of secondary sources indicated previously we also assigned strikes to categories depending on their occurrence in more or less repressive years. The unadjusted negative relationship between government repression and violence corresponds to other findings (e.g., Snyder and Tilly 1972). However, these results concerning rate of industrial growth, wage changes, and repressive activity are far from conclusive, since the unadjusted coefficients are confounded by correlations with other variables. In fact, these measures are so highly collinear with the year variables that their net effects could not be calculated in the multivariate analysis.

For the series of year dummy variables, deviations from the grand mean offer some support for our argument that the legalization of trade unions in 1890 affected definitions of violence and quality of strike reporting: the deviations after 1890 are smaller in absolute value and not systematically positive. However, since the year coefficients represent the effects of all variables not included in the model, they permit no clear substantive interpretation. The major reason for their inclusion is as a proxy for temporal shifts in various economic, political, and legal factors.

Individual Strike Characteristics

Size.—The number of participants in a strike and the number of workers in the establishment are conceptually distinct, though empirically re-

¹⁸ We also used a more sophisticated empirical representation of achievements relative to expectations, which assumes that expectations are formed by actual wage changes over *several* previous time periods (see Ashenfelter and Johnson [1969] for detailed discussion of this procedure). However, the results of that procedure differ only slightly from findings reported here.

lated, dimensions of size. Considerable cross-national evidence documents a positive relationship between establishment size and militancy, including strike frequency (Cass 1957; Cleland 1955; Revans 1956; Prost 1964). However, Shorter and Tilly (1971a, pp. 111-12) find that both violent and peaceful strikes occur disproportionately in establishments of approximately 50 workers, but that violent strikes have several times as many participants (because they cover more establishments and involve proportionately more strikers per establishment). From these findings, Shorter and Tilly argue that violent strikes imply more (but not highly structured or disciplined) organization and coordination among workers whose collective demonstrations of strength more often turn to violence.

Our results are consistent with these findings of a positive relationship between size and violence. Table 2 shows that the probability of violence increases regularly in categories with larger numbers of participants.¹⁹ Net of all other variables in the model, the level of violence in strikes involving more than 1,500 participants is 25 points above the grand mean. However, since we have information only on the total number of strikers, we cannot assess the separate contributions of the number of struck establishments, their mean size, and the overall rate of participation in the strike.²⁰

Duration.—Shorter and Tilly (1971a, p. 112) report large differences in the median duration of violent and nonviolent stoppages (31 and six days, respectively). Although we also find a positive association between duration and the probability of violence, adjusted deviations are small for all except the three longest duration categories (respectively, 8, 16, and 16 points above the grand mean). There are at least three plausible explanations for the observed relationship. The first is purely probabilistic: within (but not across) categories of the other variables, strikes may become violent because of random precipitating events (see Spilerman 1970).²¹

¹⁹ Our choice of categories for the size and duration variables is largely arbitrary, although it is based on avoiding categories with very small numbers of cases. However, reestimation of the equations with several sets of equal class ranges indicates that reported coefficients and interpretations are not a function of unequal class ranges.

²⁰ Our participants measure is the product of these three components. Although the results indicate a linear relationship between total number of participants and the probability of violence, this may not be the case for each component. For example, U- or Ω -shaped relationships between plant participation rates and violence are both plausible.

²¹ If strikes become violent solely because of random precipitants, we should find that category differences disappear after controlling for duration. As col. 2 in table 2 indicates, this does not happen. Additional analyses of each variable separately (controlling only for duration) indicated small differences between the crude and net deviations. We also tested for plausible interaction effects (e.g., between size and duration, size and type of issue, duration and type of issue). These interactions generally showed small differences from the additive effects, except for the following: (a)

If that is true, longer duration increases the likelihood that such events, and consequently violence, will occur. Second, more intense hostility between parties at the outset of the strike, as well as increasing the probability of violence, may prolong duration by making settlement more difficult. In this second alternative, longer duration has no substantive importance; it merely reflects the original psychological states of the parties involved. Finally, longer, and especially very long, stoppages may have either escalated anger (or other unmeasured psychological variables) of the participants or led to deliberate tactical changes by one or both sides. Although our results cannot discriminate conclusively among these alternatives, the discontinuous increase in violence manifested by disputes in the longest duration categories leads us to discount the second explanation.²²

Issues (offensive-defensive).—The *Statistica degli scioperi* lists the stated issues or demands over which the strike began and indicates whether each involved demands for increases in benefits or efforts to restore decreases instituted by employers. From this information, we constructed three issue dimensions which represent previous hypotheses concerning the determinants of violence: (1) offensive versus defensive demands, (2) type of issue (wages, hours, and/or working conditions vs. union related matters), and (3) number of major issues (complexity).

Offensive stoppages involve demands for increased benefits by workers. Defensive strikes are those which occur over a decrease in workers' welfare. A residual category includes strikes combining both types of demands. This dimension represents recent work (Davies 1962; Feierabend et al. 1969; Gurr 1969, 1970) which explains violence as a function of (inferred) levels of expectations relative to objective economic conditions. Some of that work (e.g., Gurr 1969, p. 570) recognizes that stable objective welfare accompanied by rising expectations (a situation which corresponds to our offensive category) may produce relative deprivation. However, these studies (e.g., Bwy 1968; Davies 1962; Feierabend et al. 1969) state or assume in their analyses that sudden drops in objective welfare (our defensive category) are most likely to increase the expectations-achievements gap and therefore the probability of violence. Our results

stoppages that were both large and very short were a net 10 points less likely to be violent; (b) stoppages that were both large and long were a net 10 points more likely to be violent; (c) there was a moderate (.05) positive effect of the interaction of size and our offensive issue dimension on the probability of violence.

²² Although we cannot assess their relative contributions, we hypothesize that a combination of random precipitants and tactical changes accounts for the observed influence of duration on violence. However, the pattern of category effects shown in table 1 indicates that, if a random process generating precipitating events is important here, it is not necessarily linear in the time intervals chosen.

offer no support for that argument.²³ Table 2 shows very small crude and net differences between the offensive and defensive categories in rates of violence.

Type of issue.—Shorter and Tilly (1971a, pp. 112–13) report no difference in the stated issues of violence and peaceful stoppages (but do find a larger proportion of violent strikes occurring over “complex causes”). On the other hand, Taft and Ross (1969, pp. 271, 360) argue that union-related issues (especially those pertaining to union recognition) were the most important cause of labor violence in the United States. We classify type of issue as (1) wages, hours, and/or working conditions; (2) union related, including recognition, demands for reinstatement of fired workers, and “sympathy” strikes; or (3) a combination of these two categories. Table 2 shows that, when all other variables are controlled, category differences (and therefore deviations from the grand mean) are virtually nil. These results challenge well-established, yet empirically untested, conflict theory which assumes that conflicts over “ideological” issues are more intense than those concerning “material” ones (e.g., Coser 1956, pp. 111–21).²⁴ We do not propose that union-related strikes are purely ideological. However, if the material-ideological distinction is to have any empirical utility, we can reasonably consider that union-related conflicts during this period had a greater ideological component than those over wages and hours. Our negative results require reconsideration of the utility of this dimension in the explanation of industrial violence and of conflict in general.

Number of issues (complexity).—Although Shorter and Tilly find a positive relationship between complex issues and violence, we expected it to disappear after controlling for size. However, table 2 shows that the level of violence increases with number of issues, rising to 7 percentage points above the grand mean for stoppages involving at least three issues. There is no direct substantive analogue to complexity. It may be that the number of issues represents the intensity of grievances prior to or at the beginning of the stoppage. However, we hypothesize that there is no strong association between grievance intensity and number of issues. If this hy-

²³ We neither endorse the logic of this argument nor claim to have represented expectations more adequately than the work cited above. However, we do maintain that defensive strikes meet the criteria which deprivation theorists specify are most likely to provoke violent action.

²⁴ This material-ideological conceptual distinction is often labeled differently. Coser (1956, p. 118) discusses personal versus supraindividual goals; Aubert (1963) distinguishes between “interest” and “value” conflicts. These scholars insist that the material-ideological dimension is important in explaining conflict intensity. However, their illustrations with substantive cases obscure the utility of the conceptual distinction, usually by showing that any situation has substantial material and ideological components.

pothesis is valid, more issues (demands) may diminish the legitimacy or realism of the strikers' position as perceived by management and non-striking workers, thereby intensifying the conflict (Dahrendorf 1959, p. 225).

Industry.—The interindustry differences in rates of violence shown in table 2 are not consistent with Kerr and Siegel's hypotheses concerning industrial militancy. Unadjusted and net deviations for industries which Kerr and Siegel rank high or medium high in militancy (mining, lumber, textiles) are all below the grand mean.²⁵ Conversely, agriculture (ranked low) has the largest positive deviation (almost eight points),²⁶ and food, leather, construction, services, and transportation (which they classify from medium to low) are slightly above the grand mean. Alternatively, Shorter and Tilly (1971a) find some support in their French data for the argument that industries with tightly knit organization and "modern" conflict strategies are less prone to violence (but more likely to manifest high levels of strike activity itself). With some exceptions (e.g., paper, transportation, and perhaps leather), the pattern of results in table 2 conforms to Shorter and Tilly's scheme. However, the small magnitudes of the effects (and of the differences between categories) indicate that "industry" is not an important determinant of labor violence.

FINDINGS: THE OUTCOME OF INDUSTRIAL VIOLENCE

Table 3 shows the effects of selected characteristics of strikes (including violence) and their temporal context on the probability of favorable outcomes for workers. Since our procedure requires a dichotomous dependent variable, we categorize results in terms of "success or compromise" and "failure." Over 64% of our 6,232 strikes ended in success or compromise for workers (we refer to this category as success).²⁷

²⁵ It is true that some of the industries (especially mining) ranked high by Kerr and Siegel are, relative to their proportion of the labor force, particularly prone to both work stoppages and violent strikes. However, our analyses take the stoppage as given and attempt to determine which of its characteristics are associated with violence.

²⁶ Although Kerr and Siegel do note that Italy constitutes an exception to their general characterization of agriculture as low in militancy, they do not offer an explanation.

²⁷ Shorter and Tilly (1974) argue that workers frequently ask for more than they expect and that compromises may justifiably be included with successes. The reverse (i.e., that employers make lower offers, so that compromises are also successes for them) is not valid here, since the outcome is defined relative to workers' demands at the start of the strike. French and Italian strike sources both define success and failure, respectively, as acquiescence to or denial of the entire set of workers' demands; compromises are the residual category.

TABLE 3

EFFECTS OF SELECTED CHARACTERISTICS OF STRIKES AND THEIR TEMPORAL CONTEXT
ON PROBABILITY OF FAVORABLE OUTCOMES FOR WORKERS* (ITALY, 1878-1903):
UNADJUSTED AND ADJUSTED DEVIATIONS FROM GRAND MEAN

CHARACTERISTIC	DEVIATION FROM GRAND MEAN		CASES (<i>N</i>) (3)
	Unadjusted (1)	Net of Other Variables in Model† (2)	
	Temporal Context		
Rate of industrial growth:			
Slow	-.0507	...‡	950
Moderate	-.0588	...	580
Rapid0321	...	4,702
Rate of real wage change0151	...‡	6,232
Government repression:			
Heavy	-.0819	...	570
Moderate	-.0468	...‡	1,715
Light0321	...	3,947
Frequency0057	.0341	6,232
Year:			
1878	-.1938	-.1287	20
1879	-.1438	-.0950	28
1880	-.1252	-.1051	27
1881	-.1438	-.1012	38
1882	-.1438	-.0989	46
1883	-.0225	.0001	66
1884	-.0663	-.0753	85
18850595	.0500	125
1886	-.1053	-.1022	108
1887	-.1032	-.1107	74
1888	-.0351	-.0485	92
1889	-.0377	-.0380	110
1890	-.0405	-.0462	125
1891	-.0813	-.0781	153
1892	-.1138	-.1156	120
1893	-.0087	-.0007	144
1894	-.0188	.0017	121
1895	-.0014	.0303	137
1896	-.0326	-.0141	215
1897	-.0396	-.0192	239
1898	-.0953	-.0744	303
1899	-.0660	-.0394	261
19000269	.0335	413
19011024	.0776	1,586
19020065	.0020	1,013
1903	-.0690	-.0530	583
Individual Strikes			
<i>N</i> participants (size):			
5-50	-.0787	-.0755	2,248
51-1000169	.0130	1,327
101-2500274	.0362	1,342
251-4000601	.0474	583

TABLE 3 (Continued)

CHARACTERISTIC	DEVIATION FROM GRAND MEAN		CASES (N)
	Unadjusted (1)	Net of Other Variables in Model† (2)	
401-6001204	.1073	275
601-1,0001045	.1019	214
1,001-1,5001172	.0891	96
1,500+1119	.1139	147
Duration (days):			
1	-.0358	-.0156	1,666
20050	.0102	907
3-40196	.0131	1,059
5-80259	.0126	1,212
9-150024	-.0104	587
16-250243	.0115	382
26-350471	.0127	155
36-50	-.0793	-.0798	133
50+	-.0326	-.0196	131
Offensive-defensive:			
Offensive0377	.0088	4,463
Defensive	-.0943	-.0286	1,175
Both	-.0961	-.0098	594
Type of issue:			
Wages, hours, working conditions ..	.0245	.0244	5,172
Union related	-.2091	-.1480	737
Both0842	-.0526	323
N issues (Complexity):			
1	-.0377	-.0285	4,930
21394	.1013	1,150
3 or 41726	.1586	152
Industry:			
Mining0026	.0097	463
Mechanical/metallurgical	-.1196	-.0726	422
Clerical and services	-.0483	-.0266	321
Construction0228	.0281	1,240
Chemical0685	.0911	59
Wood§0699	.0823	136
Paper	-.1900	-.1393	75
Printing	-.1158	-.0325	127
Textiles	-.0181	-.0209	1,254
Leather	-.0381	.0013	152
Clothing0013	.0281	240
Food	-.0632	-.0310	343
Transportation0244	.0347	328
Agriculture0787	.0149	1,072
Violence:			
Yes	-.0835	-.1114	702
No0106	.0141	5,530
Grand mean = .6433	6,232

* Proportion ending in success or compromise.

† All variables in the table were included in this model.

‡ Coefficients for these variables could not be calculated because of high collinearity with other contextual variables in the model.

§ Although this category is composed mostly of strikes in craft occupations, it contains 17 stoppages by sawmill operatives.

Contextual Variables

Unadjusted coefficients of the contextual variables are generally consistent with previous findings (although we doubt that the effects of industrial growth reflect anything more than a temporal correlation with fluctuations in labor organization). The rate of real wage change suggests that workers' prosperity improves their bargaining position and chances of success (e.g., Rees [1952], who also indicates that strikes do not regularly affect changes in prosperity). The negative relationship between government repression and success rates is not surprising (Shorter and Tilly 1974). However, as in the analyses of industrial violence, these measures are so highly correlated with the year dummy variables that their net effects could not be calculated.

Since several studies (e.g., Forchheimer 1948; Knowles 1952; Peterson 1938) find a positive relationship between the frequency and success of industrial conflict, we constructed a measure of frequency by calculating the number of strikes occurring in each province ($N = 69$) and year (for ease of interpretation, the figures report changes in success probabilities per 1,000 strikes). However, the unadjusted and net deviations shown in table 3 are not consistent with previous findings of a strong positive relationship between frequency and success. Strikes occurring in province years with 1,000 more strikes (larger than any actual interprovince differences in the data) would have only a net 3% advantage in the probability of success.

Finally, the series of year variables shows no coherent pattern. Although there are some indications of a correspondence between peaks in labor organization and large positive deviations in success (1885 and 1901), some years in which union membership dropped sharply (e.g., 1894 and 1895) also show nonnegative success probabilities.

Individual Strike Characteristics

Size.—As expected from previous work, there is a substantial positive relationship between number of strikers and the probability of success for workers. However, success rates peak (at about +10%) for the category including 401–600 workers and remain at nearly the same level for larger categories. These results indicate that increasing size improves the bargaining or power position of workers relative to employers. If Shorter and Tilly's finding that strikes occur disproportionately in establishments of about 50 workers also holds for Italy, larger size implies prior coordination among workers in several establishments and probable organizational superiority over the employers affected by the stoppage. We attribute the lack of increase in success rates for the largest size categories to the greater

likelihood of official (including police) intervention on the employer's side in very large stoppages (see Rigola 1947). Such intervention apparently offset the organizational advantages of workers in large strikes.

Duration.—Previous studies (Hiller 1928; Ross and Hartman 1960) argue that workers were less able than employers to hold out in the "test of economic strength" which characterized early strikes and that shorter stoppages should therefore have ended more favorably for workers. Although there is some empirical support for that view (Peterson 1938; Knowles 1952), our findings contradict it in two ways. First, table 3 shows no simple relationship between duration and outcome. Although strikes in the two longest duration categories have success rates below the grand mean, single-day stoppages were also slightly (a net 1.6%) more likely to fail. Second, the association between duration and strike results is much weaker than expected from other studies. If early strikes were indeed endurance tests, table 3 gives little indication that conflicts of any given duration substantially favored one side over the other.

Offensive-defensive.—The unadjusted category differences show that offensive strikes are 13 points more successful than defensive stoppages. Although this finding suggests that substantial strategic advantages accrue to the side which takes the initiative, *net* success rates favor offensive disputes only slightly, partly because offensive demands are associated with larger size and multiple issues. Moreover, analyses (not reported here) which omit only the contextual variables show a 5% advantage for offensive strikes, net of other characteristics of the individual stoppage. These results indicate that apparent (unadjusted) advantages of workers' initial actions are due almost solely to their occurrence under favorable economic and political conditions.²⁸ However, we note that the observed offensive-defensive differentials in success levels may well underestimate the advantage of initial action, to the extent that decreases in workers' welfare were not followed by a work stoppage.

Type of issue.—Strikes over material issues such as wages, hours, and working conditions were far more likely to be successful than were union-related disputes. Table 3 shows unadjusted and net differences of 23 and 17 points, respectively, between the two categories. This result corresponds to Gamson's (1975, pp. 40–43) finding that groups with stated demands which are not "radical" (i.e., which "do not attack the legitimacy of present distributions of wealth and power") are more likely to be successful. The large disadvantage of strikes over union-related issues in our

²⁸ All analyses were originally carried out only with characteristics of individual strikes; this is the only interpretation changed at all by including the contextual variables. These results suggest that, to the extent that temporal shifts in economic and political conditions affected industrial violence within this period, they were largely mediated through characteristics of individual strikes.

analysis is due to (1) the fierce determination of Italian employers to maintain unilateral authority in the workplace (Horowitz 1963; Neufeld 1961; Rigola 1947);²⁹ and (2) our classification of compromise with success. Given the nature of union-related demands (recognition, reinstatement of fired workers), it would have been difficult for the parties to view them in other than zero sum terms.

Number of issues (complexity).—We find very large differences in level of success by number of issues. Even controlling for other variables, success rates for strikes over two and at least three major issues are, respectively, 13 and almost 19 percentage points above those of single-issue stoppages. These differences may again be explained in part by our inclusion of compromise on a single issue, especially when that issue was union related. However, smaller but nontrivial differences persist even when we analyze outcome differentials in terms of complete success for workers versus compromise or failure (figures not reported here). Therefore, as in our analysis of differentials in industrial violence, issue complexity had a substantial impact on subsequent characteristics of the stoppage. However, the data cannot indicate whether the number of issues reflects (or generates) different perceptions of the parties involved or some structural component not captured by other variables in the model.

Industry.—We expected that better-organized industries would have higher success rates. That pattern holds to some extent: mining, construction, clothing, transportation (especially railroad), and, in certain periods, agricultural workers were among the best organized categories. However, it is by no means strong (the positive effects for most of these industries are very small) and there are some clear exceptions (e.g., the well-organized printers are a net 3% below the grand mean).

Violence.—The unadjusted success level of violent strikes is over 9 percentage points *below* that of nonviolent disputes (and 8 points below the grand mean). With other variables in the model controlled, the difference in success rates favors peaceful stoppages by more than 12 points. Much of the 3% decrease from the unadjusted to the net difference may be attributed to the strong associations of size and complexity with both violence and outcome (in a reanalysis which included only those violent incidents which the arrest data indicate were collective, the net disadvantage of violence was reduced only from 12 points to 10). These results correspond neither to Shorter and Tilly's findings on French strikes nor to Taft and Ross's conclusions concerning industrial violence in the United States. Although violent strikes in Italy were considerably less likely to

²⁹ These accounts indicate that Italian employers were notorious for their reluctance to recognize workers' attempts to organize or bargain collectively (as were French employers; see LeFranc 1967; Lorwin 1954). This situation persisted longer in France and Italy than in other Western countries.

end in success for workers, they were by no means "almost always harmful to the union" (Taft and Ross 1969, p. 360). Although the less representative samples of those studies obviate clear comparisons, we are unwilling to attribute these apparent international differences in the outcome of industrial violence either to differences in timing or to some national structural variable. There is no compelling evidence of such differences in labor relations contexts in this period (Snyder 1975). Moreover, during the years analyzed here there was very little international variation in the relative frequencies of categories of strike outcomes (Hiller 1928, p. 208).

Type of violence.—From the descriptive accounts which (beginning in 1894) were provided for each stoppage listed in our source, we compiled information on types of violence during the strikes. Table 4 presents a reanalysis of differentials in strike outcomes,³⁰ replacing the violent/non-violent dichotomy with four categories: (1) violence against fellow workers (intraorganizational), almost always by striking workers against those who refused to join the stoppage; (2) violence against police and/or establishment property or personnel (extraorganizational), including attacks on hired strikebreakers who did not previously work in the establishment; (3) both types listed above; most frequently a combination of violence against nonstriking fellow workers and against the police who intervened to protect their freedom to work; and (4) nonviolence.

TABLE 4
EFFECTS OF TYPE OF VIOLENCE ON PROBABILITY OF FAVORABLE OUTCOMES FOR
WORKERS* (ITALY, 1894-1903): UNADJUSTED AND ADJUSTED
DEVIATIONS FROM GRAND MEAN

TYPE OF VIOLENCE	DEVIATION FROM GRAND MEAN		CASES (N) (3)
	Unadjusted (1)	Net of Other Variables in Model† (2)	
Against fellow workers	-.0658	-.1151	246
Against police and/or establishment personnel/property0074	-.0373	40
Both	-.1138	-.1603	35
Nonviolent0044	.0079	4,472
Grand mean = .6593	4,793

* Proportion ending in success or compromise.

† Differences are net of all variables shown in table 3.

³⁰ The 4,793 cases on which this analysis is based include all peaceful stoppages occurring from 1894 to 1903 and all violent strikes during those years for which "type of violence" was clear. We omitted many ambiguous cases from this portion of the analysis. Table 4 does not present results for other variables because they are substantially similar to those reported in table 3.

Table 4 shows that, controlling for all variables included in table 3, nonviolent strikes remain most likely to succeed. However, there are substantial differences in success levels by type of violence. Strikes involving violence against police and/or the establishment (mostly the latter) are less than four points below the grand mean. Relative to success rates for other forms of violence, this result (as well as the positive unadjusted deviation) lends some support to Shorter and Tilly's "bargaining by riot" thesis. However, their finding of greater success for violence is not replicated here.⁸¹ The success level of strikes involving violence only among workers is 11% below the grand mean. Whether this form of violence indicates greater hostility between strikers and nonstrikers, a weakened capacity for collective action, or both, it clearly worsened the bargaining position of workers.

These characteristics of intraorganizational violence account in part for the lowest success level being that of the category including violence among workers *and* against police or (less frequently) the establishment. The net success rate for this combined category is 16 percentage points below the grand mean and 5 below that of conflicts involving violence only among workers. Evidence from the supplementary accounts suggests that attacks on police further reduced the probability of success by spurring arrests which broke the strike. As we have indicated, virtually all accounts describing violence against police report at least several arrests, while arrests are rarely mentioned in those reporting police intervention but not violence by workers. It is of course possible that violence by police against workers may have occurred frequently (although the accounts never report it). Therefore, what our source implies is that violence reported as *originated* by workers against police may instead reflect *responses* of workers to the differential exercise of official force. More generally, this possibility indicates that systematic information on the actions of authorities and employers would be quite valuable.

SUMMARY AND CONCLUSIONS

These analyses of industrial violence in Italy from 1878 to 1903 indicate that work stoppages were likely to become violent when they were large

⁸¹ We suspected that Shorter and Tilly's finding that violent strikes were more likely to end favorably for workers (versus our unadjusted difference of $-.08$) was masking the effect of size. Therefore, we selected all violent strikes involving more than 400 workers; their crude success rate was 7% above the grand mean for all stoppages. While we do not claim that all Shorter and Tilly's violent strikes involved so many workers, this result is consistent with our hypothesis. Although this finding implies an interaction between size and violence, its inclusion in the model indicates only a small (3-4-point) net positive advantage of large, violent strikes on success rates. Tests for other interactions indicate an effect of comparable sign and magnitude for size and offensive-defensive, and no importance of size-duration and duration-issue dimension interactions.

in size and long in duration and occurred over multiple issues. We also find interindustry variation which offers no support for the Kerr-Siegel interindustry militancy theory but which suggests greater violence rates in industries lacking tightly knit organization and discipline. Inclusion of variables representing the economic and political setting in which strikes occurred indicates that such variables have some net impact on industrial violence. However, these contextual variables do little to alter the (more substantial) effects on industrial violence of characteristics of individual strikes. These results suggest that, prior to the regulation of industrial violence, parameters affecting the probability of violence were largely insensitive to temporal shifts in political and economic conditions.

Although the results do not always discriminate unambiguously among plausible alternative arguments, they indicate greater importance of organizational than psychological and symbolic determinants of violence. Specifically, our findings are negative concerning the influence of material-ideological and offensive-defensive issue dimensions on industrial violence. These findings challenge widely held arguments that ideological conflicts (Coser 1956; Aubert 1963) and relative deprivation (Davies 1962; Gurr 1970) induce psychological states which are more likely to precipitate violent action. Additionally, the differences between our results and those of aggregate studies of similar relationships (Feierabend et al. 1969; Gurr 1968) indicate methodological and strategic advantages of disaggregated analyses of individual event data (Gamson's analysis of groups offers some similar advantages).

Differentials in strike outcomes are generally consistent with a model which explains workers' success in terms of factors which improve their bargaining position relative to that of employers. Strikes were more likely to end favorably for workers when they were large in size, involved offensive demands which were not union related, and occurred over multiple issues. However, we find no substantial influence of frequency or duration on outcome. Net of these variables (including industry and proxies for temporal context), there are sizable differences in success rates by type of violence. Violent strikes, especially those which involved violence among workers and against police, were much more likely to fail than peaceful stoppages. These results raise the possibility that previous findings of the success of violence in industrial conflict (Shorter and Tilly 1971a), and perhaps in other forms of protest (Gamson 1975), are due to the lack of statistical controls for other determinants of success.³² We recognize the

³² However, our multivariate reanalyses of Gamson's (1975, pp. 176-89) published data support his finding that violence promotes success. This result (opposite to ours) suggests that the form of collective action may determine the effectiveness of violence. In particular, the easy identifiability and physical proximity over long periods (e.g., in picketing activity) of workers in industrial protests must have made

possibility of longer-run "positive functions" of violence (Coser 1956), perhaps as a spur to subsequent organization or political inclusion. However, industrial violence in Italy was not an effective means for workers to achieve the stated objectives of their strikes.

Finally, the results show that several characteristics of violent strikes (e.g., size, issue complexity) are also determinants of success which are at least as important in their effects as is violence itself. Therefore, these results indicate strongly that the outcome of violent action cannot be considered in isolation from the preconditions of violence. Only when theoretical and empirical models address both issues will the relationship between violence and its consequences become more tractable.

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Defense Spending and Defense Voting in the House: An Empirical Study of an Aspect of the Military-Industrial Complex Thesis¹

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In an attempt to investigate systematically one aspect of the military-industrial complex thesis, several measures of the impact of defense spending on congressional districts were developed and correlated with two Guttman scales of House voting on defense and foreign policy issues. The party and region of the representatives were controlled. While for the House as a whole there was little support for the hypothesis that representatives from districts highly dependent on defense spending were more likely than representatives from nondependent districts to vote for jingoistic foreign and defense policies, certain subgroups such as the very senior members did evidence such correlation between defense spending concentrations and voting. These results are discussed in terms of pluralist, elitist, and other theories of societal governance.

For the past 15 years, one of the major controversies in social science has concerned the question of how national policy is determined and who determines it. Some have argued that a relatively small group of people sometimes called the "military-industrial complex" determines policy across a broad spectrum of issues and areas. Writers like Mills (1956) and Cook (1962) have contended that *one* result of this elite domination of societal policymaking has been overly large defense budgets and overly aggressive or jingoistic foreign policies.

Some theorists in this area have felt that many large and powerful corporations are heavily involved in the military-industrial complex and benefit economically from an oversized military establishment and the supernationalistic policies which justify it. They are said to use their political power to insure continued high levels of defense spending well beyond need. The attitudes which encourage this kind of spending are felt also to encourage belligerent policies.

¹ I wish to thank Mayer N. Zald, Richard A. Peterson, John McCarthy, and Nancy Hendrix Cobb for their helpful comments on this and earlier drafts. This paper is a revised version of one read at the 1972 meeting of the Southern Sociological Society. I also wish to thank Ned Becker for his help in the revision of this paper for publication.

This study focuses on a single aspect of the many relationships which could be derived from the military-industrial complex hypothesis. The basic issue of the paper is the nature of the relationship between concentrations of defense spending and voting on certain issues in the House of Representatives. The important question asked is whether Defense Department spending policies which result in concentrations of defense spending by geographic regions affect congressional voting measurably, especially in the area of defense and foreign policy. The central hypothesis is that a member of the United States House of Representatives who represents a district economically dependent upon defense spending will be more likely to vote for aggressive, belligerent foreign policies and for measures which serve to retain high levels of defense spending than will his colleagues who represent districts not dependent upon defense expenditures.²

This particular question and this particular hypothesis are derived from the more inclusive notions about the military-industrial complex. The justification for this study is based on the essential role Congress plays in the creation and maintenance of the "complex" as described by most theorists. While this may appear contradictory to the Mills notion that Congress is relatively unimportant and operates at the "middle levels of power," it should be remembered that the essential component of a military-industrial complex is a high level of defense spending, which can be authorized and appropriated only by the Congress.³ The fact that for 20 years Congress has not used effectively its power over the purse (and the fact that presidents have been allowed to appropriate the congressional power to declare war through executive police and emergency actions and have effectively circumvented the power to raise armies through peacetime use of the draft) does not negate the powers of Congress in this area given by the Constitution. These facts simply make it more important to understand why "Congress's success in cutting the military budgets presented to it is unimpressive even when compared with the experience of Tudor parliaments and Henry VIII" (Russett 1970, p. 26). In short, even if Mills is correct in his assertion of the powerlessness of Congress in this area, the method by which a "power elite" circumvents congressional

² The existence of the roll-call vote expedites study of decision making in the House. The relatively small and homogeneous districts of House members make it less likely than in the case of senators, for example, that the importance of large bases or defense plants can be hidden or counterbalanced by other economic entities. The two-year term of office can be expected on the average to tie the representative somewhat more closely to his district than a Senator is tied to his state. Senior members of the House are perhaps the best examples of exceptions to this rule.

³ This is, of course, not the only element of a military-industrial complex. A high level of defense spending alone is not conclusive evidence of the existence of such an entity. It is perhaps a necessary though not sufficient condition.

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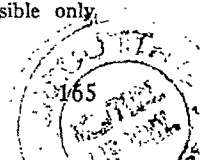
power and paralyses that body remains to be investigated and is a topic warranting study.

It is not my purpose to analyze completely either the tools and weapons of the executive and legislative branches of government, respectively, in the area of foreign and defense policymaking or all of the possible methods by which the powers of Congress might be short-circuited. Instead, I hope to establish the truth or falsity of one hypothesis about the relationship between a military-industrial complex and Congress through an empirical examination of the relationship between defense spending concentrations and defense-related voting.⁴ This particular hypothesis has been selected because (1) congressmen are usually quite responsive to the demands of the organized and major segments of their constituencies (Turner 1951); and (2) any group of business and military leaders whose prosperity requires high levels of defense spending must go to Congress for the necessary authorizations, appropriations, and related votes.

There has been only one previous study of the relationship between defense spending and defense-related voting for the House of Representatives (Cobb 1969). When findings in an area rest on a single study or a small group of studies, additional work is appropriate for reasons of reliability. The earlier study found no relationship between defense spending and congressional voting in the area of foreign policy. This finding contradicts expectations based on the military-industrial complex hypothesis. The present study seeks to confirm or deny the earlier findings.

In addition, it seeks to go beyond the earlier study by means of several important differences in data and methods. In the first place, more complete and more refined measures of defense spending concentrations are now available. Instead of combining military prime contracts and civil ones as the basis for a measure of defense spending concentrations, this study separates them. And instead of a single statewide figure on jobs generated by defense spending for all congressmen from a particular state, it uses measures of military and civilian defense payrolls as a percentage of total income computed separately for each congressional district. More

⁴Horowitz (1969) has commented that it is impossible to study a broad-gauged entity like the military-industrial complex in a tight, delimited study such as this article. This rejection of limited ends is certainly in the Mills tradition, even if somewhat impractical. In the same vein, Gitlin (1969) quotes Mills as saying, "I can take a small portion of this very large topic and try to prove something about it in detail; or I can take the whole topic and try to be merely provocative. Naturally I choose the second course." Provocative he was, and broad-gauged attacks on problems are essential. However, it might be argued that formulations like those of Mills are only the beginning of the process rather than the final product. If real progress is to be made, "grand theories" like that of Mills must be subdivided into middle-range problems and put to the test. His, and other, hypotheses must be tested, refined, and modified. Systematic empirical investigations are in order. They are possible only when some subdivision is made in order to create manageable problems.



refined measures should permit more accurate determination of the presence or absence of the hypothesized relationship.

Second, the present study utilizes a better measurement of defense-related voting. The earlier study used a six-item Guttman scale of foreign policy votes; this one measures voting through the use of two Guttman scales: a five-item foreign policy scale much like that used in the earlier study and an 18-item defense-related scale including votes on foreign policy, defense authorization and appropriations, and other relevant matters.

Third, patterns found in any particular cross-sectional study are open to the charge that they are peculiar to the year studied and not generalizable to other years. This study uses data from a somewhat later period in order to help determine whether earlier results were idiosyncratic.

Fourth, the present study contains a more complete analysis of the problem, including examination of the relationship between defense spending concentrations and defense voting for various subgroups within the House. An attempt is made to search for patterns within subgroups which conform to the structure of power bases and relations within that body.

In addition to my earlier study of the House, there have been a small number of studies of the relationship of defense spending and congressional voting in the Senate (Gray and Gregory 1968; Russett 1970; Cobb 1973). The existence of these studies, which parallel those of the House, makes this study more important, since the findings of the former group conflict with each other. Gray and Gregory and Russett argue that their findings of some small associations between spending and voting support the notion that defense spending is being used to support supernationalistic voting in a manner predicted by the military-industrial complex hypothesis. Cobb found almost no evidence to support the idea of a relationship between defense spending and senatorial voting.

THE J-SCALES

Two Guttman scales were developed for use as dependent variables in this study. The first, or short, scale (see table 1) involves issues of foreign and domestic policy related to the need and the ability to maintain a supernationalistic posture in the world. One bill extends the draft, another tries to punish nations which trade with North Vietnam, another is an attempt to slap down a friendly and conciliatory gesture toward the Organization of American States, and two others attempt to punish nations which have broken diplomatic ties with this country. No defense spending bills are included. It is felt that the policies represented in the scale are

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TABLE 1

THE SHORT J-SCALE
1967, 90th CONGRESS, 1st SESSION
(REPRODUCIBILITY .98)

Congressional Quarterly Almanac Item	Issue*	Difficulty† (%)
1. 79	Selective Service Act of 1967—bill extending the statute and prohibiting a lottery	8
2. 188	Bill prohibiting importation of cotton from countries having severed diplomatic relations with the United States during the past year	18
3. 187	Motion to kill a bill which removed cotton quotas from Egypt and the Sudan	29
4. 201	Motion to recommit with instructions to report out a bill withdrawing presidential power to authorize trade in defense goods with nations trading with North Vietnam and withholding "most favored nation" status from Poland	50
5. 30	Derwinski motion to recommit a bill which would have offered encouragement to the OAS and would have offered to try to help implement results of a forthcoming conference	58

NOTE.—The coefficients of reproducibility in the two scales used in this study are higher than the usual cutoff of .90. It should be noted that such high coefficients are the rule rather than the exception in studies of the relationship of defense spending concentrations and congressional voting on defense and foreign policy issues. The Gray and Gregory study utilized five Senate voting scales, all with coefficients of reproducibility of .95 or better. Russett created seven separate scales in two time periods, with coefficients ranging between .92 and .96. Even recomputing them counting absences as one-half errors, only two of the scales were below .90 (.88 in each case). In my own studies of the House (1969) and the Senate (1973), there were four scales, with coefficients of reproducibility ranging from .94 to .97. The size of these coefficients in this and other studies is increased by the standard procedures of eliminating nonscale items and nonscale individuals including those who were absent so often that no scale score could be determined. Finally, I have used all indications of preference such as pairing as equivalent to voting.

* On scale items 1, 2, 4, and 5, a negative vote was felt to be the nonjingoistic or restrained response. On item 3, an affirmative vote was felt to be nonjingoistic.

† Difficulty is defined as the percentage ($N = 400$) voting or in other ways indicating a choice in favor of the nonjingoistic response.

symbolic of the jingoistic complex of attitudes and beliefs with which high levels of defense spending are often justified.⁵

The second, or long, scale (see table 2) contains 18 items, including those used in the short scale described in table 1. This scale is interpreted as tapping the same underlying dimension of supernationalism or jingoism measured by the first scale. In addition to foreign policy measures, there are provisions to punish Americans who aid the Viet Cong or interfere with domestic troop movements, to audit the Inter-American Bank, to cut the appropriations for the Peace Corps, and to punish officials who help young people avoid the draft through Peace Corps service. Finally, there

⁵ Those who benefit by high levels of defense spending should not be expected to be universally cynical about their objectives and justifications. Many should be expected to believe Cold War doctrines, even if the genesis of such beliefs may be questioned.

TABLE 2
THE LONG J-SCALE
1967-68, 90th CONGRESS
(REPRODUCIBILITY .97)

<i>Congressional Quarterly Almanac</i> Item	Issue*	Difficulty† (%)
1. 67-18	Supplemental Defense Appropriations Authorization—adoption of a conference report authorizing a fiscal 1967 supplement for Vietnam	3
2. 68-130	Fiscal 1969 Military Procurement Authorization—bill to authorize procurement of aircraft, missiles, vessels, and tracked vehicles as well as research and development funds	4
3. 67-17	Brown move to recommit fiscal 1967 supplemental defense department appropriations bill with instructions to add a sense of Congress amendment to the effect that no funds were to be used to carry out war in or over North Vietnam	4
4. 68-74	Amendment prohibiting the Commodity Credit Corporation from financing or exporting agricultural commodities under Food for Freedom if the nation has traded with North Vietnam	5
5. 67-79	Selective Service Act of 1967—bill extending the statute and prohibiting a lottery	8
6. 67-152	Military Construction Authorization—bill authorizing fiscal 1968 military base and military family housing construction	8
7. 68-162	Motion to recommit fiscal 1969 military construction appropriations measure	9
8. 67-211	Aid to Vietcong—motion to adopt rule for consideration of a bill to establish penalties for delivering goods to the Viet Cong and for obstructing troop movements in the United States	12
9. 67-188	Bill prohibiting importation of cotton from countries having severed diplomatic relations with the United States during the past year	18
10. 67-187	Motion to kill a bill which removed cotton quotas from Egypt and the Sudan	29
11. 67-135	Inter-American Bank—Selden resolution to demand an audit of the bank	31
12. 68-23	Arms Control and Disarmament Agency—Derwinski move to reduce level of funding authorization and reduce statutory life span of agency	42
13. 67-201	Motion to recommit a bill with instructions to report out a bill withdrawing presidential power to authorize trade in defense goods with nations trading with North Vietnam and withholding "most favored nation" status from Poland	50
14. 68-104	Peace Corps Authorization bill—Gross move to cut Peace Corps appropriations level	52
15. 67-30	Derwinski motion to recommit a bill which would have offered encouragement to the OAS and would have offered to try to help implement results of a forthcoming conference	58

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TABLE 2 (Continued)

Congressional Quarterly Almanac Item	Issue*	Difficulty† (%)
16. 67-218	Peace Corps Authorization bill—Gross move to cut funds and forbid use of funds to pay any official who attempts to secure a draft deferment for any member because of Peace Corps service	58
17. 68-24	Passage of Arms Control and Disarmament Agency extension and funding bill	76
18. 67-219	Passage of fiscal 1968 Peace Corps Appropriations measure	83

NOTE.—If items 1-6 are eliminated, reproducibility remains at .96. In scales such as these, there is always the possibility that the coefficients of reproducibility have been inflated by the inclusion of too many votes that are too nearly unanimous or too many votes with nearly the same percentage voting each way. While the scale in table 1 contains only one vote which is at an extreme of unanimity and maintains considerable separation between the items, table 2 contains many extreme items on one end and several items which are fairly close together. The need for a large number of items in at least one of the scales prompted the inclusion of the items on the extreme. Table 1 with its short scale serves as a check on the scale in table 2. It should also be noted that recomputation of the scale in table 2 omitting items 1-6 (the more extreme items) produces only a minimal drop to .96. Finally, the suspension of the rule against inclusion of any near-unanimous votes is proper with a large scale.

* Negative votes (or preferences) were considered nonjingoistic on items 1, 2, 4-6, 8, 9, 11-16. Positive votes were considered nonjingoistic on items 3, 7, 10, 17, and 18.

† Difficulty is defined as the percentage ($N = 395$) preferring the nonjingoistic choice.

are actual defense spending authorization and appropriations measures which scaled together with the other defense-related issues. This fact is interpreted as support for the contention that foreign and domestic policy measures such as those utilized in the short scale are related to defense spending measures. All these types of issues are interpreted as tapping the same underlying dimension, although actual appropriations measures tap it at one extreme since, for reasons discussed below, few representatives were willing to vote against such spending measures.

An advantage of these scales is that both Republicans and Democrats could be scaled together. This is interpreted to mean that members of both parties viewed the issues as being on the same underlying continuum and viewed them as being in a single order along that continuum or dimension.

MEASURING DEFENSE SPENDING

It was possible to construct four different measures of defense dependency. The first is a measure of what I term "Defense Involvement (Military)" or DIM. This index measures the extent to which a district is involved in the defense effort through having military prime contracts funneled through it either to be completed in the district or to be in turn subcontracted to plants and installations in other districts. Data on the allocation of military prime contracts were obtained.⁶ Since the industrial activity

⁶ The statistics on military and civilian prime contracts for each county in the nation were supplied to me in 1968 in the form of a copy of the computer printout used by

stimulated by this money put into a district is most meaningfully thought of as a percentage of the total industrial activity, DIM is defined as the total of military prime contracts awarded to a district as a percentage of the value added by manufacture, a measure of industrial activity:

$$\text{DIM} = \frac{\text{military prime contracts}}{\text{value added by manufacture}}.^7$$

One of the real defects involved in using prime contract data is that approximately 50% of each prime contract on the average is subcontracted to other firms or subsidiaries (Bolton 1966, p. 17). The subcontractors may be—but often are not—in the same congressional district as the firm which received the initial prime contract award. This means that an index based on prime contracts is not the ideal indicator of where defense funds are actually spent—where they finally become wages and profits. The announcement of large contracts by political leaders in the district does tend to compensate for this difficulty by insuring that the congressman and his district feel dependent upon these funds even if some of the moneys allocated are subcontracted out of the district.

The second measure of defense spending is called "Defense Involvement (Civilian)" or DIC. While the measure used in the earlier study of the House combined civilian and military prime contracts (Cobb 1969), they have been separated here. The reason for the separate compilation of these indicators is to allow determination of whether some part of the prime contract total may be less visible and may have less impact on a district. It might be hypothesized that civil functions contracts would be less visible and identifiably defense related than military prime contracts. If so, including them in the total contract figure would be likely to reduce the correlation between defense spending concentrations and the vote of the area's representative in Congress on defense-related issues. The DIC is defined as the total value of civil functions prime contracts as a percentage of the total industrial activity, again measured by "value added by manufacture":

$$\text{DIC} = \frac{\text{civil functions prime contracts}}{\text{value added by manufacture}}.$$

The third measure of defense spending, called "Defense Dependency (Military)" or DDM, is concerned with the size of the military payroll

the Pentagon. This information and the figures on military and civilian payrolls by county were provided through the courtesy of Mr. Samuel Zark of the Department of Defense.

⁷ As noted in the text, the numerator of this and all other indices is based on unpublished figures supplied by the Pentagon on a county-by-county basis; the source of the denominator is the 1967 *Census of Manufacturers*.

in a district as a percentage of the total income of that district. Like DIM and DIC, this index is based on figures supplied by the Pentagon on a county-by-county basis. Aggregation of these figures into totals for congressional districts was a relatively simple matter of addition for whole county districts, those comprising one or more entire counties. For split-county districts, I apportioned funds allocated to a county on the basis of the ratio of the congressional district's population to the population of the county in question. Data based on smaller units such as townships were not available. This was something of a handicap in the allocation of funds in the split-county districts. For this reason, and because urban, populous districts are heavily interdependent economically, it must be expected that the analysis will be less reliable in these areas:⁸

$$\text{DDM} = \frac{\text{total military payroll}}{\text{total income}}.$$

There is another source of error in the denominator. The total income of the district is computed from census data on the income of counties. Because the 1960 census had to be used, the figures were somewhat dated by 1967.

The final measure of defense spending is called "Defense Dependency (Civilian)" or DDC. Here we are concerned with the civilian payrolls of the Department of Defense. The denominator is the same as was used in the computation of DDM. Therefore, the same problems might be expected to arise in connection with the use of this indicator. The advantage of separating the two kinds of payrolls again hinges on the possibility that military payrolls are more apparent to observers in a district. The merchant dependent upon the patronage of uniformed military personnel is perhaps more aware of the source of his prosperity than the one who deals with civilian employees of a government arsenal or base who look just like his other customers:

$$\text{DDC} = \frac{\text{total civilian payroll}}{\text{total income}}.$$

An advantage of DDM and DDC is that there is no problem of sub-contracting. The payrolls allocated to a district can be presumed to have been paid out in that district. This is not to say that all payrolls are spent within the district where they are disbursed, although this is quite likely in the larger, single, or multiple county districts.

⁸ The lack of township data is a severe handicap in studying New England, where counties are larger and less useful than in other sections of the country.

FINDINGS

Two independent variables were coded for each congressman: his party and the region he represents. Past research (Turner 1951) has suggested that both were quite important in the prediction of voting in the House. Turner's study and the others he cited indicated that a representative's party is the most important determinant of his vote. My earlier study (Cobb 1969) confirmed this idea. The data reported in the tables of this study, however, show that, although party is very important, region is more important in the prediction of the defense-related votes in question.

TABLE 3
PERCENTAGE OF VARIANCE ON *J*-SCALES EXPLAINED BY
INDEPENDENT VARIABLES IN A GIVEN ORDER

INDEPENDENT VARIABLE	PERCENTAGE OF VARIANCE		SIGN
	Short	Long	
Party affiliation156	.202	—
Region represented242	.289	...
DIT*007	.008	—
DDT†000	.003	—

* Defense Involvement (Total). This is the sum of the civilian and military prime contracts as a percentage of the total value added by manufacture. The overall low levels of zero-order correlations indicated that separate computation of DIAM and DIC would not be fruitful.

† Defense Dependency (Total). This is the total military and civilian payroll in a district as a percentage of the total income of that district.

Table 3 shows the percentage of variance in the *J*-scales explained by these independent variables. When entered in the order shown, region makes the largest contribution to total explained variance. Party also contributes a substantial amount. The theory discussed earlier would suggest that even after the variance explained by these two theoretically fundamental independent variables was removed, defense spending measures should still add a significant amount to the total explained variance. As table 3 indicates, this has not been found to be the case with these data. When a measure of defense spending (DIT = military contracts + civil functions contracts/value added by manufacture) was added, there was no significant increase in the percentage of variance explained in either the short or the long scale of defense-related voting. Therefore, for the House of Representatives as a whole, there is no support in this table for the idea that the dependence of a congressional district on defense spending has a relationship to the manner in which its congressman votes on these issues.⁹

⁹ It should be noted that this finding is consistent with the earlier studies. Russett found no correlations between Senate voting and defense-related expenditures in either of the periods he studied when the defense spending measure was based on

Defense Spending and Defense Voting

Using another measure of defense spending (DDT) based on payrolls confirms this finding of a lack of explanatory power of defense spending. Once the variance attributable to party and region is removed, the defense spending index based on military and civilian payrolls in the district adds no explained variance of significance to the total. Therefore, since defense spending does not explain a significant portion of the variance for the House as a whole in 1967-68, table 3 contains no evidence to support the hypothesis that a congressman from a district economically dependent upon defense spending will be more likely to cast a jingoistic vote on foreign- and defense-related policy issues than his nondependent colleague.

The first line or row in table 4 lists the zero-order correlations between the various measures of defense spending and the two voting scales for the entire sample. The use of a second, longer scale including actual defense spending bills does not change the pattern of the correlations. The only significant correlation is between voting and DDM. However, for both of the payroll-based measures use of the longer scale results in somewhat stronger correlations, while for the prime contract-based measures its use results in slightly weaker ones.

As has been noted, split-county districts—usually those in urban areas—are more subject to error than whole-county ones. Therefore, it was suspected that the inclusion of these areas in the total sample (the whole House) might be reducing the correlation between the measures of defense spending and the *J*-scales. For that reason, separate correlations between the *J*-scales and the measures of defense spending were run with the split-county districts removed. Results of such tests are reported in row 2 of this table. In only two cases does the presumably more accurate whole-county subsample yield even a marginally stronger correlation between spending and voting than was found for the whole House. This seems to indicate either that the more rural, whole-county districts experienced a somewhat weaker correlation between the *J*-scales and spending or that the stronger correlations for the whole sample are at least partially a result of error. In any event, there is little support here for the original hypothesis of the study.

The next test reported in this table was an attempt to strengthen the correlations by assuring a homogeneous sample on variables which are also related to the dependent variable—the voting scales. A separate correlation was obtained for Republicans and Democrats. The possibility existed that significant correlations in opposite directions could be canceling each other out when both of the parties were considered together. The results of this table are quite interesting and begin to specify the

prime contracts. Gray and Gregory found no significant relationship between their Cold War-ethnocentrism scale and their indicators of military spending. Cobb found no relationship in the House between spending and voting in 1964.

TABLE 4

CORRELATION BETWEEN SEVERAL MEASURES OF DEFENSE SPENDING AND J-SCALES BY VARIOUS SUBSAMPLES

SAMPLE	N		DIM		DIC		DDM		DDC	
	Short	Long	Short	Long	Short	Long	Short	Long	Short	Long
Whole sample	400	395	-.039	-.018	-.059	-.035	-.114*	-.167*	-.035	-.078
Whole county districts only	201	198	-.038	-.034	.011	.079	-.095	-.136*	-.036	-.074
Republicans	174	173	-.104	-.039	-.060	-.065	-.117	-.123	-.083	-.041
Democrats	226	222	-.070	-.075	-.121*	-.080	-.200*	-.279*	-.066	-.168*

* Statistically significant.

TABLE 5

CORRELATION OF VARIOUS MEASURES OF DEFENSE SPENDING WITH J-SCALES BY REGION

Region	N		DIM		DIC		DDM		DDC	
	Short	Long	Short	Long	Short	Long	Short	Long	Short	Long
South	95	91	.066	.090	.218**	.325**	-.078	-.134	-.025	-.041
Border	26	26	.296	.324	.005	.038	.009	.057	.368	.321
North East	101	101	-.056	-.078	-.125	-.157	.084	-.064	.105	-.058
North Central	110	110	-.090	-.120	-.113	-.142	-.099	-.110	-.033	-.060
Mountain West	17	17	-.081	-.055	.211	.324	.240	.268	-.055	.025
Far West	50	50	-.074	.022	-.036	-.020	.069	-.051	.043	.001

Note.—Region is defined in the following manner: South: Texas, Oklahoma, Arkansas, Louisiana, Mississippi, Alabama, Georgia, Florida, South Carolina, North Carolina, Virginia, and Tennessee; Border: Kentucky, Maryland, Delaware, Missouri, and West Virginia; North East: Pennsylvania, New York, New Jersey, Connecticut, Massachusetts, Rhode Island, Vermont, New Hampshire, and Maine; North Central: North Dakota, South Dakota, Wisconsin, Kansas, Minnesota, Iowa, Nebraska, Illinois, Michigan, Indiana, and Ohio; Mountain West: New Mexico, Arizona, Nevada, Utah, Colorado, Idaho, Wyoming, and Montana; Far West: California, Oregon, Washington, Alaska, and Hawaii.

** Statistically significant, but not in the expected direction.

relationship in question. The only significant findings are for Democrats on payroll-based measures, especially those based on the military payroll. While separation by parties does strengthen several of the correlations, it is clear that the few, small, significant correlations found for the total sample are due primarily to the influence of patterns which really hold only for the Democrats.

Table 5 represents another attempt to reduce the impact of variables associated with the *J*-scales which may be suppressing the correlations between defense spending and voting. Separate correlations were computed for each region. Of the 48 correlations, 14 were in the expected direction; however, none were significant. Only two correlations were significant, and they do not support the hypothesis because they are not in the predicted direction. Table 5 does not lend support to the hypothesis that military spending and voting on defense-related issues are correlated, at least for the entire sample broken down by region. This finding does not contradict the findings of the previous table with regard to party groupings and such a relationship.

Table 6 reports another series of tests. It was hypothesized that certain representatives, who either were members of powerful committees and committees with direct bearing on the defense appropriations process or had a great deal of seniority, might be more likely to find themselves rewarded with large amounts of military spending in their districts if they voted "properly." This might be expected because the votes and influence of senior members of the body and members of important committees are simply more important than the support of the average back-bench representative. Power in Congress is associated with membership on prestigious committees and with length of service. Therefore, I computed separate correlations for those with high seniority and those who were members of powerful committees.

Though few of the correlations were significant, there is support for what might be termed the "Mendel Rivers Effect."¹⁰ The correlations between the measures of defense spending and the *J*-scales were uniformly stronger among the high seniority groups than among representatives with less seniority. In five out of eight cases, the correlations between the *J*-scales and defense spending were also stronger for members of powerful committees than for those who were not members of those committees (though the differences were less than those between the two seniority groups). The most striking pattern in this table is the uniformly significant

¹⁰ The late Mendel Rivers, former chairman of the House Armed Services Committee and a jingoist of some note, was the beneficiary of enormous amounts of defense spending. Each of the Services and many top defense contractors had facilities located in his district. It was suggested that one more defense installation would cause his coastal district to sink into the Atlantic.

TABLE. 6

CORRELATION BETWEEN SEVERAL MEASURES OF DEFENSE SPENDING AND *J*-SCALES BY VARIOUS SUBSAMPLES
BASED ON WITHIN-HOUSE POWER SOURCES

SAMPLE	N		DIM		DIC		DDM		DDC	
	Short	Long	Short	Long	Short	Long	Short	Long	Short	Long
Whole sample	400	395	-.039	-.018	-.059	-.035	-.114*	-.167*	-.035	-.078
High seniority	66	65	-.122	-.061	-.055	.004	-.345*	-.469*	-.167	-.285*
Low seniority	334	330	-.026	-.011	-.079	-.060	-.081	-.122*	-.009	-.030
Member of a powerful committee	153	152	-.073	-.024	-.062	-.056	-.114	-.159*	.011	-.053
Not a member of a powerful committee	247	243	-.019	-.014	-.057	-.022	-.115*	-.175*	-.057	-.090

NOTE.—For purposes of this table, those who have served in the House for 10 or more terms are rated as high seniority. Those with fewer than 10 terms have been placed in the low seniority category. Five committees were considered powerful and prestigious. They are the Appropriations, Armed Services, Foreign Relations, Ways and Means, and the Rules Committees.

* Statistically significant in the expected direction.

TABLE 7

CORRELATIONS BETWEEN *J*-SCALES AND SEVERAL MEASURES OF DEFENSE SPENDING BY SENIORITY GROUP

SPENDING MEASURE	SENIORITY IN TERMS																	
	1S	1L	2S	2L	3S	3L	4S	4L	5S	5L	6-7S	6-7L	8-9S	8-9L	10-12S	10-12L	13+S	13+L
DIM	-.051	-.013	-.101	-.197	.297**	.314	-.102	-.038	-.047	-.051	-.096	-.095	-.069	.031	.000	.077	-.293	-.220
DIC	-.126	-.064	-.269*	-.283*	.103	-.006	-.030	-.024	.103	.080	-.031	.013	.122	.098	-.059	.087	.231	.362*
DDM	-.090	-.178	-.005	-.159	.127	.045	-.189	-.089	-.004	-.145	.243	.266	-.047	-.023	-.096	.380*	.523*	.322*
DDC	-.187	-.051	-.194	-.191	.116	.094	-.063	.029	.117	.150	.003	.061	.141	.083	.040	.174	.387*	.396*
N	.57	.56	.63	.62	.47	.47	.42	.41	.43	.42	.36	.36	.46	.46	.40	.40	.25	.25

NOTE.—Column 1S refers to the correlation between the short J-Scale and the particular measure of defense spending for those with one term of seniority. Column 1L refers to the correlation between the various defense spending measures and the long J-Scale for those with one term of seniority. Column 2S refers to the correlations between defense spending and the short J-Scale for those with two terms of seniority, etc.

* Statistically significant.

* Statistically significant.

** Statistically significant, but not in the expected direction.

series of correlations between the long voting scale and DDM. This is interpreted to mean that patterns in the relationship between spending and voting are structured to conform to the power patterns within the House. When congressmen are separated into groups on bases which are meaningful in terms of within-House power relations, the expected correlations begin to appear between voting and the spending measure based on military payrolls. In general, committee membership does not seem to be as important as seniority in terms of differentiating House members as to the strength of the relationship between spending and voting. Defense spending (based on military payrolls) is much more effective in explaining voting among senior members than among junior members of the House.

In table 7 an attempt is made to specify more precisely the nature of the effect demonstrated in the previous table. It shows that there is no pattern of continuous increase in the correlations between spending and voting as the seniority of a member increases. There is, however, a consistent and relatively large jump between the group comprising all representatives with fewer than 13 terms in the House and those who have served for 13 or more terms. With few exceptions, the correlations obtained between all measures of defense spending concentrations and both of the voting scales for this very senior group are considerably stronger than those obtained for the same relationship in every group of lesser seniority. All of the correlations between voting and spending based on payrolls were significant, and, though not statistically significant, all of the correlations between voting and spending based on prime contracts were stronger than those for almost every other seniority group. These patterns are interpreted to mean that a large proportion of the spending-voting effects detected in earlier tables results from the relatively strong correlations among the very highest seniority group. They are also interpreted as partial support for a modified version of the hypothesis that congressmen from dependent districts are more likely than others to vote for defense-related measures.¹¹

SUMMARY OF CONCLUSIONS

It has been found that the votes of congressmen can be explained in appreciable measure by their party affiliations and the regions in which their districts are located. These variables are interpreted to be largely symbolic of stable ideological predispositions. They are indicators which represent a pattern of factors and processes which influence the behavior of the individual congressman.

¹¹ This pattern gives some clue to the direction of the relationship. It is unlikely that defense spending determines voting among the most senior representatives, who are probably too secure and powerful to be swayed by such considerations. Spending concentrations are more likely to have followed their votes and influence in the House than to have caused them. This is, of course, not to say that success in bringing defense-based prosperity did not contribute to their stability in some measure.

Although there is little support for the hypothesis that defense spending concentrations affect voting in the whole House, there is some indication that the relationship exists, especially with regard to measures of defense spending based on the military payroll, among the more powerful, senior members of that body. Several significant correlations in the tables are attributed to a peculiarity of the structure of the House—seniority. It appears that the significant correlations between DDM and the *J*-scales for the whole sample are most heavily attributable to the strong correlation between these indices among Democrats. Perhaps these correlations can, in turn, be partially explained by the strong correlation existing within the very highest seniority group.

DISCUSSION

The lack of more general support for the basic hypothesis of the study warrants discussion. Pressures from constituencies often have considerable influence on the voting of representatives. The military and large industrial contractors are certainly well organized and powerful constituents. Bolton (1966) has demonstrated the partial dependence of the country on defense spending for prosperity and the fact that disarmament would probably mean at least temporary economic dislocations of considerable magnitude, especially in certain regions and sectors of the economy. It is, therefore, important to attempt to explain why there is so little relationship between defense spending and voting among all but the most senior and powerful representatives.

Figures in this study show (1) that for most representatives there is little or no relationship between defense spending concentrations and voting on defense-related issues, and (2) that actual defense authorization and appropriations measures were at one extreme of the *J*-scale continuum, that is, very few representatives voted against actual defense spending measures. A possible reason for these two findings is suggested by the work of several authors who have been concerned with ideas about the nature of societal governance.

Lieberson (1971) begins to expand the range of models of societal governance beyond the traditional elitist and pluralist notions. In his study of military-industrial linkages, he finds that neither of these ideas adequately fits the data and that a third is suggested. He first sketches the older models: "On the one hand, the elitists argue that a relatively small group of people, representing a relatively narrow range of interests, determine national policy across a range of domains including foreign affairs, military spending, and major domestic programs. By contrast, the pluralist school views national decision making as a process influenced by a broad and diverse array of interest groups, with no single group or

set of groups powerful enough to consistently dominate the national political system" (p. 562).

Feeling that neither model adequately explains the situation with regard to defense spending, he suggests a third approach which he labels the "hypothesis of compensating strategies." He states that "... military spending can be explained by a high level of interest on the part of one segment of American business accompanied by a relative lack of concern on the part of the other segments of industry" (p. 578). He feels that industrial concerns concentrate their efforts in areas which benefit them most directly. They ignore small losses in other areas in order to concentrate their forces in areas more relevant to their concerns. Industries like aircraft manufacture, ordnance production, and electronics design and construction would suffer considerable losses if armaments spending were to decline precipitously. Such industries, among others, would be likely to promote continued high levels of defense spending.

A most interesting convergence with Lieberman can be seen in the work of Lowi (1964). He, too, is dissatisfied with both pluralist and elitist models. While he states that these two models do provide an accurate analogy to the real operation of forces in certain "arenas of power," he believes that there is a third model of power relations which operates under certain conditions.

Lowi feels that the classic pluralist model of relatively open competition and coalition formation applies to the arena of regulatory decisions. Where there is a confrontation of winner and loser, privileged and deprived, Lowi maintains that coalitions are formed on the basis of mutual interests in order to present the strongest power position in the fight for the limited rewards available.

He feels that the traditional elitist model operates in what he terms the "arena of redistribution." The decisions in this arena involve taking valuable commodities from one large-scale social group, such as a social class, and giving them to another. Lowi notes, "Issues which involve redistribution cut closer than any others along class lines and activate interests in what are roughly class terms. If there is ever any cohesion within peak associations, it occurs on redistributive issues, and rhetoric suggests that they occupy themselves most of the time with these" (1964, p. 707).

His third model resembles Lieberman's idea of compensating strategies. When decisions are made regarding distribution, in the sense of short-term disbursement of assets without regard for limitations of resources, rather than redistribution or regulation, Lowi maintains that parties adopt a live-and-let-live, "log-rolling," or mutual noninterference strategy. "Pork-barrel" programs, defense expenditures, and patronage are examples of areas often within the distributive arena of power. These kinds of

programs can be broken down so that nearly everyone can "get a piece of the action."

Since our tax system is only slightly redistributive, the federal budget might be seen as something of an arena of distribution. Since it is possible to pacify most major interest groups in the federal budget-making process—if not through defense spending then through agricultural subsidies or harbor construction—it is tempting for Congress to use this method and view the process as one in which all groups should be kept happy if possible. This method allows politicians in Congress the luxury of avoiding the unnecessary creation of enemies. As Lieberman suggested, each party will concentrate his strength in his own area, even to the point of taking small losses in other areas. Under this model, industries would be expected neither to conspire to produce certain policies (e.g., high levels of defense spending) nor to compete democratically to produce them. Instead, they would simply ignore each other (and the defense area if they were not directly involved with it) under the assumption that each would be better off if fighting were avoided and each simply looked after his own interests.

With these ideas as a backdrop, it is possible to hypothesize that congressmen from defense-dependent districts would find themselves under pressure from military-industrial complex constituents if they were to vote against defense spending measures. Those from nondependent districts, the majority of whom also vote for defense spending measures, may be conjectured to do so because the presumption in legislative decision making will generally favor the military view under political conditions found in the country during much of the period since World War II. This might be seen in terms of both a congressional behavior pattern¹²—"log-rolling" or "back scratching"—and the fact that there has been no "peace lobby" since World War II sufficient to make defense spending appear to be regulation or redistribution.¹³ Rose (1967) points out that there have been no stable forces working in the political system against high defense budgets during the bulk of the postwar years.¹⁴ Almost no one has

¹² Matthews (1960, pp. 99–100) shows that there is a norm of reciprocity in the Senate. That is, senators vote for bills sponsored by other senators in the expectation that the favor will be returned on their own pet bills. It is likely that one representative will feel similar pressures and vote for the bill of another—as long as it does not hurt him—in the belief that the other representatives will return the favor later.

¹³ I have argued elsewhere (Cobb 1972) that during the late war and early postwar period, when what has been termed the military-industrial complex was developing, the federal budget might have been viewed as an arena of regulation. There was competition to see what spending patterns would be adopted. Any attempt to change federal funding patterns today would probably be viewed by military-industrial complex industries and others as redistribution.

¹⁴ This represents something of a break with past patterns in this country (Ekirch

had what might be termed a "vested interest" in stopping or reducing defense expenditures in the way that members of the military-industrial complex have had an interest in increasing them.

As a result, there is a situation which resembles the arena of distribution discussed by Lowi. There is likely to be a high level of interest on the part of the segment of American business which is closely connected with defense manufacturing and a lack of interest on the part of the rest of American industry. These other businesses might be expected to concentrate their efforts on improving government programs which will benefit them more directly (the notion of compensating strategies): farmers on farm policy, highway contractors on highway construction policy, etc. Under this hypothesis, no elitist association of peak industries or groups is working to keep defense spending high. There would be no need for such imposition of control, since those who do not benefit by defense spending might be expected not to fight it but to ignore it in the expectation that they will be rewarded in their own areas.

Therefore, under this notion representatives from congressional districts where these other types of businesses are located will not be "punished" by those constituents if they vote for defense spending to help a dependent colleague.¹⁵ Under these circumstances, both dependent and nondependent representatives will usually vote for actual defense appropriations and authorizations measures.

This means that these kinds of votes will fall at the extreme end of the defense-related voting scales. The entire scale of defense-related issues will be unrelated to defense dependency for most congressmen, then, because under conditions of an arena of distribution with widespread use of the notion of compensating strategies that scale is responsive to ideological factors such as those represented by party and region. The exceptions to this idea, the very senior congressmen, may perhaps be different because of their special power (the hinges of a door need oil) or, as I have argued elsewhere, the difference may be something of a historical residue of the earlier time when an economy based on defense spending was not yet so widely accepted and elements of the emerging military-industrial complex had to work to insure the flow of defense funds (Cobb 1972).

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1956). The present pattern appears to have been undergoing some change in recent years.

¹⁵ In addition, the widely perceived foreign threats posed by Stalin and communism or Hitler and fascism at an earlier point should not be neglected when causes for prodefense votes are enumerated.

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The Cost of Being Black: A 1970 Update¹

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An analysis of U.S. census data regarding race, age, education, occupation, and income of the male experienced civilian labor force in 1960 and 1970 yields four major conclusions. (1) There were large reductions in differences between white and nonwhite occupational distributions at all ages, with the change clearest for young men. (2) The reduction of differences in these distributions is clearest at the lowest and highest educational levels. (3) The absolute gap between nonwhite and white income (in constant dollars) increased. (4) The increased income gap is due in large part to a general upward shift of the labor force into education/occupation categories with a more pronounced income differential by race. Comparisons are made with changes from 1950 to 1960 in order to evaluate recent changes in a broader context.

In 1965 Paul Siegel reported changes in white-nonwhite occupational and income differentials from 1950 to 1960. A number of similar analyses of changes from 1960 to 1970 have appeared (e.g., Farley and Hermalin 1972; Fox and Faine 1973; Hauser and Featherman 1974), but none have used the decomposition techniques utilized by Siegel to indicate the sources of income differentials and none have compared the changes from 1960 to 1970 with those from 1950 to 1960. The present paper duplicates Siegel's analysis for the 1960-70 decade and presents further evidence concerning some of the hypotheses which he presented concerning the sources of some of the patterns in the data.

OCCUPATIONAL DISTRIBUTION

Table 1 contains indices of dissimilarity of occupation for four age cohorts by five educational levels. The index (Duncan and Duncan 1955) indicates the proportion of the nonwhite (or white) labor force which would have to change major occupational category in order to equalize the white-nonwhite occupational distribution for each particular age and educational level.

Intercohort Comparisons

Table 1 allows us to make two kinds of comparisons which can serve as indicators of change. First, intercohort comparisons indicate the extent

¹ This is an abridged version of a paper presented at the annual meeting of the American Sociological Association, San Francisco, August 1975. This version of the paper has benefited from many useful comments of two anonymous *AJS* referees.

TABLE 1

INDICES OF DISSIMILARITY BETWEEN WHITE AND NONWHITE OCCUPATIONAL
DISTRIBUTIONS FOR MALES AGED 25-64 IN THE UNITED STATES,
1950, 1960, 1970 BY AGE AND EDUCATION

EDUCATION (YEARS)	AGE (YEARS)			
	25-34	35-44	45-54	55-64
0-7:				
1950	26.1	28.3	28.4	30.4
1960	25.1	25.9	28.8	30.4
1970	17.7	18.4	21.9	25.7
8-11:				
1950	29.0	34.2	36.2	33.3
1960	27.4	29.8	32.0	35.1
1970	21.7	26.4	28.8	31.7
4 high school:				
1950	33.8	39.1	40.5	36.9
1960	33.4	34.1	35.8	38.7
1970	28.4	31.8	33.8	35.6
1-3 college:				
1950	38.8	39.5	39.3	29.2
1960	34.4	38.2	37.6	34.8
1970	27.6	32.7	37.0	36.5
≥4 college:				
1950	17.8	17.8	21.6	17.2
1960	18.6	19.1	18.7	19.0
1970	9.9	15.8	15.9	15.0

SOURCES.—U.S. Bureau of the Census 1953, table 11; 1963a, table 8; 1973a, tables 8 and 9.

NOTE.—Figures for 1950 are for employed males; those for 1960 and 1970 are for experienced labor force; 1950 and 1960 figures are white-nonwhite comparisons; 1970 ones are black-nonblack.

to which the white-nonwhite occupational distributions have changed from one census year to the next. Of the 20 possible comparisons for the decade from 1950 to 1960, 12 show reductions in occupational differentiation; 19 of the 20 comparisons for the 1960-70 decade show changes in the direction of a more equal distribution. Additionally, in most cases the change from 1960 to 1970 was considerably greater than the corresponding change from 1950 to 1960, a fact that quite possibly reflects greater concern with discriminatory practices during the 1960s.

Siegel noted that in 1950 and 1960 the index of dissimilarity was generally highest for men who had completed some college and dropped fairly dramatically for those who had finished college. The same pattern is clearly present for the 1970 data. Siegel had speculated that the large index of dissimilarity for men with some college was a result of white reluctance to place nonwhites in positions of authority over whites and that nonwhite men with some college education who qualified for just those sorts of managerial positions did not get them, while their white

counterparts did. Nonwhites who complete college, on the other hand, are able to move into professional positions where their clientele is more likely to be nonwhite and where they can control the nature of their own employment to a greater extent.

As table 2 shows, this was clearly the case in 1960. The white-nonwhite difference in participation in the managerial occupations accounted for a large proportion of the differences at the level of both one to three years of college and four or more years of college. For all age categories in those educational groups, the largest difference between white and nonwhite proportions in a major occupational category was for the managerial occupations. For men who had completed only one to three years of college, the occupations in which nonwhites were most overrepresented were the service occupations, with this tendency more pronounced for older men. For men who had completed college, occupational distribution depends even more upon age. As Siegel hypothesized, young college-educated nonwhites are most overrepresented in the professional occupations. For older men the nonwhite overrepresentation seems to be spread fairly evenly among the professional, clerical, and service occupations.

For 1970 (table 3) the pattern is similar, with greater underrepresentation of blacks in managerial positions for men with one to three years of college than for men who completed college; again blacks are most underrepresented in the managerial category in all age groups. In 1970 the blacks with one to three years of college are overrepresented primarily among operatives, with a shift toward service occupations for older men. In 1970 black men who had finished college were only slightly overrepresented in the professions.

Siegel's interpretation of patterns at the upper end of the educational scale fits the data relatively well. The comparative lack of differentiation at the highest educational level is probably due to the ability of men who have finished college to control the nature of their own employment. The 1970 data, however, present us with a new phenomenon: fairly dramatic decreases in occupational differences at the very lowest educational level and only moderate to small changes among men who completed grammar school. Clearly the problem here cannot be related to differences between employment processes for managers and professionals. The data for the two lowest educational levels (tables 2 and 3) suggest the possibility that these differences are related to union control of craft and operative occupational categories. Proportionately more blacks entered the operative occupational category during the 1960s than during the preceding decade, but blacks continued to be excluded from the craft occupations. It would appear, then, that policies to reduce occupational discrimination among men who have not attended college will depend primarily upon the suc-

TABLE 2
PERCENTAGE WHITE MINUS PERCENTAGE NONWHITE IN MAJOR OCCUPATIONAL CATEGORIES BY AGE AND EDUCATION: 1960

Education and Age (Years)	Professional	Managerial	Sales	Clerical	Crafts	Operative	Labor	Farm	Farm Labor	Service
0-7:										
25-34	00.3	01.9	01.3	00.9	11.5	09.0	-15.7	-00.7	-03.8	-04.7
35-44	00.5	03.2	01.6	00.7	14.6	05.1	-16.7	00.3	-04.0	-05.1
45-54	00.4	03.8	01.9	01.0	15.0	05.0	-16.1	01.8	-05.0	-07.2
55-64	00.4	04.8	02.6	01.8	15.5	03.9	-15.9	01.3	-06.7	-07.9
8-11:										
25-34	01.4	04.3	03.0	00.1	15.5	00.7	-15.4	02.5	-01.6	-10.4
35-44	01.2	05.9	03.4	-00.4	15.3	-03.5	-14.8	04.0	-01.1	-10.0
45-54	01.0	08.1	03.8	00.6	13.8	-03.0	-13.5	04.7	-01.4	-14.2
55-64	00.8	08.5	05.1	01.9	13.4	-04.3	-12.3	05.3	-02.1	-16.5
4 high school:										
25-34	04.0	08.9	05.7	-02.9	12.5	-09.5	-11.0	03.9	-00.8	-10.9
35-44	03.7	11.1	06.2	-03.7	10.2	-09.0	-10.0	03.0	-00.7	-10.8
45-54	02.6	14.8	06.8	-02.1	08.0	-08.2	-09.1	03.6	-01.2	-15.2
55-64	03.6	16.1	08.4	-00.2	06.9	-07.7	-08.5	03.6	-01.0	-21.3
1-3 college:										
25-34	08.2	12.1	09.9	-08.2	02.9	-09.7	-06.4	01.3	-00.3	-09.9
35-44	07.2	18.1	10.6	-08.6	00.7	-10.7	-06.8	01.6	-00.6	-11.7
45-54	06.3	19.7	09.9	-08.3	00.1	-08.2	-04.8	01.6	-00.1	-16.4
55-64	04.8	21.1	07.5	-04.5	-01.1	-06.6	-06.8	01.5	-01.0	-15.1
≥4 college:										
25-34	-08.4	09.2	07.7	-03.9	01.0	-02.0	-01.1	00.6	-00.0	-03.0
35-44	-04.5	12.4	06.0	-06.0	00.3	-02.8	-01.7	00.4	-00.3	-04.0
45-54	-04.2	13.5	04.7	-05.2	-00.7	-02.3	-02.2	00.5	-00.1	-04.1
55-64	-03.4	14.4	04.3	-03.7	00.4	-02.7	-02.6	00.0	-00.1	-06.6

Source.—U.S. Bureau of the Census 1963a, table 8.

Note.—For example, out of a total of 813,992 white 25-34-year-old workers with 0-7 years of education, 5,960 or 0.7% are in professional occupations. For nonwhites the total is 280,542, with 989 or 0.4% similarly classified. This table and table 3 report the differences, e.g., $.7 - .4 = .3\%$. The largest positive and negative differences in each row are in italics.

TABLE 3
PERCENTAGE NONBLACK MINUS PERCENTAGE BLACK IN MAJOR OCCUPATIONAL CATEGORIES BY AGE AND EDUCATION: 1970

Education and Age (Years)	Professional	Managerial	Sales	Clerical	Crafts	Operative	Labor	Farm	Farm Labor	Service
0-7:										
25-34	00.6	02.0	01.1	00.0	10.6	03.2	-10.8	00.2	-04.1	-02.8
35-44	00.6	02.9	01.4	00.4	11.7	00.5	-11.6	01.0	-02.7	-04.1
45-54	00.7	03.4	01.5	00.4	11.8	02.0	-13.1	02.1	-03.1	-05.7
55-64	00.6	03.9	02.2	01.0	13.2	01.5	-14.4	03.3	-03.4	-07.9
8-11:										
25-34	00.9	03.7	02.1	-01.3	13.7	-04.1	-08.6	01.3	-01.2	-06.4
35-44	01.3	05.8	02.7	-00.8	13.9	-07.9	-08.8	02.7	-00.7	-08.2
45-54	01.1	06.5	03.3	-00.6	13.7	-06.6	-10.4	04.1	-00.7	-10.5
55-64	01.2	07.0	04.3	00.0	13.7	-05.7	-10.5	05.5	-00.7	-14.8
4 high school:										
25-34	04.1	06.2	03.9	-03.3	12.0	-13.3	-05.7	02.1	00.1	-06.0
35-44	03.5	10.2	04.8	-03.2	09.9	-13.4	-06.8	03.5	-00.1	-08.3
45-54	03.9	11.3	06.6	-04.2	08.7	-11.4	-07.4	03.3	00.0	-10.8
55-64	03.4	12.9	07.9	-00.6	07.3	-09.7	-08.0	04.1	-00.3	-16.9
1-3 college:										
25-34	07.1	09.3	06.8	-07.8	02.8	-11.9	-03.3	01.2	00.3	-04.7
35-44	06.8	14.9	08.9	-09.7	00.7	-11.0	-04.0	01.4	-00.1	-07.9
45-54	08.2	16.7	10.2	-08.5	-01.8	-11.0	-04.1	01.9	-00.3	-11.3
55-64	07.0	17.6	09.8	-06.5	-02.2	-07.8	-05.1	01.9	-00.7	-14.4
≥4 college:										
25-34	-01.8	04.7	04.6	-01.7	-01.2	-02.7	-00.8	00.6	00.1	-01.6
35-44	-03.6	09.2	06.3	-03.6	-02.2	-02.2	-01.1	00.3	00.0	-03.0
45-54	00.3	09.4	05.6	-05.0	-02.0	-03.2	-01.3	00.6	00.0	-04.4
55-64	00.5	07.7	05.6	-03.7	-02.1	-02.8	-02.3	01.2	-00.4	-03.8

Source.—U.S. Bureau of the Census 1973a, tables 8 and 9.
NOTE.—The largest positive and negative differences in each row are in *italics*.

cess of efforts directed at the craft occupations, while efforts to produce change within the upper educational levels must be aimed at managerial occupations.

The pattern of change by age is as one might have expected. From 1960 to 1970 the major changes at all educational levels are among the younger groups of men (table 1), particularly those aged 25-34. This was not the case for the 1950-60 decade and seems to indicate that antidiscrimination programs are having their major impact on young men who are just entering the labor force.

Intracohort Comparisons

A second major type of comparison which can be derived from table 1 is comparison of occupational differences among the same group of men in each census year. Nine of the 15 possible intracohort comparisons showed improvement for the 1950-60 decade, while all 15 showed improvement for the more recent time period. Compared with the intercohort comparisons, however, these intracohort changes are relatively small. The most significant changes in occupational distributions are at young ages when men first enter the labor force.

INCOME DISTRIBUTION

The second major section of Siegel's article dealt with income inequality within major occupational categories, educational levels, and regions of the country. Following Siegel's methodology, white-nonwhite income differentials were decomposed; results are presented in table 4. In order to allow decade comparisons, the data analyzed by Siegel were converted to 1969 dollars and the decomposition was recomputed. A conversion ratio of 1.26 times the 1959 income was utilized, reflecting the corresponding change in the consumer price index.

Looking first at changes within educational categories, we find that (1) at all educational levels there has been a decrease in that portion of the white-nonwhite income differential which is due to differences in position in the occupational and regional structure of the nation, and (2) there has been a decrease in income differences *within* occupational and regional categories for men who have at least finished high school. Although it is somewhat discouraging to note the increased net differences at the lower educational levels, the percentage of the labor force who did not complete high school decreased for both whites and nonwhites during the 1960-70 decade, from 53% to 39% for whites and from 77% to 63% for nonwhites (see table 5).

This shift upward in educational level for both whites and nonwhites

The Cost of Being Black

TABLE 4
DECOMPOSITION OF WHITE-NONWHITE MEAN INCOME DIFFERENCES (IN 1969 DOLLARS)

Years of Education and Year	Nonwhite White	Total Difference	Composition*	Net
0-8:				
195962	2,137	1,059	1,078
196970	2,102	979	1,123
1-3 high school:				
195963	2,557	952	1,605
196970	2,612	906	1,706
4 high school:				
195964	2,803	1,035	1,768
196972	2,618	880	1,738
1-3 college:				
195958	4,023	1,812	2,211
196969	3,357	1,233	2,124
≥4 college:				
195955	5,744	965	4,779
196971	4,459	761	3,698
All educational levels:				
195953	3,587	2,207	1,380
196963	3,708	2,034	1,674

Sources.—U.S. Bureau of the Census, 1963*b*, tables 2 and 3; 1973*b*, tables 3 and 4.
* Within educational levels composition is the part of the difference due to differential representation in major occupational categories and regions of the country. For "all educational levels" composition also includes the portion of the difference due to differential access to education.

is responsible for a phenomenon which Siegel predicted on the basis of his previous analysis. In spite of a general reduction in white-nonwhite income differences within educational levels, the overall gap between white and nonwhite income has *increased* during the 1960-70 decade. Although nonwhite income expressed as a proportion of white income has increased, suggesting improvement in the situation in one sense, the decomposition in the last row of table 4 indicates that while the differences due to white-

TABLE 5
EDUCATIONAL ATTAINMENT OF WHITE AND NONWHITE MALES IN EXPERIENCED
CIVILIAN LABOR FORCE: 1960 AND 1970

YEAR AND GROUP	YEARS OF EDUCATION					D*
	0-8	1-3 HS	4 HS	1-3 C	≥4 C	
1960:						
White32	.21	.25	.10	.12	26.0
Nonwhite58	.19	.14	.05	.04	...
1970:						
White20	.19	.32	.12	.17	22.5
Nonwhite37	.25	.24	.08	.07	...

Sources.—U.S. Bureau of the Census, 1963*b*, table 1; 1973*b*, table 1.
* Duncan's index of dissimilarity.

nonwhite social position in terms of education, occupation, and region of the country have decreased, the labor force has shifted upward to an educational level at which the gap between whites and nonwhites within educational levels, occupational categories, and regions of the country is large. The increase during the decade in the total difference between white and nonwhite income is due to the increase in what Siegel characterized as "the cost of being Negro." In 1969 dollars, the fee for being black was \$1,380 in 1959 and \$1,647 in 1969.

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Commentary and Debate

REPLY TO ALLAN'S COMMENT

Allan has pointed out in a simple, elegant, and compelling argument (*AJS* 81 [May 1976]: 1498–1500) that, in the case of linearly related and normally distributed variables, the partial *tau* coefficient will not be zero when the partial product-moment correlation is zero. Hence the partial *tau* coefficient will not completely detect spurious association. His logic leaves me no choice but to agree with him.

Reynolds (1973) tried to make this point earlier with a computer simulation but introduced so many other sources of error that his argument was not convincing.

However, in granting Allan's point, I do not wish to agree that the problems are now solved and that we should follow his solution. There are three issues I would like to raise. These involve (a) the consideration of cases in which the product-moment coefficient does not detect spuriousness when it should, according to ordinal models; (b) the magnitudes of the errors of the *tau* coefficient in detecting spuriousness in the normal case; and (c) comments about Allan's suggestion that we estimate product-moment correlations from *tau* coefficients and use them in multivariate analysis.

Like others evaluating ordinal statistics, Allan assumes a linear additive model for which correlation coefficients and partial correlations are the proper and sensible statistics for analysis. Then he shows that the ordinal methods do not do as well. Specifically, in this case, he shows that the partial *tau* coefficient is not zero when the partial correlation coefficient is. It is not particularly surprising to find that ordinal methods come off rather more badly than do methods which are defined to be correct by the assumptions of the model. One could as well invent examples in which partial *tau* is properly zero—that is, there is no ordinal partial association—and show that linear methods come off badly compared with ordinal methods.

However, both of these approaches miss the main point. We want to develop and use methods that are minimally misleading when, in fact, we do not know what methods of analysis are most appropriate for the data. To show that ordinal methods are worse than linear methods when the latter are appropriate or that linear methods are worse than ordinal methods when these latter are appropriate does not directly address this point. But the point does need to be addressed, and we ought to continue work in this area.

The second question which needs to be answered is not whether there

is error in the detection of spuriousness but how much error is introduced by inappropriately using the *tau* coefficient in the multivariate normal case. Allan has illustrated his point with a rather dramatic example. He chooses r_{12} and r_{23} to be equal to .75, with the result that r_{13} is equal to .5625. The result is that the partial *tau* coefficient is .141, rather than zero.

Now correlations on the order of .75 are rare in the social sciences and are more rare in those bodies of data in which nonparametric statistics such as *tau* tend to be used. Had Allan illustrated with smaller correlations, he would have generated a *tau* coefficient that was rather closer to zero. For example, had he used $r_{12} = r_{23} = .2$, implying that $r_{13} = .04$, the resulting partial *tau* coefficient would be about .01. This is rather less dramatically different from zero than Allan's illustration.

It would be pleasant to be able to reassure ourselves that, for small levels of association, this bias is small and not troublesome. However, I have been unable to assure myself that that is so. It turns out that, in the three-variable normal case, with r_{23} being spurious, the ratio of the partial *tau* coefficient to the zero-order *tau* coefficient is constant and approximately equal to $1 - (2/\pi)$. That is, as r_{12} and r_{13} approach zero, the relative bias in the *tau* coefficient does not approach zero but, instead, approaches that constant which is approximately equal to .363. It further turns out that this constant provides a remarkably good approximation to the ratio of the partial *tau* coefficient to the zero-order *tau* coefficient in the three-variable normal case, even when the correlations involved are quite far from zero. I have prepared a note on this phenomenon that is too long to reproduce here, but I will make it available to anyone who asks me to. So it seems the partial *tau* coefficient will not properly detect spuriousness, even when the relationships involved are quite small.

However, the question still remains. Are the errors introduced by using ordinal measures of association of greater magnitude than those introduced by the product-moment system, when neither of them is exactly appropriate? Just as we have discovered that the *tau* coefficient is inappropriate for multivariate normal associations, we can invent examples for which the product-moment system gives us misleading results when the ordinal system does not.

Finally, I want to comment on Allan's suggestion about what should be done. He argues that we should assume underlying multivariate normality and transform *tau* coefficients to the corresponding product-moment correlations. Even if we want to estimate underlying correlations, it is not obvious that we should do it from *tau* coefficients. It seems to me that we could get better estimates of the underlying correlations if we transformed the variables involved, so that they were all approximately normal and interrelations were approximately linear, and then computed correlations from those transformed variables. It would seem a more efficient

use of data. If the assumption of multivariate normality is correct, I would guess that smaller errors would result from this strategy than from the one Allan proposes. In any case, before we accept his suggestion we should compare it with the alternative methods to see which works better.

In conclusion, I must admit that Allan has made me less comfortable with the use of partial ordinal measures of association than I was before I read his comment. Furthermore, his argument is so simple that I cannot understand why I did not think of it. It is so clever that I wish I had thought of it. Since I did not, I am indebted to him for pointing it out to me.

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JUSTICE AS FAIRNESS: COLEMAN'S REVIEW ESSAY ON RAWLS

Our purpose is to respond to James Coleman's treatment of John Rawls's (1971) *A Theory of Justice* in his review essay "Inequality, Sociology, and Moral Philosophy" (*AJS* 80 [November 1974]: 739-64). We are in agreement with Coleman's central claim that the time is ripe for the development of an empirical-normative social theory and that sociology as well as economics has much to contribute to this quasi-philosophical task. We also believe that Rawls's book would have been the richer for a serious consideration of sociological as well as economic material. At the same time we find Coleman's understanding of Rawls and of social contract doctrines sufficiently erroneous or misleading that we believe the potential value of Rawls's contribution to such an empirical-normative social theory may not be apparent from Coleman's review. Thus, our purpose is to provide what we believe to be a more accurate reading of Rawls and the problems raised by *A Theory of Justice*.

Initially, we shall summarize three central doctrines of Rawls.

First, he claims that a just society is one whose institutions are in accord with principles which would be agreed upon by *rational* and self-interested individuals who, while aware of general facts concerning society and human psychology, are ignorant of any information concerning their own place in society. The fact that principles of justice are established by this (hypothetical) agreement in what Rawls calls the original position

is what makes his theory a contract theory. The point of the "veil of ignorance" is, in part, to generate the social contract under impartial conditions.

Second, Rawls claims that the principles of justice which would be selected by these rational agents behind the veil of ignorance are the following: "First principle: Each person is to have an equal right to the most extensive total system of equal basic liberties compatible with a similar system of liberty for all. Second principle: Social and economic inequalities are to be arranged so that they are both: (a) to the greatest benefit of the least advantaged, . . . and (b) attached to offices and positions open to all under conditions of fair equality of opportunity" (Rawls, p. 302).

Finally, Rawls claims that the first principle is lexically prior to the second and that, concerning the second principle, part *b* is lexically prior to *a* (Rawls, pp. 302-3). Thus, where the principles conflict there is a strict order of preference.

Coleman's initial contention is that Rawls's two principles cannot in fact be deduced from the original position. The notion that rational agents behind a veil of ignorance would choose these principles is according to Coleman "an a priori psychological argument which is subject to empirical falsification" (p. 746). He suggests as an example that young voters deciding on principles of taxation over their lifetime but ignorant of their lifetime earnings might make a decision according to principles other than the ones suggested by Rawls. Such voters would "vote for that degree of tax progressivity which . . . would equalize" the tax burden between periods of high and low income (p. 747).

Three comments are called for here. First, Rawls's deduction of the principles of justice is not based on a priori psychology nor is it subject to empirical refutation. This is because questions about what rational agents would choose behind the veil of ignorance are not questions about the psychology of rational agents; they are questions about what choices are rational. The issue is whether the choice of principles is justified given the background information. That depends on whether conclusions concerning principles for a just society follow logically from premises of the sort available to someone behind the veil of ignorance (see Rawls, pp. 136-60). That is a question of logic, not of psychology, and is not subject to empirical refutation.

Second, it is hard to see the relevance of Coleman's example. Rawls's question concerns what basic principles of justice would be agreed upon by agents ignorant of their own place in society. It does not concern what view of taxation would be selected by persons ignorant of their future income. The voters in Coleman's example are not behind *Rawls's* veil of ignorance, nor are they choosing general principles to regulate society.

That they would not choose according to Rawls's principles, then, is simply irrelevant to whether those principles can be derived from the original position.

Finally, it is unfortunate that Coleman neither discusses nor mentions Rawls's arguments for the two principles. Rawls's basic claim (simplified) is that under conditions of high uncertainty (such as those found in the original position) it is rational to select principles which maximize the social minimum (Rawls, pp. 150-61). This line of argument may not succeed (see Barry 1973), but if one wishes to show that the principles of justice cannot be derived from the original position, it does seem appropriate to consider it.

Coleman's only conceptual objection to Rawls's analysis of what is just is that discussed above; however, his fundamental objection to Rawls centers on the empirical processes involved in achieving a just society. Coleman's central contention against Rawls is that the latter's principles of justice are potentially highly authoritarian in that they require almost total concentration of power in the state and in that they justify such forms of state intervention as state rearing of children.

This argument takes two basic forms. First, Coleman holds that the demands for justice could not be achieved except by means of excessive state intervention. Second, he claims that social contract theories generally are authoritarian in that they involve a total transfer of rights from the individual to the state.

His first argument in this regard centers on the degree to which the results of research on educational equality and effectiveness would imply strong governmental intervention in order to implement Rawls's principles of justice. In particular, Coleman feels that the research which indicates the greater efficacy of family and peer group environment over school controlled resources makes it likely that to follow Rawls's principle of fair "equality of opportunity would lead to the destruction of the family as a socializing institution" (p. 753) by necessitating the removal of the child from the family environment. There are several problems with Coleman's analysis in this instance.

First, the empirical evidence on which Coleman rests his claim that the family environment contributes more substantially to success in school than does the school environment is still quite controversial. Bowles and Levin (1968) point out forcefully the weakness (and possible reversibility) of Coleman et al.'s (1966) conclusion on this very point, due to the collinearity we observe between student background and school resource measures in the sample data. Given our present knowledge it seems quite possible that schools can be effective (see Jamison, Suppes, and Wells [1974] and Mood [1970] for an examination of the efficacy of school resources) and that careful and serious compensatory education efforts

could provide greater equality in educational outcomes than at present,¹ without requiring the breakup of the family.

Second, it should be remembered that the principles of justice that Rawls postulates say nothing about equality of *educational* opportunity directly; such a consideration would come into play only as an instrument to achieve justice in the distribution of social goods, primarily wealth, positions, and status. In this regard, Coleman completely ignores the large body of research reviewed (and undertaken) by Jencks et al. (1972), who conclude that neither family environment nor school environment substantially affect an individual's income or job status. Thus, if we believe Jencks and his colleagues, Coleman's discussion of equality of opportunities is irrelevant to the demand of the principles of justice for fair equality of opportunity, since an institution (family or school) which affects neither the distribution of wealth nor position cannot violate Rawls's second principle.

Third, even if we ignore Jencks's results, Coleman fails to realize that under a social system which adhered to Rawls's principles of justice, family environments would in all likelihood be much more equal than they are at present. Rawls's difference principle (part *a* of the second principle) would allow only those differences in wealth or status among families that would benefit the least advantaged societal group, and this seems to call for a much less unequal distribution of wealth and power than we see today. This diminished variance in family socioeconomic status would make school resources *relatively* more effective, thereby increasing the efficacy of compensatory education efforts. Further, since in Rawls's schema, fair equality of opportunity is lexically prior to the difference principle, there is an even stronger demand than that recognized above to reduce socioeconomic status differences among families to the point where such differences do not lead to inherited differences in an individual's ability to compete fairly for societal positions. If such is the case, compensatory education efforts may not even be needed. In any case Rawls's commentary on the problem suggests that the primary means for achieving equal opportunity is a reduction in the variance in wealth (see Rawls, pp. 71-74), not intervention in child rearing. Much of the empirical literature on the topic, including that by Coleman, suggests the plausibility

¹ Despite the death knell that has recently been sounded for compensatory education efforts, there is reason to believe that such programs can be effective, as evidenced by a number of studies (see Jamison et al. 1971; Kaplan 1966; Kiesling 1970; Smith and Bissell 1970; Stallings 1975; and Wargo 1971). Although a number of evaluations of the federally initiated Title I programs have reported the lack of success of compensatory programs, there seems to be justification for the view that much of this failure was due more to political incompetence than to educational ineffectiveness (see Murphy 1971) and that those efforts that were successful are repeatable (see Kiesling 1970).

of this view. We thus find Coleman's reaction to Rawls on this matter a bit misplaced.

Finally, if Coleman's primary concern is the effects of a Rawlsian society on the family, it seems incumbent on him to acknowledge Rawls's own treatment of the family (see esp. Rawls, pp. 128–29, 463 ff., and 510–11). Rawls clearly would object to an interpretation of the liberty principle as allowing society to make such trade-offs between the liberty of the child and that of the parents as Coleman suggests (p. 752). Indeed, Rawls recognizes, as do most contract theorists, that “children are at first subject to the legitimate authority of their parents” (Rawls, p. 463). Thus, from the discussion above, we see that Coleman's concern about the effects of a just society (in Rawls's sense) on the family is a misapprehension, which seems to be denied by both extant social science research and the formulation of Rawls's arguments.²

Coleman's second argument to show that Rawls is authoritarian involves what Coleman regards as an inherent defect in contract theories. He maintains:

The difficulty of a contract theory is precisely that encountered by Rousseau. Once the natural rights are given over to a central agent they are no longer held individually by the persons, and those persons as individuals possess no resources with which to oppose the use of the collectively held resources against them. Rawls's theory has no protection against this difficulty: in the social contract, men give over their sovereign rights to a central agent, with only Rawls's principles of justice to protect them from further aggrandizement and misuse of the resources by the central agent. [Pp. 756–57]

This claim is extremely dubious. First, it is historically inaccurate. It is significant here that Coleman chooses as his examples of contract theories those of Hobbes and Rousseau, two of the more authoritarian ones, but ignores contract theories which are both less authoritarian and (for Americans) more influential, such as Locke's. Locke's *Second Treatise* is after all perhaps the single work that most influenced the framers of our Constitution. Yet the transfer of rights from the individual to the state in Locke is very modest, being limited virtually to providing security, enforcing contracts, and protecting property. Indeed, Lockians have held not only that some rights are not transferred to the state, but also that

² At two points Rawls (pp. 74, 510–11) does note that fair equality of opportunity cannot be perfectly achieved as long as the family exists in its present form, but points out that the difference principle mitigates this problem. Actually, if Jencks (1972) is correct, Rawls is mistaken in viewing the family as a barrier to equal opportunity. If Jencks is not correct, a central research task for social scientists concerned with the implementation of a Rawlsian society would be to examine how much present wealth and status differences between families would have to be reduced to achieve fair equality of opportunity, giving due consideration to the potential efficacy of compensatory education.

there are *inalienable* rights of the individual. Coleman's claim that contract theories are inherently authoritarian seems to be based on an oddly biased sample.

Second, it seems reasonable to suppose that on this issue Rawls's theory is far more similar to Locke's than to Rousseau's or Hobbes's. Coleman claims that Rawls gives us only the two principles to protect us. From a theoretical point of view it is hard to see what further protection one could want beyond the demand for the most extensive equal liberty, especially when we recall that Rawls's first principle is lexically prior to the second and that it is to be enforced by a just constitution. Rawls does not, of course, commend any particular set of institutions to protect our liberties. But the two principles are after all ones which govern the selection of social institutions. It follows that any set of institutions which violates the principles is *ex hypothesi* excluded. Thus, it is difficult to see how Coleman's claims concerning contract theories can be justified either generally or as they apply to Rawls.

It is noteworthy that here Coleman seems to prefer utilitarian views in that they vest the person with natural rights but do not at any point transfer them to the state (pp. 744, 756). Both aspects of this view of utilitarianism are wrong. First, most utilitarians do not subscribe to any theory of natural rights. J. S. Mill, for example, in "On Liberty" explicitly disavows any appeal to natural rights and proceeds to justify individual liberty in that it is instrumental in achieving the greatest happiness for the greatest number. This strategy concerning liberty is not idiosyncratic with Mill. In fact, it is required by the basic theoretical commitments of utilitarianism. It is a matter of some import that this line of argument permits a society to interfere with basic liberty at any point where liberty appears not to lead to the greatest good and that it permits such interference regardless of the consent of the individual interfered with. Contract theories, in contrast, permit the transfer of rights to the state only with the individual's consent and often treat abuse of authority by government as a kind of breach of contract and thus as grounds for civil disobedience or revolution. A good case can, however, be made that contract theories can in principle provide a more secure defense of liberty than can utilitarian ones.

Coleman's solution to this problem of authoritarianism is to suggest a plurality of social contracts, apparently on the assumption that the dispersion of power which would result would preclude the development of the sort of omnipotent state that Rawls's views allegedly lead to. Here the following should be noted. First, to propose this as an alternative to Rawls is to confuse principles of justice with the institutions for their realization. Rawls's principles are principles governing the selection of institutions (Rawls, pp. 7-11). Coleman's pluralistic institutions, insofar

as they are intended to serve the function of preventing an excessive accumulation of power, seem to be more appropriately considered means for the realization of some (unspecified) principles. Second, Rawls's principles are quite compatible with a pluralistic society. The principle of equal liberty is understood by Rawls to include freedom of opinion, life style, and association. More important, if, as Coleman claims, a high degree of pluralism is required to maintain liberty, pluralism is required by Rawls's first principle.

Moreover, there is considerable textual evidence (which Coleman ignores) to suggest that Rawls considers the concentration of economic power the fundamental problem to be solved in implementing both principles. Consider, for example, the following:

[F]air equality of opportunity means a certain set of institutions that assures similar chances of education and culture for persons similarly motivated and keeps positions and offices open to all on the basis of qualities and efforts reasonably related to the relevant duties and tasks. It is these institutions that are put in jeopardy when inequalities of wealth exceed a certain limit; and political liberty likewise tends to lose its value, and representative government to become such in appearance only. The taxes and enactments of the distribution branch are to prevent this limit from being exceeded. [Rawls, p. 278]

Coleman might well rejoin that the means for the realization of this sort of redistribution of wealth would increase the power of a central government while reducing competitive sources of power in society and thus would result in a net loss of liberty. This is not at all clear, however. It is not obvious, in fact, that the political means for achieving this sort of redistribution would need more power than the IRS currently has, although obviously this power would have to be used differently. Of course, if Coleman's rejoinder is correct, then Rawls's principles lead to a different set of institutions from the ones he suggests. Again, the fundamental point is that Rawls's principles are ones for selecting or evaluating institutions. One cannot, then, object to them by what amounts to the claim that they lead to institutions in which these same principles are violated.

Throughout his article Coleman attempts to build the picture of a social system that could operate according to Rawls's principles of justice as authoritarian and oppressive; we have tried to indicate above why this is unlikely to be so. Coleman's view of a Rawlsian system as an "impersonal universalistic, achievement-oriented social system" (p. 749) based on "markets and private property" (p. 752) dominated by a "centralized agency" (p. 752) with relatively unlimited powers is fundamentally incorrect. Rawls does assume that a competitive market-based system with a rather laissez-faire government may satisfy his principles of justice (it may or may not; this is an empirical claim). However, Rawls's view of

a market system seems far from the impersonal corporate capitalist bureaucratic system that now exists; on the contrary, Rawls seems to call for a market model which is composed of much smaller, less powerful actors (see Rawls, pp. 258-83),³ perhaps in accord with what Ivan Illich (1971) calls "left-convivial technologies" or Schumacher (1973) describes in *Small Is Beautiful*. Furthermore, Rawls explicitly states that private property does not seem to be a necessary ingredient in a just society (Rawls, pp. 258 ff.) and thus allows for market-based socialism and worker-controlled production systems as viable alternatives.

Rawls's *A Theory of Justice* is an important work that deserves careful attention. It is perhaps the most significant attempt in recent decades to examine seriously the concepts of justice, fairness, and equality, at a time in our society when these concepts underlie almost every current policy issue. Rawls's conclusions may be theoretically debatable in terms of the choice rational agents would make behind a veil of ignorance (a matter discussed in detail by Alexander [1974], Arrow [1973], Barry [1973], Klevorick [1974], and Musgrave [1974]) or they may be impossible to implement, given present distributions of power (as Miller's [1974] Marxist critique and Buchanan and Bush's [1974] consideration of democratic politics suggest). However, we believe it is important that social scientists not reject Rawls prematurely; he has provided a fruitful conceptual framework from the perspective of which normative and empirical concerns may be integrated. We believe that such normative-empirical approaches to policy issues are increasingly required in order to generate rational solutions to contemporary problems.

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³ E.g., Rawls assigns to the distribution branch of government the task "to correct the distribution of wealth and to prevent concentration of power detrimental to the fair value of political liberty and fair equality of opportunity. . . . This would encourage the wide dispersal of property which is a necessary condition, it seems, if the fair value of equal liberties is to be maintained (p. 277).

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REPLY TO KLEES AND STRIKE

Let me turn first to the most central difference I have with Rawls, and thus the most important issue raised by Klees and Strike. This concerns Rawls's attempt to begin a theory of moral philosophy in an institutional vacuum, with two "principles of justice," or as someone else has said with some insight, an attempt to construct a theory of moral philosophy in the absence of political philosophy. I argue in my essay that this starting point implies that all individual rights must become collectively held in order that these principles be implemented. If these principles, or any principles which specify allocation of rights and resources in pursuit of a social goal,

are to be elevated to the highest position, this implies that the rights and resources must be centrally held, for employment toward this goal by the agencies of government.¹ This is why the very first issue in a social contract theory (and I am not opposed to social contract theories in general, as Klees and Strike imply, as evidenced by the fact that at the end of my essay I offered the outlines of a multiple-contract theory) is not a question of the degree or kinds of inequality to be tolerated, as in Rawls's theory, but rather the specific rights and resources to be given over by individuals to collective use through institutions of government. Rousseau and Locke both recognized this, as Rawls does not, Rousseau proposing that all rights and resources be given over, and Locke carefully delimiting the rights to be given over to collective use by the central agents. Rawls's failure to address this prior question is a major reason for the variety of interpretations given to his theory. But the failure to do so elevates his principles of justice to first position, and in that position, they command all the rights and resources necessary to their fulfillment. If the rights and resources collectively held are restricted (as in Locke's treatment, but not in Rousseau's), the fulfillment of any principles of justice must be subject to that restriction. No such subjugation of the principles of justice can be found in Rawls.

All of this can be seen in a different way if we turn to another issue raised by Klees and Strike, that is, Rawls's two starting points: first, his imagery of persons with natural rights choosing from behind a "veil of ignorance" principles under which to be governed, and second, his contention that rational individuals in such a situation will choose his two principles of justice. As I point out in my essay, this assumption that rational choice would lead to these two principles is based on a priori psychology: there is nothing in rational theory which leads to a choice of maximizing the minimum under conditions of uncertainty or risk, except for persons who are maximally averse to risk. This is a particular psychological characteristic (which, as an empirical aside, few people could be said to possess).

More broadly, there is no reason to believe that rational persons behind a veil of ignorance would make substantive choices of this sort at all. Rather, they would decide a logically prior question: the set of rights that they wished to retain as individuals, the set they wished to be held collectively, and the institutions through which the collectively held

¹ Alternatively, it might be argued that Rawls is not developing a societal theory at all, but instead a pair of principles to inform individual behavior, to make just persons of us all. But the principles are inherently social, specifying conditions under which redistribution is warranted. They attempt to define a just *society*, not a society of just individuals. In addition, Rawls, in outlining the implementation of his theory, discusses various "branches of government," making it clear that his intent is not to inform individual behavior.

rights would be exercised. Having made these prior decisions, rational persons would be content (indeed, required by their decision) to have the principles of justice, as well as other substantive social goals, follow from the exercise of these institutions. This has been a characteristic approach of political philosophy, at least since Locke, and it is the attempt by Rawls to eliminate this process in the search for satisfying principles of moral philosophy that leads to fundamental incompatibilities (as well as to the superficial attractiveness of his theory). In effect, Rawls attempts to superimpose on the forms of political philosophy (the social contract behind a veil of ignorance) a content of moral philosophy. The result is a kind of bastardized political-moral philosophy that does not have the essential properties of internal consistency.²

It is this absence of internal consistency that allows Klees and Strike to quote Rawls to refute points I made about Rawls's theory. Rawls's book abounds with paragraphs that will refute any inferences made about his theory, merely because of the inherent inconsistencies in the theory. He lays out a framework that implies abolition of private property, then defuses any criticism by stating that his theory is compatible with private property. He states principles that imply elimination of family socialization of children, but then in describing his system proposes weak educational institutions which take motivation of children as given ("a certain set of institutions that insures similar chances of education and culture for persons similarly motivated"). I have nowhere argued that Rawls's proposals are not disarming; but it is their very inconsistency that makes them so.

Klees and Strike make other comments, some of which are substantial and with which I agree, and some of which are insubstantial and fall of their own weight. In the former category is their argument for the viability of social contract theories in general as bases for social and political systems. I heartily agree, and I hope to contribute to this body of work. In the latter category are their comments on the efficacy of schools for transforming children: here they must choose between their argument that schools are so effective that they can overcome educational inequalities arising from the family (see their first footnote), and their argument, following Jencks, that schools are ineffective.

It is useful to ask here, Just why has Rawls's theory been so attractive

² A new book by a colleague of Rawls, Robert Nozick, entitled *Anarchy, State, and Utopia*, provides a political theory without such bastardization and shows the inconsistencies inherent in Rawls. For an excellent comparison between Rawls and Nozick, see Singer (1975). Singer, a philosopher whose values predispose him more in a Rawlsian collectivist direction than in a Nozickian anarchist direction, nevertheless concludes, "The devastating critique of Rawls in *Anarchy, State, and Utopia*, directed especially at the case for redistribution in accordance with the maximin rule, must very nearly complete the demolition of Rawls's impressive structure" (p. 22).

to many intellectuals? I believe it is because a central occupation of modern intellectuals is the search for a satisfying set of principles concerning inequality in society. Political theory, which specifies rights to be held individually and collectively and the institutions through which collectively held rights are to be used, does not provide that satisfaction. Or if it does, it does so only through a laborious tracing out of the substantive implications of a set of political institutions. It is a metatheory for moral issues, leaving the moral issues to be determined through those institutions. But Rawls, bypassing this process, provides an instant answer to the questions of inequality. It is, I believe, this illusion of suddenly coming upon the pot of gold at the end of the rainbow that has attracted many persons to Rawls's theory.

This is not to say that Rawls's ideas should be dismissed. In his earlier "Justice as Fairness" (1958) Rawls develops the two principles which form the central substantive core of his later *Theory of Justice*. My reaction to that paper was extremely favorable, and as I reread it, my reaction remains favorable. What is the difference? What happened to Rawls's formulation in the 13 years between the two works?

There are two major differences. First, and most striking, is that in the 1958 paper, Rawls's second principle specified that inequalities, to be just, must work to *everyone's* benefit, while in the 1971 book, the principle has become one in which the inequalities, to be just, must work only to the benefit of the *least advantaged*. In Rawls's conception of the considerations of the rational persons assenting to, or evolving, such a principle, this implies a sharp shift in the decision principle. The 1958 principle was based on the conception of a rational person who saw himself equally likely to be in all possible positions in society in the future and who would accept an inequality if it benefited him in all these potential positions.³ The 1971 principle is based on the conception of a person who thinks only of the worst conditions under which he might find himself and looks only to what would most benefit him in those circumstances. The latter, "maximin" position is not what is ordinarily considered rational behavior under uncertainty (except for persons who are maximally averse to risk); the first is ordinarily so considered.⁴ Thus if Rawls's original formulation

³ Note that since each individual is considering himself in all positions, another, more usable, principle could be adopted, which looks like utilitarianism but involves no interpersonal comparison of utility. For each pair of social states (i.e., each pair of possible social actions), each person considers himself in each possible social position and determines which policy gives him the greatest overall benefit, then sums this over all these potential positions. This is very close to "the greatest good for the greatest number" but does not imply, as does utilitarianism, the comparison of benefit to one person with benefit to another.

⁴ The first (1958) person would in decision theory be considered a Bayesian, while the second is a "minimaxer."

had been used, it would have eliminated one of my objections to his theory.

A second difference between Rawls of 1958 and Rawls of 1971 is the full-scale development of the social contract perspective in 1971. In 1958, the conception was more nearly that these principles would evolve within institutions, as "rules of the game," because they fit the tastes of a rational person, who wants to be fair to himself under all future conditions, when he is confronted with the uncertainty the future holds. By 1971, this had become a full-fledged social contract for a society, and the book attempts even to go so far as to describe the character of the branches of government that would exist under the social contract. Thus what is a relatively benign process of rational actors working out for themselves the rules of the game which would insure fairness (or justice) within social institutions becomes a full-fledged social contract for the society as a whole. This transformation imposes a requirement upon Rawls, but one he does not meet: the requirement of specifying what rights, under the social contract, are to be held collectively and thus are capable of being used to implement the two principles.

In their original form, then, and in their original context, Rawls's two principles appear to have considerable merit and to be worthy of serious examination. They remain the possible forerunner of a well-developed theory of justice.

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COMMENT ON COLEMAN'S "INEQUALITY, SOCIOLOGY, AND MORAL PHILOSOPHY"

In his lengthy review essay on John Rawls's *A Theory of Justice* (*AJS* 80 [November 1974]: 739-64), James Coleman begins by suggesting that the discipline of sociology has mistakenly eschewed a normative approach, one which would address the relations between the individual and society; he then proceeds to review Rawls's theory critically, especially as it addresses the issues of "inequality," as an illustration of the normative orientation; he concludes his essay with an outline of a normative theory which presumably corrects the defects in Rawls's presentation.

In this comment I argue the following points: first, Coleman's representation of the general structure of Rawls's theory distorts rather severely the sense of what Rawls is about; second, three of the major features which Coleman singles out for comment and criticism are misinterpreted; and, finally, the alternative which Coleman proposes actually leads us back to the dilemma upon which Arrow's analysis (see Coleman's n. 2) "hung" welfare economics.

In order to pursue these points, I begin by presenting a summary view of the underlying structure of Rawls's theory. These preliminary remarks serve as a foundation for considering the three features of the theory which Coleman singled out for extensive commentary: (1) logical defects in the theory, (2) its focus on the issue of "inequality," and (3) the loss of individual sovereignty. In the concluding remarks, I will suggest the sense in which Coleman's alternative approach is quite inferior to Rawls's theory.

The Underlying Structure of the Theory

As the title of the review essay suggests, Rawls's theory has a three-pointed foundation, but the three points are social choice, moral theory, and justice. As Coleman correctly notes, Arrow's early work (his "impossibility theorem," see Arrow [1963] and Sen [1970]) showed that, unless we assume an objective "good," as utilitarianism does, it is not possible to derive a consistent social decision from the preferred choices of free and autonomous individuals, unless those individual choices are somehow consistent across individuals. Rawls's theory addresses this problem of social choice, while avoiding the dilemma which Arrow's theorem posed. By adopting a deontological, rather than a teleological, starting point, Rawls avoids presuppositions of ends or "goods" (these are left to a constructive process) and places the emphasis on procedures (see Rawls, pp. 85 ff.). Furthermore, the two devices which Rawls employs—the "original position" and the "veil of ignorance"—are used to provide a context of choice aimed at the basic structure of society—the choice of first principles, lexically ordered, and reflecting man's free and autonomous nature.

The second corner of the foundation—moral theory—can be viewed as the point of convergence between Kantian philosophy and the work of such structuralists as Chomsky (1972) and Piaget (1965). From Kantian philosophy, Rawls borrows the notion of an autonomous and noumenal self choosing those categorical imperatives which express one's true nature. Still, if we stay in the realm of philosophy, it is anyone's guess just what the categorical imperatives would be. Consequently, Rawls makes explicit use of the structural methods of both Chomsky (p. 47) and Piaget (pp. 461 ff.) to define them tentatively. These structuralists employ a three-

term paradigm for addressing the processes by which the individual constructs his world: (1) the natural (biological) organism, (2) maturation or development, and (3) the contingent physical and social environment. Chomsky's theory of grammar maintains that there *must be* innate (instinctual) principles of the mind which both limit and make possible the acquisition of language and knowledge. While social experience is necessary to bring the innate structures into operation, it is not sufficient to account for the incredible degree of organization which results. Piaget, using a similar methodology, has studied the evolution of moral judgments, from an initial state of heteronomy to a state of autonomy, an evolution which is quite independent of the social milieu.

Rawls's theory bears a striking similarity to these programs on several important grounds. First, there is an emphasis on process or growth, and this shows up in Rawls as an accent on procedures. Second, there is the hierarchy of principles whose vertical dimension runs from nature, at the bottom, to contingency at the top. A third similarity is the role which consciousness plays in the process. Piaget's notion of "conscious realization" (1926) and Rawls's "reflective equilibrium" (pp. 48-51) have quite similar meanings. The basic idea is that operational structures develop quite apart from conscious intention or choice. However, once a structure is well formed, it may become a tool in the service of conscious, reasoned activity; for example, in the "original position" mature and reasonable men are to choose consciously the noumenal grounds for their association.

The third foundation stone of the theory—the topic of justice—marks a departure from Chomsky and Piaget. In Rawls's words (p. 3), "justice is the first virtue of social institutions." This focus on social institutions reintroduces the complexities of social choice mechanisms with which the study of language or thought does not have to deal, but, given the foundation in moral theory, we can approach social choice as a constructive process according to principles of rightness. Unfortunately, by missing the constructive aspect of the theory, Coleman misreads it as leading toward bureaucracy and the loss of individual sovereignty. Rawls's idea of "strict compliance" (e.g., p. 8), for instance, means that once the grounds for justice are chosen by representative men and are publicly recognized, the members of society ought to be intrinsically motivated to abide by them; that is, once they have made their choice, we need to assume that they will live with it; otherwise their contract would be unstable and no higher level structures could be erected.

These three cornerstones define the basic outline of Rawls's theory. Overall, there is the emphasis on the reasoned choices of all men, producing the basis of a social contract. But, as Arrow's work indicates, the relationship between individual choices and social decisions is problematic.

To circumvent this problem, Rawls used moral theory and a structural approach. From moral theory he draws a set of normative guidelines, while structuralism provides the procedure for moving up and down between actual social arrangements, viewed as contingent structures, and the deeper nature of man as free and autonomous.

Coleman's Main Criticisms

Logical defects in the theory.—The first of Coleman's major criticisms is that "there is a defect in the logical structure of the theory" (p. 746). This assertion of a defect has four parts: (a) the hypothetical "original position" is a fundamental postulate; (b) the actual content of the choices made in this position is also a postulate; (c) since these are independent postulates, one should be deducible from the other; (d) Rawls justifies their connection by recourse to an a priori argument (something he rejected elsewhere) which is subject to empirical falsification. Coleman then proceeds to offer an example of choice in the original position which would not lead to the two principles, thus casting a shadow of doubt over the connection between the two postulates.

My first response to this accusation is that it demands more from the theory than was intended. In no sense did Rawls intend to produce a rigorous deductive system: his position is quite obviously tentative and procedural. One important effect of this is that it encourages the audience to review the evidence—for example, to consider the flaws in utilitarian and commonsense approaches—and decide whether or not "reasonable" men, choosing behind a veil of ignorance, would adopt these principles. While I find the arguments for the two principles persuasive, they are not logically necessary, nor are they intended to be. In point of fact, Rawls goes through an iterative procedure (see secs. 11, 13, and 46) whereby he adjusts and revises his statement of the principles. So Coleman has seriously misconstrued the mode of the argument, reflecting, I think, the biases of a positivistic orientation (see Coleman's comment on the "mistaken orientation" of sociology, p. 742). Because the nature of the problem is normative, introducing purpose and choice, the arguments are contingent and hypothetical: under these conditions what principles would reasonable men choose? As we go about offering guesses as to what the choices would be, we can engage in the circularity of "reflective equilibrium" with the ultimate end, that of discovering those choices which reflect our deepest nature. As Rawls suggests in his discussion of the Kantian interpretation (sec. 40, pp. 251 ff.), "the original position may be viewed, then, as a *procedural* interpretation of Kant's conception of autonomy and the categorical imperative" (p. 256, emphasis added). The procedure is to lead us to those principles from which we might construct

a just society. Thus Rawls had no intention of arguing from a context of choice to a determinate content of such a choice any more than Chomsky would argue that all speech is determined by innate structure.

My second response is directed at the issue of an *a priori* psychological argument. It is quite clear (in chap. 3, esp.) that Rawls does not employ such an argument, because it is foreign to the enterprise. For instance, he states, "The intuitive idea of justice as fairness is to think of the first principles of justice as themselves the object of an original agreement in a suitably defined initial situation" (p. 118). Then, immediately following, he says, "It must be shown, then, that the two principles of justice are the solution for the problem of choice presented by the original position. In order to do this, one must establish that, given the circumstances of the parties, and their knowledge, beliefs and interests, an agreement on these principles is the best way for each person to secure his ends in view of the alternatives available" (p. 119). The whole monograph can be read as an effort to establish, by reasonable justification, that the two principles which Rawls presents are those which would be chosen. There is nothing *a priori* about this.

There are several other flaws in Coleman's interpretation of the argument and its psychological nature. Rawls argues (see secs. 9 and 70-75) that a theory of justice (concerned with social institutions) is, at bottom, a theory of individual moral judgments and moral sentiments. Furthermore, moral philosophy is "Socratic: we may want to change our present considered judgments . . ." (p. 49) as circumstances change or when their regulative principles (deep structure) are brought to light. In the sections mentioned, Rawls describes the similarities between structuralism and his theory, and takes note of Piaget's research on the evolution of moral judgments. In this sense, the connection between a situation of choice and the actual content of a choice is supported by both a procedure and empirical research.

Finally, Rawls does in fact reject *a priori* psychological arguments, but in a context very different from the original position. The difference is a large one, so that one wonders whether Coleman, who fails to see it, understood Rawls's point. The sentence Coleman places in a footnote (n. 7) appears in a discussion of Rousseau's principle of limited toleration: members of the faithful, Rousseau supposed, would find it intolerable to live with those believed damned. Rawls rejects this *a priori* toleration argument, and thus the dilemma which it posed, on three grounds: it is dogmatic (a basic postulate, if you will) and not based on free or reasoned choice; it is arbitrary, since men do not ordinarily choose their faith, but inherit it; and it assumes, paradoxically, that common interests (among the faithful) lead to absolute conflict (with infidels). Now, it is part of the genius of Rawls's approach, based as it is on a hierarchy of levels

(see the discussion of stages, sec. 31), that we can find in it, at a deep level, common interests which help resolve or relativize conflicts which arise at the surface. In this sense, Rousseau's dilemma is posed at the surface, where both common interest and absolute conflict are based on accidents instead of reflecting our nature.

This relationship of the deep level to the surface, combined with the emphasis on procedure, is nicely illustrated in a footnote discussion of Braithwaite's study of morality and game theory (Rawls, n. 10, p. 134). Braithwaite shows how game theory (pure procedure) can be used to resolve a conflict between two musicians. However, as Rawls indicates, the solution reflects contingencies (advantages of playing a louder instrument) which the original position seeks to eliminate: the individual threat-advantage given by the accident of preferring one kind of instrument over another.

This sense of levels is quite relevant for my third response to Coleman's "logical defect" argument, specifically with reference to the example he offers, which contains an empirical falsification of the two principles. There are at least four flaws in Coleman's (pp. 746-47) example. It is *not* an example of choice behind a veil of ignorance, since the individuals are voting "early" in their lifetimes: under the veil, they would not know whether they were voting early or late. A legislative decision—equalizing tax burdens—is *not* an issue to be voted upon in the original position, but one appropriate for the third level (see the discussion of stages, sec. 31), where the veil has been partially lifted. The concept of a tax "burden" carries a negative connotation, one which requires a definition of the good before such a judgment could be rendered. Since the theory treats the "right" as prior to the "good" the connotations of burden would *not* arise in the original position. Finally, the example shifts subtly from considering social choice to individual preference. Before Coleman's example could falsify the principles, assuming that the issue would arise in the first place, he would have to show what collective decision would be made, *not* what an individual or representative individual might choose.

The focus on inequality.—Coleman argues that Rawls's theory is aimed at the question: "When is inequality justified" (pp. 746 and 749). While this interpretation is conveniently suited to Coleman's own interests, it is a misrepresentation of Rawls's aims on a number of counts. First of all, Rawls (e.g., pp. 3 and 11) indicates that his aim is to present an extended "contract theory" as a viable alternative to utilitarian doctrines which have dominated our thinking on social welfare over the past century. Now two of the major flaws of utilitarian doctrine are that it is indifferent to the initial status quo and that it seeks only an average or net social welfare at the aggregate level. Furthermore, it is against this kind of indifference to the relation between individual and social welfare that

Coleman's dilemma-for-Rawlsian-theory (pp. 750 ff.) is directed. So the dilemma is really addressed to the kinds of doctrines which Rawls seeks to correct.

Second, this interpretation of the aims of the theory could easily be misread to mean that the theory is a defense of the status quo, which it most assuredly is not. Surely the veil of ignorance, under which men know the general features of social structure but not their place in the society, cannot be construed as a status quo. Furthermore, the "priority principle" (e.g., sec. 8), according to which liberty is always prior to opportunities for, or division of, advantages, makes it quite clear that these basic liberties are the main subject of the theory. Thus, the priority rule would suggest that the principal aim is to establish equality (first principle) as prior to inequalities in the distribution of goods and opportunities (second principle).

Third, since Rawls provides a very patient and explicit articulation of the second principle (see esp. chaps. 2 and 5), there ought not to be any serious problems in interpreting it. Nevertheless, Coleman has created a monstrosity arguing that a Rawlsian society must either "pluck" the child from his disadvantaged home or "acquiesce" in its social responsibility—that is, allow the kind of free choice of neighborhoods which leads to a homogeneous composition and, thus, to de facto inequality of opportunity. Now, it is clear that the context of Coleman's argument is at the surface, reflecting inequality in educational opportunities in an existing society and our frustrations resulting from our inability to bring actual arrangements into line with our ideals. But the appropriate context is at the deep level, the original position.

Let us begin there, where we would not know whether we would be members of a minority or majority, young or old, and assume that we had chosen the final version of the second principle (p. 302) and were aware of the results of the research to which Coleman refers. Granting these simplifications, we would all choose to create socially heterogeneous neighborhoods and schools because, first, that would be "to everyone's advantage," and second, we take "unity of the self" and "self-respect" (for others as well as ourselves) as fundamental goods, goods that we would all want, whatever else we wanted.

In summary, then, it is a gross misinterpretation to read Rawls's aim as one of setting the just grounds for inequality. Equality is the first and inviolable principle chosen. Then, the second principle in this serial construction lays out a "just" basis for inequality of advantages and opportunity: they would, in a fundamental sense, be justifiable only if they are to everyone's advantage, as the discussions of the "difference" and "fairness" principles make clear. But this way of wording the second principle—as it applies to "inequality"—clearly separates the *need* for inequality

—for example, leadership offices—from the prerogatives to usurp privilege and prestige for one's own advantage. Furthermore, even when Rawls and Coleman both discuss inequality, it is clear (as the Braithwaite example shows) that their contexts of meaning are as different as are deep and surface structures. Finally, Rawls's theoretical structure is intended to serve as a framework against which to judge those arbitrary social arrangements and injustices present in a particular society, and to construct just replacements for them. The very notion of "plucking" the child from his home or "acquiescing" in the face of injustice runs contrary to this enterprise, because such a choice implies an "expert" observer-manipulator role, which, apparently, Coleman sees as relevant in the pursuit of equality.

The loss of individual sovereignty.—Coleman's third criticism of the theory is that individuals give up their sovereignty, with the corollary assertion that the theory would lead to a centralized, bureaucratic, and intransigent authority. A representative quotation from Coleman would be: "In short, in Rawls's theory, persons, via a social contract, give up their rights and resources and vest them in a central agency which, in a disinterested and dispassionate way, redistributes these rights and resources, including the basic liberties, to the members of society" (p. 752). Although it is possible, given the tentative nature of the theory and recent events in our own society, that these things might happen, they are rather implausible outcomes of Rawls's theory. They most assuredly could not be deduced either from the theory or from Coleman's inconsistent discussion of it. For instance, compare the quotation above with the following: "... Rawls's priority of the liberty principle would restrict the state from interfering with those choices [to form homogeneous neighborhoods], because that would reduce the liberty for all only to increase the equality of opportunity" (p. 753). If the central agency has the right to distribute rights, including liberties, how does it happen that the central agency cannot interfere with the right to choose one's neighborhood?

One of the more specific of Rawls's points, which is the obverse of Coleman's assertion of the loss of sovereignty, is contained in his treatment of "civil disobedience." The discussion of this and the related matter of conscientious refusal (see secs. 55–59) provides some interesting insights into how Rawls defines the relationship between fundamental cooperation and the more surface conflicts of interest. Disobedience is inherent in the system because: (a) no one must obey an unjust rule or law; (b) since justice has its roots in the consciences of reasonable men, members of a society can differ on what constitutes a just or unjust rule; (c) at the same time the basic structure of society provides procedures for resolving conflicts; (d) thus civil disobedience can be read as an appeal to the majority's sense of justice in that, in one's considered judgment, the

principles of justice and cooperation are not being respected, in some particular situation. Rawls concludes this discussion by saying: "The absence of a final (central) authority to decide, and so of an official interpretation that all must accept, does not lead to confusion, but is rather a condition of theoretical advance. Equals accepting and applying reasonable principles need have no established superior" (p. 390).

One possible way to account for the discrepancies between Rawls's theory and Coleman's interpretation is to suggest that there are defects in the mode of sociological thinking, at least when it encounters normative thinking, and that these defects reflect a surface bias due to empiricism. In several places Coleman describes polar types (e.g., pp. 747 and 755) and proceeds to suggest that Rawls's theory must be of one type or the other, but not both. For instance, he uses the terms "ascending" and "descending" to characterize two distinct conceptions of government—one individualistic, the other collectivistic. He then proceeds to characterize Rawlsian theory as collectivistic. But Rawlsian theory is both individual and collective in its fundamental structure. For instance, the whole of part 2 is given over to a discussion of institutions, with the emphasis on a constructive procedure, while the whole of part 3 is concerned with ends, with the emphasis on individual nature: moral conscience, plans of life, self-respect, and such motivational concepts as self-fulfillment, guilt and shame, and envy.

Beyond Coleman's misattribution, then, the interesting point is that he wants us to choose between these polarities as if they constituted extreme points on a physical axis. But in Rawls's theory the concept of "reflective equilibrium" and the notion of stages suggest that the theory requires that we ascend and descend in a two-way spiral of social construction, ascending to build or legislate new structures, and descending to the basic principles to check that these new structures are consistent with justice. This kind of symbolic examination obviates the kinds of physical polarities upon which Coleman insists.

A second example of this problem arises when Coleman refers to Parsonian "pattern variables." In arguing that Rawls is proposing a very special kind of society, he states that the society would have to be universalistic and achievement oriented. By implication, he also states that the society would have to be affectively neutral (see the quotation above from p. 752). Now the interesting thing about the theory, especially when we attend very carefully to its levels and stages (e.g., see sec. 31) is that it really involves all of these patterned alternatives. Thus, to suggest an interesting wrinkle, the basic contract, agreed to in the original position, would be universalistic-ascriptive in the specific sense that certain rights (primary goods) are *inalienable* for *all* men (and perhaps for all of nature). At the surface of society, on the other hand, the structure

of relationships would be particularistic and achievement oriented, in the special sense that men, considered as an aggregate, are mutually disinterested though they choose to associate on a limited basis, and unlimited in their potentials for pursuing the Aristotelian principle of self-development (sec. 65). In a similar line of argument, Rawls sees men as both affective and affectively neutral. For instance, "obligations" imply a powerful commitment (based on self-reflection and choice) to the basic, cooperative structure of society. But envy and resentments would not even exist, in the ideal case, since each man has his own "plan of life" and the right and opportunity to pursue it, subject to resource limitations and the two fundamental principles.

In summary, I trust that it is by now obvious that there is little plausibility to the assertion that Rawls's theory leads, inexorably, to central control and a loss of individual sovereignty. The only explicitly central feature of the theory is the idea of a basic social structure—a public sense of justice—based on the choice of free and sovereign individuals. In missing these points, Coleman has confused the deep structure with the surface structure.

Concluding Remarks

Toward the end of his essay, Coleman proposes an alternative starting point for the question of justice, and gives a brief outline of how his theory would work. In this final section I direct a few general remarks to his proposal and conclude with a specific criticism of his theory.

He begins by suggesting that Rawls's theory of justice was developed independent of general theories of society and of moral philosophy. The discussion above should make it clear that Rawls's work does reflect, and in fact is based upon, a general moral philosophy, but the question of a general theory of society is, perhaps, not so obvious. I maintain that there is a general theory of society (that was Rawls's explicit intent), so the question is "what kind of general theory?" rather than its presence or absence.

Rawls's work is in the tradition of "normative" science, which has the "aim of expressing normative principles for the organization of society" (Coleman, p. 739). As such, it is centrally concerned with human values and purposes, which give the science a quality of being artificial and oriented to design (e.g., Simon 1969). Thus, a normative science is oriented to what ought to be and seeks means and principles for designing and constructing social organization accordingly. In such a context, the role of the scientist is not exclusively that of an impartial observer seeking to describe empirical arrangements and their underlying lawfulness:

such surface structures are contingent and arbitrary. But the point is that Rawls's normative theory is general, not merely special, as can be demonstrated in two ways.

First, as Coleman himself notes, the normative science concerned with social welfare "has been hung on" the problem of "an interpersonal comparison of utility" (pp. 739-40 n.). This is especially true with regard to comparisons which assume cardinality. The crux of this problem is that utilities, satisfactions, and preferences are not objectively known, but reflect individual dispositions. In the light of this dilemma, Rawls's work represents a considerable advance in several respects. In the "original position" *men are to choose* the basic moral principles of social organization. One such choice would be "primary goods"—those things "which it is supposed a rational man wants whatever else he wants" (Rawls, p. 92). A second advance comes about through the "difference principle," which requires that " . . . the higher expectations (whatever these might be) of those better situated are just if and only if they work as part of a scheme which improves the expectations of the least advantaged members of society" (p. 75). Thus, by emphasizing both natural "goods" and procedural principles for distributing opportunity and advantages, Rawls has led us around the dilemma.

Second, neither of these concepts—original position and difference principle—provides specific meanings for utility or values, but both constrain the social construction process by, on the one hand, defining a basic moral standpoint, and, on the other, involving the pursuit of goods and interests in a *total system* (the connection between individual pursuits and the welfare of the least advantaged). Furthermore, the constructive process is a common cause, providing no special "impartial spectator" role to the sociological theorist or researcher: people are to choose and build, although their choices might be better informed by the provision of theoretical insights and surface descriptions. The fact that orthodox sociology has adopted a "value-neutral" stance and concerns itself more with what is measurable—static structure—than with what ought or could be, should not be used as a basis for asserting that Rawls lacks a general social theory. If anything, his theory may be more general than other social theories, because it is congenial to both structure and a structuring process; it integrates the individual and social processes; and it provides a procedure for discovering the natural basis for human association, one which is independent of historical accidents.

My second remark is concerned with two of the main concepts which Coleman employs as he lays out his alternative: "resources" and "power." While some resources may be physical objects, no resource exists independent of the values and purposes of social actors. Thus, if we try to

classify these things (e.g., as alienable or inalienable) and to trace out the social implications of the many possible "investment" patterns, we are going to run square into the problems of determining their meaning across individuals and finding some system of principles for guiding their distribution and effective use. These problems put us firmly back into the normative context.

As ordinarily used in sociological circles, the concept of power has two parts: the ability of some social unit to choose and produce a certain effect and the control over sanctions which can be manipulated so as to lead others toward the desired effect or to neutralize them. Now, viewed interpersonally, this concept involves a high degree of arbitrariness, not only in the choices made by the powerful unit, but also in the neutralization of others who might make contrary and connected choices. From a normative perspective, such a condition is highly undesirable for two reasons. First, the allocation of power is arbitrary unless we find natural (i.e., moral and just) grounds for such allocation. Second, to the extent that it is arbitrary it is not necessarily in anyone's best long-run interest, since, unless we assume that the powerful are endowed with the best survival strategy, power lessens the variety of choices that the social system has at its disposal. So, the point is that Coleman's proposal is purely formal and does not provide a basis for deriving "natural" meanings for either power or resource. Rawls's theory, on the other hand, appears to handle these problems quite well—for example, the difference principle.

My specific criticism of Coleman's alternative theory is that it really leads us back to Arrow's "impossibility theorem." In a sense, what Coleman proposes is one kind of surface-structure reflection of Rawls's deep-structural theory of justice and morality, but without Rawls's work it is an empty hypothesis. For instance, Coleman argues that there are two protections against the arbitrary exercise of authority: the plurality of corporate actors and the possible "convergence of expectation streams." But the antagonism between pluralism and convergence is precisely the problem which leads to Arrow's analysis and conclusion, which I summarized at the beginning. If we cannot discover some individual (natural) basis for both pluralism and expectation streams, as Rawls has attempted to do, Coleman's alternative is an empty formalism fencing with all possible worlds. But, to close on a positive note reflecting the spirit in which the entire comment is intended, it is entirely conceivable, since Rawls's strengths are at the deep level and Coleman's, as representative sociologist, are at the empirical surface, that the two positions are vertically complementary.

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Review Essay

Ethnicity: Theory and Experience. Edited, with an introduction by Nathan Glazer and Daniel P. Moynihan. Cambridge, Mass.: Harvard University Press, 1975. Pp. ix+531. \$15.00.

Irving Louis Horowitz

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This volume is one of several riding the crest of the current revival of interest in ethnicity. Given their joint collaboration in the past on a standard work, *Beyond the Melting Pot*, it makes perfectly good sense for Messrs. Glazer and Moynihan to team up again. That the result is a mixed blessing perhaps reflects the present state of ethnic studies as much as any particular weaknesses in the 16 contributions to this volume. The fact that the volume is based on conference proceedings adds to the reviewer's difficulty. One might say that the parts are greater than the whole. Indeed, it is the whole that I had the greatest difficulty with, in the form of the group of essays entitled "Toward a General Theory." In the field of ethnicity it is easier to talk of specific linguistic groupings and national minorities than to deal with the conceptual problems of yet another large social stratification variable.

The keynote paper by Glazer and Moynihan is less candid than the brief acknowledgments page, which admits that "we are all too aware that the conference and the book that followed drew heavily from the banks of the Charles River, and that many other persons unrepresented in this book except in footnotes have been working for many years in developing our understanding of ethnicity" (p. vi). Were this recognition of limits carried over to the collection itself, the results might have been better. It is inconceivable that nearly every paper is written by a Harvard professor, or by persons who (with one or two exceptions) have been associated with that citadel of learning as visiting potentates. The assumption that all knowledge about ethnicity can be located at one university is breathtaking, reminiscent of Kipling's concept of the British empire: that only those who speak English can be civilized and all others are heathens. Glazer and Moynihan's choice of representatives is indicative of serious problems in social science research: as if one outpost of the intellectual empire were uniquely endowed to speak on the subject of ethnicity. Fortunately, the welter of materials on ethnicity now pouring forth provides the necessary corrective and positive reprimand to such educational imperialism.

All could be forgiven if the introduction set the tone for what follows. In point of fact, it sticks out like a sore thumb. Unquestionably the least

successful paper in the volume, it is conceptually fuzzy: we are promised a theory of why ethnic identity has become more salient and self-assertive over time, only to look in vain for such a theory. Indeed, even the editors do not appear to be certain why ethnic identity has come into its own at this point in time. They resort to a connotational caveat, claiming that they neither celebrate nor dismiss ethnicity. But the problem is that they also neither explain nor analyze it.

They do assert the following: First, ethnicity is strategically efficacious as an organizing principle. But they do not indicate whether the strategy has been evolved by ethnics themselves, by social scientists doing the analysis, or by community organizers in search of new modes of protest. This problem crops up throughout the literature on ethnicity, but it is especially apparent in many of these papers. Second, Glazer and Moynihan propose that the world is not really developing but that we are witnessing the refeudalization of society. While I agree that there is a return to ascribed status as against achievement status in social stratification, why this constitutes feudal behavior is not made clear. It sounds more like an effort to avoid the fact that socialism, specifically in its Leninist approach to the national minority question, has co-opted the rhetoric of nationalism and ethnicity better than either the melting-pot monism or ethnic pluralism of capitalism.

The most astonishing claim is made at the outset: that ethnicity is a "new word," or at least has a "new usage." Furthermore, the editors assert that there is no possible hope of doing without ethnicity in a society as subgroups assimilate to majority groups. The implication of these remarks, if Glazer and Moynihan are right, is that "a very great deal of radical and even liberal doctrine of the past century and a half is wrong" (p. 5). But what in fact characterizes a good deal of 20th-century radical and liberal doctrine alike is a competitive effort to appeal to and solve ethnic problems through a differentially interpreted notion of the rights of all peoples to national self-determination.

Even the assumption that their viewpoint is unique is hyperbolic: some dictionaries of sociology do not refer to "ethnicity" or "ethnic groups," and one rather impressionistic dictionary of sociology defines only "ethnic group," not "ethnicity." However, this casts doubt on the merit of dictionaries of sociology, not on researchers. I can hardly believe that a volume produced in 1975 would ignore the monumental contribution of Tomatsu Shibutani and his coworkers in *Ethnic Stratification: A Comparative Approach*, published in 1965. All 626 pages are dedicated to the concept of ethnicity in exactly the sense described in the present volume. Then there is the 1945 work by W. Lloyd Warner and Leo Srole entitled *The Social Systems of American Ethnic Groups*, which also goes far beyond the concept of ethnicity as a subgroup. It seems to me that the view

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REPLY TO CONDON

I will not respond extensively to Professor Condon's comment. Perhaps the reason is that it arrived after I had already replied at length to Klees and Strike's, and after I had just finished a lengthy review essay on Robert Nozick's new book (*Anarchy, State, and Utopia*),¹ in which Rawls is once again discussed at length. I have little more to say at this point about Rawls's book. But perhaps the reason is that I find it difficult to respond to the comment, since Condon's reading of Rawls and mine are very different. He sees all sorts of levels in Rawls, he understands social choice differently than I do, and he uses words and phrases which are strange to me and far from those I find in Rawls.

Condon argues that my misunderstandings of Rawls are explainable because I am a sociologist and Rawls is a moral philosopher. It is *this*, he suggests, which makes me see Rawls as collectivistic, Rawls's society as centralized and bureaucratic, and it is *this*, he feels, which underlies all my criticisms of Rawls. But Robert Nozick, to whom I have already referred, a moral philosopher in Rawls's own department, has made in his book most of the same criticisms of Rawls that I make. *His* interpretations of Rawls's work as violating individual rights and treating all resources as centrally held can hardly be due to his surface, empiricist, sociological bias. They are due instead to his conception of the proper construction of a theory of society: a set of individuals with natural inviolable rights who choose interdependent or cooperative activity. Whether Nozick's own theory is adequate or not (and as I indicate in my essay, I believe it is not), his criticisms of Rawls that parallel my own are in a long tradition of moral philosophy, beginning perhaps with Locke.

¹ The review is to appear in a subsequent issue of this journal.

Just one comment on the logical incompatibility of Rawls's two starting points: as a number of commentators have noted, the choice of Rawls's difference principle under a veil of ignorance follows only if the choosers are pursuing a minimax decision rule. Most decision theorists would agree that this is not rational except for those who are maximally averse to risk. Thus if Rawls's rational actors were any other kind of persons, they would not choose his difference principle under a veil of ignorance. But even this presupposes that the task of decision under the veil of ignorance is a very narrow one. As I pointed out in my essay on Rawls, under an appropriately wide range of choice, rational actors will not choose how goods are to be allocated (Rawls's difference principle), but rather will answer a prior question: they will choose what set of rights to give over to the collectivity (thus determining what rights the collectivity has over their actions or their resources) and what set of rights to reserve to themselves as individuals.

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COMMENT ON BRANDL'S REVIEW OF *UNRAVELLING SOCIAL POLICY*

If a scientist wrote a book on chemistry and arbitrarily selected uranium ore to illustrate a method for chemical analysis, should a reviewer conclude that the author advocated the use of atomic energy? If a psychologist selected a dream involving murder to illustrate a method of dream analysis, should his treatise be considered a case for murder? Were one to follow the logic of Brandl's review of my book *Unravelling Social Policy* (*AJS* 80 [November 1974]: 820-22), he would have to answer yes to these questions. The book is an attempt to develop a conceptual model of social policies and to derive from it a framework for policy analysis and development. The use of the framework is then illustrated in a detailed analysis of a cluster of complementary and alternative social policies; mothers' wages, children's allowances, and parents' wages. According to Brandl's review, however, the book is a medium for advocating mothers' wages.

Brandl disregards the major thrust of the book, the theoretical propositions in part 1, according to which social policies in any human society reflect choices, constrained by prevailing dominant-value premises, concerning the selection and development of resources, the division of labor

or allocation of statuses, and the distribution of rights to concrete and symbolic goods and services—choices which shape the living circumstances of individuals and groups, their relations to each other and to society, and the overall quality of life in a society. Since, in accordance with this conception, policy choices deal primarily with allocative and distributive processes, a society's position concerning the value dimension of equality-inequality is considered to be of crucial importance. Brandl should have noted these and other theoretical propositions and examined their validity and utility for policy analysis and synthesis instead of merely observing that the book does not produce "a comprehensive theory for the study of public policy formation." Brandl suggests that "the framework of questions can be useful for organizing analysis." He adds that "it simply is not a theory." Nowhere in the book did I claim that the framework is a theory. What I claimed is that the analytic framework is derived from theoretical propositions concerning the nature and dynamics of social policies, rather than constructed inductively, or even randomly, without a basis in theory. This claim should have been examined critically by the reviewer.

Having disregarded the theoretical aspects of the first part of the book, Brandl erroneously presents it as "a case for mothers' wages" which "comes across as arbitrary." Part 2, in which the mothers'-wages and related policies are analyzed, is entitled "An Illustration of Policy Analysis and Synthesis." It is in no way a brief for or against these policies but a systematic analysis of their likely consequences throughout society. Hence the fact seems irrelevant that the mothers'-wages policy and other policies examined in that section are not consistent with the author's commitment to an egalitarian social order. Advocacy is viewed in the book as a process separate from analysis. Decisions on advocacy should derive from comprehensive, systematic analysis of objective aspects of a policy and from an evaluation of these objective aspects in terms of one's subjective value premises. Since analysis indicates that mothers' wages would not overcome social inequalities but would merely reduce them, this policy would not be advocated, as a final goal, by individuals committed to an egalitarian social order, although they might consider such a policy a desirable interim step. These issues are explored in the book.

Brandl also disregards the political epilogue of the book, an exploration of the implications of the theoretical propositions for egalitarian social goals and strategies toward these goals. Thus the book will have to speak for itself on this subject.

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from Harvard is the problem. Were there a view from Chicago—Robert Park, Ernest Burgess, Louis Wirth, or some of their legendary students—it would become apparent that work on ethnicity, as well as the concept itself, is by no means a recent invention.

Glazer and Moynihan miss a perfect opportunity to show that the conservative tradition, linked to German conflict theory, understood ethnicity better than either the liberal or radical tradition. The whole of *Foundations of Sociology*, written by Ludwig Gumplowicz in 1885, is oriented to the concept of ethnicity in precisely its present national, secularized usage. The problem with *Ethnicity: Theory and Experience* is intellectual overkill: Glazer and Moynihan are more interested in establishing the uniqueness of their argument than in probing what is known about ethnicity in the modern period. Ethnicity has emerged as a variable equal to class and race, but the argument that it is new depends ultimately on the contents of this and other books. Curiously enough, the introduction is written as if the rest of the book were a foothill instead of a mountain. In view of the welter of interesting theoretical observations and ethnography in the evidence, it is unfair to reduce an assessment to one paper or to prejudice a review of the troops because they all came from the same place.

The paper by Harold R. Isaacs is characteristically well written and refreshingly free of jargon. But I am not quite sure that he captures the spirit of the book, for his notion of ethnicity reduces ultimately to tribalism, precisely the older use of the term "ethnic groups." Parsons attempts to overcome this shortcoming. His paper has the singular merit of showing that ethnic groups are far more than tribal instincts or impulses, but that ethnicity can at one and the same time be a form of personal identification, national mobilization, and universal myth. In this sense, Parsons serves his systemic scaffold well by integrating the concepts of ethnicity into his own general theory of societal evolution. The paper by Milton M. Gordon supplies an excellent typology relating ethnicity as a dependent variable to class and race, and to independent variables of ideology, power, and the political nature of society as a whole. No indications are given as to how one measures such things as "intermediate trends of power" in an "egalitarian, pluralist society," but the sentiment is certainly noble. The essay by Donald L. Horowitz (no relation) is less a statement on ethnic identity than one on comparative ethnic identities. Its main concern is to show that the general effect of nationalist amalgamation was to superimpose identity and that the ethnic problem is a function of societal layers being imperfectly integrated. The notion that international regional integration leads to amalgamation whereas nationalism drives toward separation is neat and seems testable. However, Horowitz ignores the ways in which ethnicity may function precisely as a transnational cohesive factor. The final paper in the theory section, by Daniel Bell, is provocative. It has

the advantage of showing that ethnicity is an aspect of the fusion of status order with the political order, while class is a dimension of the relationship of the economic order to the political order. I am not sure that Bell's case for ethnicity as a kind of primordial attachment to those who are *Gemeinde* in order to create a group defined as *Fremde* can be empirically confirmed. There is a good lesson to be learned in any purely sociologicistic account of ethnicity that minimizes the psychological propensities that underwrite many sociological variables. However, the caveat "toward a general theory" does not hold up. It would be more accurate to say that these papers offer five different models in search of a general theory.

The best portion of the book unfortunately draws the least attention from the editors, perhaps because it is the least controversial. The papers by William Peterson, "On the Subnations of Western Europe," and by Andrew Greeley and William McCreedy, "The Transmission of Cultural Heritages: The Case of the Irish and the Italians," are both carefully done. The regional papers by Orlando Patterson, "Context and Choice in Ethnic Allegiance: A Theoretical Framework and Caribbean Case Study," and by François Bourricaud, "Indian, Mestizo, and Cholo as Symbols in the Peruvian System of Stratification," are really fine. I have some reservations about Martin Kilson's paper, "Blacks and Neo-Ethnicity in American Political Life," and John Porter's, "Ethnic Pluralism in Canadian Perspective." Each author in his own way missed the opportunity to talk specifically about ethnic diversity within dominant nationalities. Kilson deals only with the black experience as an aggregate mass serving as role model for newer white ethnics. While blacks do so serve, it is somewhat disappointing that he does not deal with the sorts of questions of ethnicity within black life that divide Jamaicans, Indians, East Africans, West Africans, third-generation blacks, and first-generation blacks within the United States, by income and occupation. Porter's paper is dismaying because, as far as I am concerned, it marks a step backward from *The Vertical Mosaic*, his classic book on Canada written in the mid-sixties. One would hope that Porter, having turned his attention to French and English groups, would at least provide explanation of problems of exploitation as he did in the earlier work. What we get is short-run problems and long-run optimism. The overemphasis on linguistic problems at the expense of economic ones means that Porter simply has not taken seriously criticisms of his work by fellow Canadians.

The final section, "The New States," concerning actually the Third World and old empires, again has some excellent papers. Particularly noteworthy is the study by Mazrui of ethnic stratification in Uganda. As he does in so much of his work, Mazrui introduces and infuses his efforts with extremely interesting theories by dropping phrases like "Military-Agrarian Complex." His analysis of ethnicity as it reflects itself in the military

makes for refreshing and unusual emphasis in the volume. The brief paper by Richard Pipes is a bit too impressionistic. Written in an *ex cathedra* manner, it might have benefited by the sort of careful work done by Teresa Rakowska. The paper by Lucian W. Pye on ethnic minorities in China is sound, but it would have been enriched by an analysis of how the Chinese-Communist regime is dealing with precisely the problem of ethnicity and linguistic minorities. He speaks of a growing sense of "Chinese anxiety." However, he does not deal with the corresponding efforts of the Chinese government in policymaking at this level. The work of John Lum would be especially important in this connection.

It is hard to summarize this book and a mistake to dismiss it. There is that irritating cocksureness that pervades much of the writing: a kind of genteel conceit that all is right with the advanced world, and slightly less so with the Third World (a phrase, by the way, remarkably absent in this collection). There are only two papers on "the new states." India and China are cast with "the old empires," while the Caribbean and Peru are somewhere in limbo between "the old world and the new." In part, this geographic timidity stems from the fact that so few pieces are written by ethnics themselves. I do not want to get into a metaphysical dispute over *verstehentheorie*, the argument that "you have to be one to study one." These debates are irrelevant to the main point: a good deal of the literature is simply blocked out because of ethnic bias. Even a scholar like Porter displays little first-hand knowledge of the French-Canadian literature. Bourricaud takes only perfunctory account of developments since his 1967 book, *Pouvoir et société dans le Pérou contemporain*. The work of Julio Cotler and Anibal Quijano does not raise even a faint whisper. The same lack of being *au courant* is evident, but to a lesser degree, in the work of Pye. With such a subject as ethnicity, one would have hoped to elicit the contribution of more of the people directly involved as participating subjects, no less than objects of analysis. Alas, this is not the case; and, alack, this is the ultimate shortcoming of the book.

Innerworldly Asceticism and American Foreign Policy: A Review Essay

Redeemer Nation: The Idea of America's Millennial Role. By Ernest L. Tuveson. Chicago: University of Chicago Press, 1968. Pp. 238. \$10.00 (cloth); \$3.25 (paper).

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Max Weber wrote *The Protestant Ethic* a decade before he stated his general argument concerning the role of religion in social change. While many are familiar with the earlier thesis and its ensuing controversy, fewer recognize that the thesis concerning the Protestant Ethic is properly seen as an *illustration* of a more encompassing view of religion and social change. Briefly put, the larger proposition states that widespread social change is likelier if persons are led to regard their earthly existence as (a) important and (b) an occasion to improve things. This point of view Weber called a theodicy of "innerworldly asceticism," the second term coming from the Greek "to exercise (control over)," with the first term implying that such exercise is to be carried out in ordinary mundane activity. Social history, given this perspective, is a sequence of institutionalized behaviors subject to periodic interruption; if the interruption leads to a more inner-worldly ascetic theodicy, the social impact of the interruption will be greater. These interruptions may very well be material forces, but before they change social behavior they must be meaningful, thought about, or otherwise dealt with mentally. Theodicies are relevant, therefore, because they are often invoked and/or modified as meanings change. Weber regarded these meaning changes as a proper focus of sociology.

Later efforts to "test" the Protestant Ethic thesis by comparing Protestants and Catholics ignore, therefore, Weber's own warning of 1905 that the capitalistic spirit no longer needs the support of a "caged" Protestant Ethic. They fail also in not investigating differing theodicies. Not only is a single illustration of a theory misread, in other words, but so also is the line of reasoning making up the theory.

So it is that studies believed to bear on the Protestant Ethic thesis routinely report negative—but actually irrelevant—evidence, and studies that truly bear on the Weberian perspective are rare indeed. One of these rarities is Ernest L. Tuveson's *Redeemer Nation*, a book all the more remarkable because the author, a professor of English, gives no clue that he even had Weber in mind while writing it. In this peculiar sense, the book may be regarded not only as another "illustration" of the Weberian

social change theory but as an "independent" test of its utility. It ranks in quality and approach with David Little's *Religion, Order and Law* and Benjamin Nelson's *The Idea of Usury*, for all three brilliantly trace histories in which changes in theodicy encouraged changes in institutionalized behavior.

The behavior in this case is American "expansionism," the rather consistent outlook of 300 years that this nation is "chosen" to show the way for all humankind and thus is engaged in a continuing war between good and evil.

Now, it is not at all obvious that the person or nation with a monopoly on virtue is thereby obliged to redeem others. A real alternative is to seek isolation (join a monastery), that is, be "innocent in a wicked world." In Weber's terms, it is possible to be ascetic without being innerworldly, in which case one concentrates on controlling oneself and ignores as much as possible the full range of ordinary, worldly points of contact. Isolationism, from Jefferson onward, has held periodic appeal for America.

But the major appeal has been otherwise—interventionist, not isolationist; imperialistic, not withdrawn. America not only has regarded the Old World as corrupt and the New World as innocent, but has felt compelled to "redeem" that Old World. Why?

Tuveson, of course, is not unmindful of economic and other materialistic forces that have pushed Americans beyond their borders. Indeed, he may be too willing to concede the point: ". . . millennialist ideas probably did not inspire the greatest decisions of our nation simply by their own power. The expansion of the nation, the Civil War, the entry into the Second World War—all would have occurred in the course of things" (p. 213). What he does contend is that the history of American foreign policy cannot be understood without understanding the ideas by which participants saw their actions as meaningful. And those ideas were largely innerworldly and ascetic.

These are Weber's and not Tuveson's labels. Tuveson extracts more proximate terms—"apocalyptic Whiggism"—from colonial documents in which two themes predominate: "The balance of powers (idea) . . . is rooted in Renaissance political philosophy and is a feature of Whig thought. . . . It is surely secular enough. But the menaces [the documents refer to] are the whole collection from the Revelation; the man of sin is a diabolical figure quite alien to Lockean philosophy. The combination might be called apocalyptic Whiggism; it is the prototype of what was to be perhaps the central American attitude toward government" (p. 24).

Surely it is no exaggeration to see in apocalyptic Whiggism the dual inclination that Weber called innerworldly asceticism—to take this world seriously and improve upon it. Of course, the term reflects its own time and space, but "it is not hard to extend the theory: Jacobins are only the

old Papists writ in different script. It will prove equally easy to fit other menaces, as they emerge, into the same pattern—the slave-trader, the ‘Hun,’ the Bolshevik” (p. 113). Some of the clearest expressions of the view are to be found in Woodrow Wilson’s efforts, in his final speeches, to persuade the opponents of ratification of participation in the League of Nations. We must “see it through to the end and make good . . . [the] redemption of the world,” Wilson pleaded in Pueblo, Colorado. “America,” he said in Cheyenne, has “the infinite privilege of fulfilling her destiny and saving the world” (quoted on pp. 211–12). In short, even if the phrase “Manifest Destiny” did not appear until 1845, the idea behind it has long pervaded American thinking on its place in the world.

What is the source of this idea, according to Tuveson? Note that the question he faces is not, “What produced American foreign policy?” but is, instead, “Why has the dominant thinking about American foreign policy been apocalyptic Whiggism rather than something else?” The answer sounds straight out of Weber:

The crucial change came, I believe, with the reversal of the Augustinian interpretation of history, which had prevailed during the Middle Ages and the Renaissance. Augustine assumed that the City of God, the mystic body of the faithful, must live, usually subject to some degree of persecution, separate from the world of action . . . until the Last Judgment would roll up history itself. . . . [With the Reformation] a new examination of the whole Scripture occurred. . . . Perhaps the millennium was to be an earthly utopia, an age at the end of all history, in which, not Christ in person, but Christians and Christian principles would really be triumphant. . . . The old conception of a “chosen people,” called to fight the battles of the Lord, was revived. . . . [N]ow it appeared that God must use peoples, armies, governments, to attain his ends; God had re-entered secular history as a participant. [Pp. ix, x]

Tuveson thus traces the manifestations of apocalyptic Whiggism, especially during the century before and the century after the Revolutionary War. Finding mere expressions of it is not difficult, of course; educational and political spokesmen are as likely to speak the “chosen people” rhetoric as are clergy and literary figures. A national history that contains a Mark Hopkins, William McKinley, and Woodrow Wilson does not need its millennialist thinking embellished by a Timothy Dwight, Alexander Campbell, or Herman Melville. But America’s was; and the question is the degree of impact post-Augustinian thought has had on “common-place” events in American life.

On this score, Tuveson, while not involved in a systematic content analysis, is acutely aware. He not only cites elitist speeches; he also shows that millennial utopianism fairly oozes around everyday social structure. It is one thing to find Revelationist symbols in Edward Hicks’s paintings of lions and lambs living together in “The Peaceable Kingdom”; it is

another to see importance in the popularity of those paintings (p. 61). It is one thing to recite the long list of 19th-century theologians who embraced a millennialism; it is another to note that "this kind of thinking about history reached the common man as, it is safe to say, that of Herbert Spencer or Henry Thomas Buckle never did" (p. 54). It is one thing to recognize the numerous images drawn from the Book of Revelation contained in "The Battle Hymn of the Republic"; it is another to recognize the circumstances of its composition. Julia Ward Howe, a friend of Emerson and Oliver Wendell Holmes, was a religious liberal with no faith in special revelation or religious mysteries. Brooding over a visit to an encampment of the Army of the Potomac, however, trying to understand and then convey the meaning of "this darkest moment of American history," she found that the strict Puritanism of her childhood "rose out of her deep memory." Tuveson writes: "Her experience, I suggest, is typical of what has happened to many other Americans. When urgent and baffling questions about the right course for the nation have arisen, the apocalyptic view of its history has come to the front: at such times as the expansionist eras, the Civil War, the First World War" (p. 199).

It is precisely its demonstration of such ways by which the "culture" of apocalyptic Whiggism influenced American "social structure" that makes Tuveson's book a valuable component of the Weberian library. Probably, anti-Weberians will not be convinced; it is, to repeat, more an illustration of Weber's perspective than "proof" of it. But Weberianism, it seems, is more a matter of taste than a theory to be proved, so perhaps being a tasty delight is warrant enough. It was Marx who pointed out, years before Weber, that Luther "shattered faith in authority because he restored the authority of faith. He turned priests into laymen because he turned laymen into priests." But it was Weber who saw this change as innerworldly asceticism and who insisted that the consequences of this change were huge and varied. *Redeemer Nation* is a welcome addition to the literature documenting just how insightful Weber was.

Book Reviews

The Broken Covenant: American Civil Religion in Time of Trial. By Robert N. Bellah. New York: Seabury Press, 1975. Pp. xvi+172. \$7.95.

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Robert N. Bellah, who first made his name as a sociologist who focused on Japanese religion, made a second reputation for himself when, in 1967, his essay on the concept of a "civil religion" in America stimulated a debate that has continued into the present. *The Broken Covenant*, based on the Weil Lectures of 1971 but revised to include comment on subsequent events, is a collection of essays on a range of themes, all of them footnoting or enlarging upon that "civil religion" idea.

Here there is nothing of the neutral and objective social scientist engaging in cool empirical studies. Those who criticize academics for their attempt to be value free will find no reason to attack Professor Bellah. His tone throughout is sermonic, elegiac, and marked by deep commitment to what he calls "*the* American tradition" and the dreams it has embodied. From one point of view, the book stands in the tradition of the old New England jeremiad. The prophet mounts the pulpit, recalls the prophecies and the covenants, and finds everything in his day wanting. From another angle, it is a regretful meditation by a disappointed lover, for Bellah only six years before the Weil Lectures (when he first made an oral presentation of the "civil religion" theme at a study group of the American Academy of Arts and Sciences) was full of optimism about the covenant.

At that time he argued that America had a full-blown, developed, institutionalized national faith that was only in part derivative of and was in some ways competitive with church religion. In the spirit of the Kennedy inauguration address, he was dreaming dreams of the fulfillment of the American tradition. True, he saw ominous portents. Kennedy had been assassinated; the Johnson administration was beginning to fall apart as the Vietnam War escalated and urban riots became common threats. Could the churches continue to feed into the national faith? As theologians talked of the death of God, Bellah wondered where the sense of "transcendent justice" would be renewed. But still there was a sense of intactness to the tradition and much hope.

In the Weil Lectures everything is changed. No Calvinist preacher, scolding a congregation for having original sin and acting upon its predispositions, portrays a bigger gap between divine intention and human reality than does Bellah. Throughout, his is a language of virtue lapsed, morality forgotten, covenant broken. While the lectures show a sensitivity to American Indian and black peoples' aspirations and frustrations, for him "*the* American tradition" has been embodied chiefly in the white

communities of Protestant provenance, in the New England and Virginian founding fathers; in the mainstream literary line, the Melvilles and their kind; in the political tradition in times of testing. (Curiously, while the Kennedy inaugural address was his prime document in the essay of 1967, he now lists only Washington, Jefferson, and Lincoln as successful articulators of the covenant. One detects a slight sense of embarrassment here, a backing away from his overstatements in the years between the New Frontier and the Great Society. In an essay not included in this book, Bellah has even remarked that had he had the Nixon second inaugural in front of him, the new essay would have been vastly different.)

Is Bellah's religious interpretation of America, then, wholly captive to the zeitgeist, the mood of the moment? Certainly it is colored by it, for he is a sensitive person whose manner of living, speaking, and even writing makes it impossible for him not to be responsive to change. His antennae are always out, and his nerve endings are exposed. Sometimes he could be faulted for a dependence upon the current mood. Yet through it all there is a consistent intellectual framework involving a structure of promise from the past that might be lived into but that will ordinarily be denied and "broken."

Three times the covenant has been tested, the nation has been tried. First, in the Revolution; second, in the Civil War; third, now, in the top-to-bottom moral breakdown and loss of virtue. Bellah's identification with the Puritan sense of order is very strong. Yet he would not see order incarnated in law-and-order regimes but in a very subtle, if eventually drastic, national conversion and reawakening to its covenant.

So far as program is concerned, Bellah is mild and timid. Having surveyed, in an essay on the taboo against socialism, the corruption and enduring materialism in the economy, he advocates an alternative. But no leftover robber baron would quake in the presence of Bellah's rhetoric: he is not sure whether a form of "decentralized democratic socialism" would provide a better base, but he *suspects* that it can; he does not want radical revolution, precipitate shifts—he wants people "with all due care and deliberation" to draw up proposals of far-reaching scope. His programmatic appeals die the death of a thousand courteous qualifications and mufflings. In 1971, such a voice of civility was certainly to be welcomed, given the volatility of American society and the threat of violent rupture. Now the nation has become quiet and morose; clearer proposals could be argued with more force. One suspects that Bellah has little to offer in this respect. He is very much the preacher who believes that the only hope for salvation is in the changed heart, the quickened conscience.

This is not a book for historians, sociologists, and theologians to read in order to improve their methods, find new areas for empirical research, or promote alternatives. It seems designed more for their time in the sanctuaries than in the studies, more for their hearts than their heads. Yet if professional scholars do not take pains to mourn the breaking of covenants, the amateurs will take over. Among them are people of good will and great intelligence; but many others seek repressive solutions to the prob-

Iems Bellah explores. His is a welcome if rare alternative to the demagogic voices that are always looking for the occasion to be heard.

The Divided Academy: Professors and Politics. By Everett Carll Ladd, Jr., and Seymour Martin Lipset. New York: McGraw-Hill Book Co., 1975. Pp. xv+407. \$17.50.

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The Divided Academy is a major contribution to academic self-analysis. It does what the best in social science should do—confirm some things we already know, disabuse us of conclusions that were never true, and inform us about the nature and dimension of emerging problems. The empirical base of the book, in addition to a thorough grounding in past research, consists of two huge surveys sponsored by the Carnegie Commission on Higher Education. They sent out 100,000 questionnaires, ending up with over 60,000 usable ones, and were thus able to get reasonably large samples of all sorts of academic institutions and disciplines.

American academics are on the left. By how much? We gave Adlai Stevenson 18% more votes than the rest of the population and George McGovern 17%. While in 1970 only 14% of the general population favored compulsory busing to achieve racial integration, in the professoriate over three times that proportion favored that policy, though no other largely white group in the population came close. Even in regard to campus events—and faculty members are more conservative in dealing with their own affairs—they occupy a more permissive position than the rest of the population.

Why are we academics so critical? Ladd and Lipset, following Veblen, argue that being intellectual means being skeptical and that being a theorist means being concerned with structural rearrangements according to broad principles rather than following experience on a case-by-case basis. Why, then, was natural science once the radical field that social science is today? "A field of study becomes highly ideological when, under a given set of circumstances, it offers a fulcrum for the rejection of established social arrangements; and from the 17th through the 19th centuries, natural science occupied this position in different countries at varying times" (p. 73).

Academy and discipline far outweigh hearth and home. College freshmen may have political views close to their parents', but by the time they are seniors they often do not. In general, class-based views of academia fare poorly. For one thing, academic positions have high status in the population. For another, those who are least satisfied with their position in the academy are the most conservative. "Uniformly," Ladd and Lipset write, "*those who are the most satisfied show up as the most liberal and critical in political orientation*" (p. 83). The higher up one goes in the social sciences, the better paid and more secure the professors, and the

more elite the institution from which they come, the further to the left they are in politics. Ladd and Lipset reanalyze the major study of sociologists by Alvin Gouldner and Timothy Sprehe to show that "in fact, the largely unpublished data . . . reveal a discipline disproportionately left-of-center in its politics, socially critical, highly activist, convinced its role involves action to promote social change" (p. 111).

As we all suspected, social scientists are the most liberal, with people in humanities and the law next; physical and biological scientists are in the middle together with people in education and medicine; engineering, business, and generally applied professional schools such as agriculture contain the most conservative people. How do we explain the fact that psychology, anthropology, and sociology stand considerably to the left of economics and political science within the social sciences? Ladd and Lipset argue that the disciplines on the left deal with individuals and groups under severe stress, whether the topic be individual deviance or mental illness or race relations or delinquency, while those comparatively to the right are necessarily more concerned with problems of government and the economy. One set of social scientists identifies with people in trouble, the other with institutions in need.

The authors claim that all traditions in sociology are against the established order in that they foresee inevitable breakdown. I would generalize the phenomena to practically all academic work. Which of us would become famous by telling readers or students that things are exactly the way they seem? Stability is always on the surface. It is latent rather than manifest functions that are every professor's stock-in-trade; otherwise, it would all be obvious, and there would be no need for any of us.

All of this, of course, is not to say that most social scientists or even sociologists are radical, but "*. . . there is a much higher proportion of radicals among social scientists than among any other group in American society that may be defined by occupational criteria*" (p. 122). Service to society in the form of consultantships to government or commitment to research, which often requires outside funding, need not lead to less criticism in favor of more accommodation. The evidence is that the more eminent the faculty members, the more critical their position; governmental consultants are more likely to oppose governmental policies, such as the war in Vietnam, than are those not similarly placed.

Age is an interesting variable. Younger faculty members are more liberal than older ones. The growing shortage of tenured positions in universities means, therefore, that faculties will be less liberal than they otherwise would have been, though quite possibly still more liberal than prior generations. Thus the political complexion of the academy may unintentionally become more conservative for demographic and economic reasons.

What issues will agitate the academy in the 1970s? Lipset and Ladd suggest unionism. They find that "liberal professors are more pro-union than their conservative brethren at all types of institutions and in all age and achievement groupings. At the same time, holding ideology constant,

we find that faculty at major centers of research and scholarship are significantly less supportive of unionism than their colleagues at academically weaker institutions" (p. 258). Here is one meaning of the divided academy: relative liberalism pushes faculty in one direction and professional status propels them in another. Trade unionism is egalitarian and scholarship is meritocratic.

When students are brought into the picture, the disadvantages for them of a system based on seniority become evident. "The AFT [American Federation of Teachers], from New York to California, has sharply opposed student involvement in judging the quality of their teachers" (p. 288). After all, students would then be determining who is hired and fired, thereby becoming a surrogate management. Students and younger faculty may cooperate on military spending, but will they agree on teaching and tenure? As the New Jersey Colleges AFT put it, "The traditional humane qualities which have characterized the academic community" require "a *high* rate of award of tenure" (p. 294). For young faculty in a tight job market, such a position may be attractive; students who wish access to the best minds may decide otherwise.

Following David Riesman, Ladd and Lipset suggest that the advent of unionism may give private universities strong advantages over public ones. Public universities may find it increasingly difficult to be selective in admissions and in promotions. Hence private universities, unburdened by bureaucratic rules and union contracts, might become more attractive for scholarship. On the same line of reasoning, of course, one would have expected private elite universities to stand up better against attacks on research and faculty government in the 1960s whereas, I believe, the record shows that they did not. Perhaps their very independence led faculty to believe they were cut off from the larger society and that they lacked appeal to political leadership.

More variables than are mentioned here may affect the academy. Shortages of funds may affect private more than public universities. Student interests may prove more powerful at the public rather than the private level. The shortage of faculty positions in both places may necessitate selectivity. Perhaps the 1970s and 1980s will offer more than an intensification of the trends of the 1960s.

In considering possible implications of their findings, Ladd and Lipset worry that "the growth of a critical intelligentsia disposed to support 'the adversary culture' and reject the worth of dominant political and economic institutions is undermining the capacity of governing systems in modern societies to maintain social equilibrium" (p. 312). They observe that intellectuals who run media are considerably to the left of the general population (though not so far left as the academy). And nothing, it seems, succeeds like success in breeding criticism. Perhaps the authors' worries are based on their belief that "almost certainly, the intellectual stratum will provide in a continuing fashion the principal nexus from which pressures for social change will emanate in postindustrial America" (p. 313). I wonder whether we are not taking ourselves too

seriously. The belief that the strongest force for change in the modern world is knowledge is constantly challenged by daily events. Fanaticism based on will rather than achievement is on the march. This ambivalence—the professoriate is too powerful and not powerful enough; merit is creative, merit is cruel; the academy should serve the people but not too much—is characteristic of the best thinking of our time. Even this outstanding book should not be expected to answer every question.

Japanese Radicals Revisited: Student Protest in Postwar Japan. By Ellis S. Krauss. Berkeley: University of California Press, 1974. Pp. xiv+192. \$11.00.

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The radicals referred to in the title of this book are former student activists of the *Zengakuren* whose massive demonstrations against the renewal of the United States–Japan Mutual Security Treaty in 1960 were a major precipitating factor in postwar Japan's most serious political crisis. Ten years later, in 1970, when the treaty was once more to be renewed, Ellis Krauss carried out the research for this study. His purpose was to see what had happened to those student activists of the 1960 struggle in the years that had passed since they had graduated from college. His "revisit" took the form of a follow-up study of a sample of men whose ideological "postures" as university students had been the subject of a 1962 survey by a Japanese sociologist (see Kazuko Tsurumi, *Social Change and the Individual: Japan Before and After Defeat in World War II* [Princeton, N.J.: Princeton University Press, 1970]).

Krauss's sample consists of the 53 men from Tsurumi's original sample of 100 students whom he was able to locate in one or another of the major cities of Japan's main island of Honshu and induce to respond to a questionnaire. The results of that questionnaire, mainly in the form of responses to fixed-choice items, plus the transcripts of in-depth interviews he conducted with 23 of the respondents, form the data base of the follow-up study. In addition, Krauss also had the raw data from the 1962 study to serve as a basis of comparison. Among the 53 men in the sample, 28 had either held office in the *Zengakuren* in 1960 or had been active members of one of its major factions. Most of the empirical analysis in the book is based on comparisons of this group of former leaders and activists with the group of 25 less active men who had been either "intermittent activists"—that is, "had never belonged to a major faction in the student movement but who had briefly participated in the [antitreaty] demonstrations in one form or another" (p. 28)—or nonactivists who had taken no part at all in the 1960 demonstrations.

The author makes it clear in the introduction that his primary concern is with the adult political socialization process: how the political attitudes and behavior of the respondents, especially the former leaders

and activists, had changed in the eight years since their graduation from college, and what factors in their experiences with Japan's adult world had been instrumental in producing those changes. But the actual analysis of this issue, which constitutes by far the most interesting and successful part of the book, does not appear until chapters 4-6. Preceding it, Krauss devotes chapters 2 and 3 to an attempt to deal with the pre-adult socialization of his respondents in order to determine why some of them became student activists in the first place. His treatment of this question, beset with serious problems of evidence and inference, is both less successful and less important than the discussion of postcollege change. So, reversing the book's order, let us look first at the latter.

The central question is, of course, how the former leaders and activists have changed. Popular mythology in Japan has it that the radical *Zengakuren* leaders of yesteryear are today's budding young captains of industry, having been co-opted by Mitsubishi, Mitsui, and the other corporate giants of Japanese capitalism. Krauss's data refute this myth, showing that the majority of the former leaders and activists (54%) have entered the professions, especially academia and journalism, as opposed to only 29% who have joined the business world; moreover, less than one in five (18%) were working for one of the giant corporations. On the other hand, only two of the former activists had made professional careers of their activism. Nevertheless, the preference of the former activists for the academic and journalistic professions contrasts markedly with the great predominance (64%) of business careers among the less active group and, Krauss argues, shows that the former activists by and large sought "an occupational environment that would allow them to fulfill their personal political values" (p. 98).

As for those values themselves, Krauss also lays to rest the equally common misconception that college radicals routinely undergo a post-graduation "conversion" to conservative values: the former leaders and activists were still overwhelmingly leftist in their political views and self-identifications in 1970, were still more active politically than the group of former intermittent activists and nonactivists, and continued to be hostile to the Security Treaty, although their interest in the issue had waned. But the eight years of adulthood had not been wholly without significant change. In particular, Marxism had lost much of its appeal to many of the former activists. In view of the centrality of Marxism to the intellectual basis of political opposition in postwar Japan, Krauss's discussion of the reasons for its declining appeal among those who had once been among its most ardent adherents is among the most valuable parts of his analysis. One might wish that he had dealt with this issue systematically in a single chapter rather than spreading it over parts of two chapters. Nevertheless, his integration of questionnaire data on the overall pattern of attitudes toward Marxism among the respondents in 1962 and 1970 with quotes from the in-depth interviews revealing why some of the former college radicals had come to reject Marxism in part or whole after leaving college constitutes a solid contribution both to the study of ideol-

ogy and politics in postwar Japan and to the general study of ideological development and change.

The final section of the analysis of postcollege change is concerned with the impact of adult roles, particularly occupation, on political beliefs and behavior. Krauss shows that, in general, working in the conservative worlds of business or government, as opposed to the politically more progressive professions, has little apparent effect on the degree to which the former activists maintain progressive or radical views critical of society and polity. But the job does have more of an impact on the beliefs of the less active group: "In all the dimensions of belief tested, the less active students who went into the professions are more likely to have a sympathy for Marxism and a progressive orientation than the less active students who are in business and government roles" (pp. 135-36). Krauss also argues that the impact of occupational role is greater on behavior than belief for both types of former students, but the evidence he presents on this point (p. 141, table 20) is not especially compelling.

On the whole, Krauss's analysis of the adult political socialization of his respondents is competently and interestingly done. Unfortunately, however, it is also extremely brief; the empirical analysis of the topic is crowded into only 66 pages of text. In comparison, the author spends 54 pages treating the *preadult* socialization of his sample in an ambitious attempt to "improve upon the static cross-sectional analyses of political socialization so common in the literature . . . [by using] both longitudinal and retrospective methodologies to trace the political development of the sample through each of the stages of their lives—from the family, to the school and university, and finally into adult roles" (p. 155). Although his data are admirably suited to the tasks of a follow-up study, the "retrospective methodology" of which the author speaks, namely, the use of recall by his adult respondents of their own—and their parents'—political characteristics during their childhood, presents epistemological difficulties that the author ignores and calls for caution in the uses and interpretation of the data, to which he pays no heed. His discussion of the reliability and validity of recall data is wholly contained in two sentences in the first chapter to the effect that "answers to retrospective questions . . . may be subject to the distortions of memory and selective perception" (p. 22), and that the recall method may supply "somewhat less reliable data on the earlier [i.e., childhood] formative process" (p. 22). With but these perfunctory remarks to indicate that the validity of the data may be at issue, the author undertakes an analysis of the relative influence of family, peer group, and schoolteachers upon the political interest, attitudes, and identifications of both subgroups of his sample during their childhood and teenage years. He concludes that "the family's role as a direct transmitter of party identification, as an agent of politicization, and as a model of political behavior seems quite limited. The most politicized, active students who supported left-wing parties often came from families of nonpoliticized, nonactive parents who did not transmit party preferences" (p. 53). More important than parents as socializing agents of the

activists were their middle school and high school peer groups, followed by their high school and middle school teachers. Only among the less active group does the author find the family to have had any political influence of note: "The family's principal role was in influencing about one-half of the less active students during the precollege period" (p. 64).

But the process by which these findings are derived is unlikely to induce many readers to place much confidence in them. The author's method for ranking the influence of the various agents illustrates vividly his casual attitude toward the evidence, as well as an insensitivity to both the limitations of recall data and the opportunities afforded by a follow-up methodology to reduce those limitations. His ranking is based on a direct comparison of the respondents' answers to two items in Tsurumi's 1962 survey asking whether or not they had discussed politics with their middle school teachers and high school teachers, with their answers to two items in his 1970 survey asking how often they discussed politics with their families and with their friends when they were in middle school and high school. The author treats the responses to these two pairs of questions, *asked eight years apart, by different investigators, and with different response options*, as if they had been elicited at the same time, by equivalent stimuli, and could be treated legitimately as comparable data. In a book that, in view of its main focus, should be especially sensitive to the effects of the passage of time on perceptions of the past, this is lamentable. Had Krauss replicated the Tsurumi questions in his own survey and offered her original response options on all four items, he would then have had comparable data on the four agents. That data would, of course, still be recall based; but by correlating the 1962 and 1970 responses he could have taken advantage of the follow-up nature of his study to provide evidence on the reliability of the recall over time.

If the basis for Krauss's findings on preadult political socialization is questionable, so too is the breadth of their applicability in contemporary Japan. The author himself explicitly recognizes early in the discussion that his sample is highly unrepresentative. But the discussion of the findings often reads as if they were applicable to, at least, all college-educated young Japanese. This is evident most clearly in the analysis of the transmission of partisanship from parent to child. Krauss strongly implies that his finding of no parent-child correlation in partisanship is broadly applicable to all those young adults who have been exposed to the socializing influence of the left-wing environment of the Japanese college campus. Perhaps; but that inference certainly cannot be validly drawn on the basis of his sample, which is not representative even of the college-educated youth of today. The sample is heavily weighted toward graduates of several of Japan's most prestigious and most politicized campuses, notably Tokyo and Keio Universities, and is also disproportionately made up of former activists and leftist students. Only seven of the 53 respondents in the sample had taken no part at all in the antiretreaty demonstrations, although the proportion of such nonactivists among even the students at Tokyo University, the *most* politicized campus in 1960, was estimated,

in a figure he himself cites (p. 29), at 35%. In short, the sample may be appropriate for a number of purposes, but the study of the broad patterns of preadult political socialization in today's Japan is not high on the list.

Two final points bear mentioning. First, the presentation and analysis throughout the book of the tabular material based on the questionnaire are not likely to satisfy methodologically minded readers. Chi-square significance tests, the only measures of association used, are given for some tables but not for others, without any explanation and with no discussion of the appropriateness of the statistic to the sample. And many of the tables are placed in the midst of the text without any identifying labels. Moreover, an apparent editorial oversight has led to the omission, in my copy at any rate, of the category labels for one of the variables in table 3 (p. 43). Second, the citations of the literatures on Japanese political behavior and on political socialization omit reference to much pertinent research. The 25 or so Japanese language citations in the bibliography, for example, include neither any of the works published on the panel and quasi-panel studies of social and political attitudes conducted every five years since 1953 by Hayashi Chikio and his associates, nor the work of Miyake Ichiro on party identification in Japan. And even the otherwise excellent discussion of the importance of major political events in stimulating political interest chides the literature for ignoring such events as socializing influences without making any reference to important works which *have* addressed that issue, such as the numerous articles about the effects on American children, teenagers, and college students of the assassination of President Kennedy, or the work of Richard Merelman ("The Development of Policy Thinking in Adolescence," *American Political Science Review*, vol. 55, no. 4 [December 1971]) showing the impact of political events on hastening the development of ideological styles of political thought among American teenagers.

In sum, then, the significance of this book lies in the contribution it makes to our knowledge of the important, and little studied, process of political socialization—and resocialization—in early adulthood. However, the scope of that contribution is limited by the author's decision to place nearly equal emphasis on preadult socialization, a topic for which his data are neither adequate nor appropriate.

Beyond Contract: Work, Power and Trust Relations. By Alan Fox. London: Faber & Faber, 1974. Pp. 408. \$12.00.

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Prompted by widespread social disorganization created in the wake of industrialization and by the need to forge new bases of solidarity, Émile Durkheim wrote *De la Division du travail social*. Such, at any rate, is the popular explanation of Durkheim's intellectual preoccupations. It is not surprising, therefore, that, some 80 years later, a self-proclaimed Durk-

heimian from Oxford should be lamenting "the galloping contagion of distrust and inflation" in Western industrial societies.

Alan Fox begins *Beyond Contract: Work, Power and Trust Relations* with the division of labor, arguing that industrial advance has led to a decline in discretion exercised in work. There is a tendency, Fox claims, for diminishing discretion to lead to diminishing trust, but the relationship between the two is mediated by such variables as power relations, ideological relations, and participants' expectations. Aware of organizations in which low levels of discretion are associated with high levels of trust and, conversely, high levels of discretion with low levels of trust, Fox argues that such deviant cases are to be understood in terms of perceived interests. That is, where relations between groups are viewed as antagonistic, low levels of trust are the result, irrespective of the level of discretion. Equally, where shared interests prevail, high trust is bound to occur. From these observations, one might conclude that structural antecedents of antagonistic interests, and not discretion, govern the level of trust in work relations. While Fox presents discretion as the key to understanding the evolution of industrial relations (with little empirical support), some contemporary economic historians, such as Stephen Marglin, suggest that levels of both discretion and trust are the product of antagonistic relations of production.

Fox returns again and again to the origin of the interests pursued by different groups in the industrial enterprise. He dissociates himself from unitary conceptions which treat the industrial organization as a monolithic entity and refuse to recognize the existence of divergent interests. He also rejects the pluralist framework because of assumptions it makes about the equal distribution of power between worker and management. Both perspectives are pronounced legitimating ideologies. The capacity of industrial management to secure the active consent of the labor force is not indicative of a "spontaneous consensus" but, according to Fox, a result of persuasion, socialization, and coercion. Workers offer only a "pragmatic" acceptance of their subordination. What is missing from the book is an illuminating and detailed account of the organization of consent. In engineering consent to domination, what are the distinctive contributions of the organization of production, the trade union and collective bargaining, the operation of the labor market, ideology propagated through mass media and educational systems, the organization of politics, socialization in the family, and so on? How do these structures interact and combine to produce consent? What is the relationship between force and consent? There is no shortage of writers, from Marx to Parsons, upon whom Fox could have drawn to give some body and theoretical foundation to his analysis.

Perhaps one reason for the underdevelopment of the argument is that Fox does not have a precise notion of the underlying structures of domination and exploitation to which consent is elicited. A second reason is that his analysis is too general, seeming to refer to any industry irrespective of political, economic, and technological context. An examination of

variations of, for example, class consciousness among industrial nations and among different enterprises within nations would have shed more light on the mechanisms for generating consent. Fox's analytic rigor and empirical insights do not match his ability to raise and skirt around important questions.

Fox points to the limited nature of the employment contract. It regulates the sale of labor power but not its use on the shop floor. The contract brings workers to the factory gates, but once they are inside, discretion, trust, and power define the character of industrial behavior. Organizational change, too, can be looked upon in terms of what Fox refers to as trust dynamics. In low trust dynamics, a low trust response from one group is countered with a low trust response from another group; in high trust dynamics the spiral runs in the opposite direction. The escalation of trust in either direction is halted by such constraints as the unequal distribution of power. Fox attempts to link the direction of trust dynamics to changes in the discretion embodied in work roles. But trust dynamics are a product of a multiplicity of other factors apart from the discretionary content of roles, and there is no reason to believe that the latter should be more important than any of the others. Fox's analysis would have benefited also from a distinction between changes in the structure of work (for example, the automation of the tin-plate industry described by Chadwick-Jones) and changes which occur within a fixed structure of work (for example, those in a gypsum plant studied by Gouldner).

Fox likens low trust relations to economic exchange, in which obligations are narrowly prescribed, and high trust relations to social exchange, based on diffuse obligations. The thesis propounded is that movement from small-scale (preindustrial) society to industrial society is characterized by a movement from high trust to low trust relations. Like so many sweeping generalizations, it is at best a half truth. Anthropological studies of politics in small-scale societies, and what we know of slave societies, by no means offer unqualified support for such a view. (In coming to his conclusion, Fox relies on a simplification of the work of such theorists as Spencer, Maine, Marx, and Durkheim.) In the only place where Fox even quotes a definition of trusting behavior—it consists “of actions that (a) increase one's vulnerability, (b) to another whose behavior is not under one's control” (p. 66, quoted from Zand)—the definition contradicts his thesis. For in small-scale societies where social control is immediate, there is no opportunity to exercise trust. It is misleading, therefore, to speak of such societies as characterized by high levels of trust.

Moreover, it is by no means apparent that trust diminishes as industrialism advances. As Talcott Parsons has been arguing for the last 20 years, the continuity of industrial society is contingent on the institutionalization of “symbolic media of interchange,” namely, money, power, influence, and value commitments. Their effectiveness rests on common understandings (shared values) and, most significantly, on trust. The rise of contractual relations is predicated on the appearance of “noncontractual

elements of contract," a fact which implies increasing levels of trust. Such an interpretation raises the question of the origin and reproduction of that trust necessary for the persistence of industrial society. The so-called galloping contagion of distrust can be understood only in terms of the prior and more fundamental problem—the continued existence of high levels of trust alongside inequalities in wealth and power.

I turn now to a related issue which Fox tends to obfuscate: the distinction between the distribution of trust and the amount of trust in a particular social system. When he links discretion, embodied in a particular role, to trust it is often unclear whether he is considering the quantum of trust found in the entire set of role relations or the trust embodied in one particular role relation. He is most frequently concerned with a decline of trust in what he refers to as the vertical dimension between those who dominate and those who are dominated. Yet this by no means implies a movement in the direction of a low trust society. A shift in the level of trust in one role relation may stimulate compensating shifts in other role relations. One might, for example, inquire into the consequences of a decline in trust between management and worker for other relations, such as those among workers. In asserting the rise of low trust society, Fox may be mistaking changes in the level of trust for changes in its distribution. Speculation about change in the level of distribution of trust requires not only empirical examination but also careful specification. There is a world of difference between, for example, trust expressed by workers toward top management and that expressed toward the foreman. Changes in the one may be offset by changes in the other. Furthermore, in crediting a relationship with trust, Fox appears to assume a symmetry in trust sentiments. Is it not possible for a foreman to trust a subordinate and the subordinate not to trust the foreman? Precisely between whom is there less trust? Fox's theorizing is premature. Without empirical data to fashion the connections among a promising set of concepts, his conclusions are unimaginative, unenlightening, and unconvincing. Generalizations cannot precede an examination of variations in the distribution and levels of trust within and between different social systems. But such an examination would require a more rigorous definition of trust with careful distinctions, first, between behavioral and attitudinal expressions of trust and, second, between voluntary and institutional forms of trust. Again, Fox touches on these issues but fails to explore them in any depth.

The book closes with a utopian ring. The restoration of trust is to be found in the pursuit of social justice. Paradoxically, until this point, the book is devoted to explaining the inevitability of "the galloping contagion of distrust and inflation" between managers and workers, rulers and ruled. In trying to forge a link between the intellectual and moral preoccupations of Durkheim on the one hand and certain radical critiques of capitalism on the other, Fox has come to grips with neither. His work reflects a continuing dilemma of sociology in the face of political and economic crises, such as the one which has overtaken British society.

The Politics of Turmoil: Essays on Poverty, Race, and the Urban Crisis. By Richard A. Cloward and Frances Fox Piven. New York: Pantheon Books, 1974. Pp. xiv+365. \$10.00.

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In this collection of essays (all but one previously published) spanning the decade from 1963 to 1973, Richard A. Cloward and Frances Fox Piven present a postmortem of "the political crisis of the 1960s precipitated by the dislocations that followed the great black migration to the cities" (p. xiv). But this examination includes a curious feature: the autopsy began before the crisis was clearly ruled a cadaver. Because most of the work (11 of the 17 essays) was completed before 1969 while black unrest still continued to provoke massive direct federal intervention in the affairs of cities, the essays together form an interpretive chronicle of the crisis as it moved toward its demise.

Cloward and Piven analyze the crisis in terms of the potential for political returns from activity of a group so marginal that it is without any regular political resources: the urban, black, nonworking poor. Their analysis represents a solution to the riddle "what might have been and why it wasn't." "What might have been," according to the authors, were concessions offered by a liberal Democratic party leadership, terrified lest its electoral coalition (which included working-class whites in northern cities) disintegrate under the pressure from the widespread disruption of major institutions by the poor who no longer acknowledged their legitimacy. In other words, the only bargaining chip the poor possessed was a negative one: withdrawal of their tacit support of institutions in order to provoke those who stood to lose political advantage in the wake of such disruptions. Hopefully, the resulting settlement would include reparations to the poor as part of the repair. But reparations offered were few and tapered off as the poor's "disruptive and irregular tactics" (p. 86) metamorphosed into attempts at more conventional political method under the supervision of professionals in an atmosphere of co-optation.

Cloward and Piven group their reasons for "why it wasn't" under four large sections contrasting their strategies with those of others (in three sections there are comments and rejoinders) and describing what they saw taking place. In the first section, "The Public Bureaucracies and the Control of the Poor," the authors describe the poor as wedded to the labyrinthine public service bureaucracies which are dominated by professionals who act as mercenaries for the affluent. In obtaining benefits which those professionals control, the poor are not only atomized but also "educated in the beliefs and strategies of action appropriate only for groups in higher economic strata" (p. 25). And while service advocates may help Mrs. B get her welfare check, planning advocates narrowly funnel political action into planning which is "ineffective in compelling concessions, but may be

very effective indeed in dampening any impulse toward disruptive action" (p. 48). The poor are diverted from their only resource.

In the next section, "Mobilizing the Poor: How It Can Be Done," Cloward and Piven provide an example of how the disruptive strategy could be employed in welfare by encouraging the poor to claim "benefits held back by the system" (p. 137) and to challenge the idea that the dole is shameful. Overloading the rolls would anger the working class, leading to necessary removal of the dry rot in the system. But there are stumbling blocks: there is no identifiable "latent structure of interaction" (p. 83) to underpin organization; contrary to conventional organizing lore, here the goal of formal organization only dilutes the tactic; and the lure of acceding to institutional rules, such as holding rent in escrow during a rent strike, is dangerous, "for in playing by institutional rules, they take on the full complement of inhibiting requirements imposed by powerful groups who can make the rules" (p. 160).

In the third section, "Blacks and the Cities: The Prospects for 'Black Power,'" is found the argument that the black poor must develop separatist politics and communal organizations to further their particularistic interests in the cities where they live. Even so, the outlook for political gain is bleak, since the cupboard is bare in most cities. Cities collect a mere 7% of tax revenues (p. 263); ethnic machines have been replaced by professional bureaucracies so that municipal jobs are protected by boundary-keeping measures; and city governments are being absorbed by metropolitan ones, tipping the balance in favor of suburban whites.

In the concluding section, "The Great Society: Who Got What and Why," the War on Poverty is analyzed as an attempt to pull the Democratic coalition together: the federal government created direct links to the ghetto, securely tying the black poor to the party's national level while also opening up the local structure for them; the white working class in the city was reassured by programs that focused on pathologies within the ghetto. Public goals were "political resources" (p. 309), since the Great Society funds even paid for voter registration drives in the ghetto (p. 279). While the black poor made qualified gains in welfare, and some of them who were "already poised to enter the middle class" (pp. xii-xiii) gained mobility, the real beneficiaries of increased black demands upon public services were the municipal workers who demanded and collected hazard pay and "new guaranties of job security" (p. 325). The book ends with an allusion to yet greater victors whose spoils were camouflaged by the urban skirmishes: "Schoolteachers turned against the ghetto, the taxpayers against both, but no one turned against the concentrations of individual and corporate wealth in America" (p. 340).

This collection of essays suffers from two common problems of its genre: first is the repetition of general commentary in tandem with an absence of detailed evidence and systematic analysis. For example, in at least two passages (pp. 275 and 285) can be found the statement that black electoral fickleness in supporting Stevenson in 1952 and 1956 led to Kennedy's courting the black vote in 1960; little evidence is offered

to flesh out this analysis. Another example is found in the contention that Chicago, in retaining its patronage-fed machine, has escaped many of the considerable problems New York now faces (p. 328), among these the fiscal crisis which is in part the result of untempered wage and salary demands made by municipal workers upon a city government unable to deny them. Both examples deserve more treatment than they receive. Second, the collection is subject to the related problem of seeming contradiction: in an assessment of the future of Black Power, local government is variously described as "greatly weakened since the heyday of the ethnic urban machine," vulnerable, and problem ridden (p. 263). On the other hand, in an explanation of the political nature of the War on Poverty, it is described as "recalcitrant" and a "major impediment," "an obstacle" (p. 277). One further criticism: although I ordinarily consider it a cheap shot to fault authors for what they did not do, I think that Cloward and Piven miss an obvious opportunity to clarify their argument by not analyzing critically the structure and function of the National Welfare Rights Organization (NWRO).

Even so, *Politics of Turmoil* is a worthwhile addition to the glut of anthologies on "poverty, race, and the urban crisis," because it uses a theoretical framework which can make sense of seeming confusion and isolated activity. It imposes an ordering upon a political phenomenon which, given its roster of contextual and organizational imperatives, is of historical moment.

Looking Ahead: Self-Conceptions, Race and Family as Determinants of Adolescent Orientation to Achievement. By Chad Gordon. Washington, D.C.: American Sociological Association, 1972. Pp. iii+120. \$5.00 (non-members); \$2.75 (members).

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The point of departure in *Looking Ahead* is the set of issues regarding racial differences in family instability and their consequences for the cognitive, affective, and social development of children. Chad Gordon undertakes to evaluate some empirical evidence bearing on these relationships in a way which promises to avoid the pitfalls of earlier work. Actually, he goes beyond these particular issues by setting forth an empirical model for the development of self-conceptions and aspirations in young people which contains aspects of social structure, family structure, and intervening processes.

The book is best described as a research monograph. Perhaps its most unique offering is that it analyzes data from the *Equality of Educational Opportunity* survey (EEOS) (J. S. Coleman, E. Q. Campbell, C. J. Hobson, J. McPartland, A. M. Mood, F. D. Weinfeld, and R. L. York [Washington, D.C.: Government Printing Office, 1966]) which have heretofore been virtually ignored by both the original report and subsequent analyses

of the data (e.g., *On Equality of Educational Opportunity*, ed. F. Mosteller and D. P. Moynihan [New York: Random House, 1972]). The EEOS questionnaire for ninth and twelfth graders contained considerable information on academic self-conceptions, self-esteem, and aspirations. Gordon takes advantage of the availability of these measures in addition to measures of parental aspirations (reported by the child), verbal ability, family structure, parental status, and race. The data for ninth graders from the metropolitan Northeast were sampled using a 5% systematic random sample ($N = 1,684$) of the total number of students. The data provide wider regional representation than most studies dealing with racial differences in self-conception and aspirations among high school students. Also, the response rates for the metropolitan Northeast were among the highest within the EEOS, so the quality of the data is reasonably high.

Chapter 1 provides the theoretical orientation for the study. Chapter 2 sets forth a causal model of the relationships among social structure (race and social class), family structure, intervening social-psychological factors (verbal ability, parental aspirations, and self-conceptions), and relevant educational outcomes (educational desires and occupational expectations). The causal linkages are generally palatable and consistent with existing literature, but some readers may be inclined to doubt some of the assumptions of one-way causation which are explicit in the model. Chapter 3 describes the sample and the measures of the variables.

Chapters 4–6 present the analyses of the data. First, several hypotheses are examined one by one using a cross-tabular format (pp. 29–87). Then the parameters of some recursive models are estimated by the method of path coefficients (pp. 88–110). The reader may justifiably wonder why both sets of analyses are presented, since essentially the same findings appear in each instance. Perhaps some judicious editing could have reduced this monograph to the size of a journal article. There are some important differences, however, between the two sets of analyses which require comment. First, only in the cross-tabular analysis does Gordon bring race explicitly into the model in any systematic way. This is important since the main effects of race on family structure, self-esteem, and aspirations net of social class speak directly to the issues raised by Moynihan and others. As I point out in more detail below, Gordon's failure to deal explicitly with race as a variable in his path analysis obscures his findings and constitutes a major weakness in his analytic strategy. Second, the cross-tabulations allow the detection of nonlinear and nonadditive effects. Researchers using path analysis must routinely test the assumptions of linearity and additivity before launching into a path model. Gordon's approach is commendable, but his presentation lacks any formal treatment of these issues. In this sense his subsequent analysis is not informed by the prior examination of the data in detail.

The analyses of the data yield the following findings. (1) Controlling for social class, black children are more likely than whites to come from unstable homes, to report positive parental aspirations, to have lower verbal test scores, and to have positive educational aspirations but with

lower occupational expectations; they are also less likely than whites to have positive self-conceptions. (2) Within racial groups, social class affects family structure, parental aspirations, verbal test scores, self-conceptions, educational aspirations, and occupational expectations. (3) Family structure affects verbal test scores, parental aspirations, self-conceptions, educational aspirations, and occupational expectations, net of race and social class. (4) Verbal test scores affect parental aspirations, self-conceptions, educational aspirations, and occupational expectations, net of race, social class, and family structure. (4) Parental aspirations affect self-conceptions, educational aspirations, and occupational expectations, net of race, social class, family structure, and verbal test scores. (6) Self-conceptions affect educational aspirations and occupational expectations, net of all prior variables. I have not summarized the magnitudes of the effects and the manner by which they are transmitted, nor have I summarized the interactions found by race.

Most of these findings are not surprising, given the previous literature, but some deserve further comment. The finding that black children are less likely than whites to have positive self-images is, as Gordon observes, at odds with other findings regarding the self-conceptions of minority-group children (including some of his own research). It is worth mentioning that a more recent review of the existing literature by Morris Rosenberg and Roberta Simmons (*Black and White Self Esteem: The Urban School Child* [Washington, D.C.: American Sociological Association, 1974]) suggests that blacks do not have lower self-esteem than whites, and this point is further supported by their research in Baltimore. The evidence on this issue is clearly mixed, and the resolution of the discrepant findings is not obvious. The EEOS data show clear differences in global self-esteem and self-determination favoring white children within categories of social class (pp. 40-42). Unfortunately, these differences are obscured in Gordon's subsequent discussion of the question (p. 106). He discovers that the race effect on self-esteem reduces to zero when, in addition to social class, the intervening variables (family structure, verbal ability, and parental aspirations) are controlled. He concludes from this that black students do not have lower self-esteem and that the earlier finding is called into question. In drawing this conclusion Gordon ignores the fact that race affects self-esteem via verbal ability and reported parental aspirations. To conclude that race has no effect because the direct effect is zero is nonsense. Part of the confusion here results from Gordon's way of handling the data, which is not conducive to estimating race effects (see below).

One of the purposes Gordon had for undertaking the analysis of the EEOS data was to add clarity to the issues raised by the Moynihan Report. Gordon hoped to argue that the effects which Moynihan attributed to cultural factors associated with race are due to social class instead. He attempts to make a convincing case that the effect of social class on family structure is stronger than is the race effect (pp. 31-35). Unfortunately, his operational definition of social class is dependent on family structure in

that the "lower-class" category contains those students who had no working father, and the student's score on the family-structure variable also depends upon whether the father is present in or absent from the home (p. 17). Recognizing this, Gordon retreats to the position that the hypothesis (that social class has a stronger effect on family structure than race) applies only to the comparison of working- and middle-class youth. In fact, the data show that middle-class black youth have a higher incidence of unstable family patterns than those from working-class families (contrary to expectation), whereas white middle- and working-class rates are about the same (also contrary to expectation). Moreover, the data indicate that within social class categories black students are over twice as likely to come from unstable families. Gordon concludes that "lower-class position is so strongly associated with weak paternal role or with complete family disruption that differences between working- and middle-class patterns are overshadowed, and this pattern is nearly as malignantly strong among whites as among blacks" (p. 36). This conclusion is unacceptable given the data presented and the qualifications that must be placed on "lower-class" differences in Gordon's analysis. The analysis does not fulfill Gordon's promise to clarify the issues raised by Moynihan.

Although *Looking Ahead* has the appearance of being methodologically sophisticated, it contains several basic flaws whose correction would have lent clarity to the issues addressed. First, Gordon did not pool the black and white data from the EEOS using routine weighting procedures. The problem could, of course, have been managed when he selected the 5% subsample. He insists at several points that, since blacks were oversampled in the EEOS, he is required to carry out his analyses separately by race. Laying aside the virtues of within-race analysis, it may be said that if the data had been appropriately weighted to take care of the oversampling problem, both the main effects of race and its interactions with other variables could have been systematically examined. As it is, we are provided only weak information on the main effects of race (based on unweighted data), and unsystematic information on the interactions. Second, in all instances only standardized measures of effects are computed, for example, correlations and path coefficients, and no metric effects are presented. This is especially a problem in the comparison of effects between the races (pp. 104 ff.), that is, race interactions. What is worse, no awareness of the problems of making comparisons among groups using standardized coefficients is apparent in the discussion.

The issues raised by this monograph are relevant to the study of inequality, stratification, education, socialization, and the family. I am reluctant to say that it advances our understanding of racial differences in self-conceptions and aspirations. These are critical substantive matters, and Gordon deserves praise for raising them. It is unfortunate that poor methods undermine the full attainment of these objectives. At the same time, the book gives us an impression of the ways in which these outcomes depend upon social background, family structure, and intervening processes. This is a valuable supplement to the existing literature. In addition,

some indication of the differences in the process by race is presented, but again the successful completion of this task is thwarted by methodological difficulties. It is clear that we need further systematic treatment of the issues raised in *Looking Ahead*, using samples from known populations. I look forward to the continued discussion of these issues which further research will bring.

Ambition and Attainment: A Study of Four Samples of American Boys. By Alan C. Kerckhoff. ASA Rose Monograph Series. Washington, D.C.: American Sociological Association, 1974. Pp. vi+106. \$5.00 (nonmembers); \$2.75 (members).

Carlton A. Hornung

University of Maryland

This book, part of the Arnold and Caroline Rose Monograph series, is a study of the status attainment process that does more than simply rediscover the significant relationship between socioeconomic status of origin and status of destination. Alan Kerckhoff seeks an interpretation of the role of social psychological factors in the process by which individuals move from one point to another in the stratification system.

In the first chapter the author discusses six factors (ability, opportunity, performance, sanctions, understanding, and ambition) that, within the context of family and peer relationships, are central to the study. The second chapter provides an overview of the design of the study. Data were collected in 1969 from samples of males in the sixth, ninth, and twelfth grades and from the 1963 cohort of high school graduates in a typical middle-size American city (Fort Wayne, Indiana).

The causal models presented in the third chapter are derived from the work of Otis Dudley Duncan. The model of attainment is applied to the graduate cohort, and a similar model of ambition is applied to the in-school cohorts to explain expected education and occupation. The analysis follows two parallel strategies. One focuses on differences across the four cohorts as indicative of probable changes over time in the effects of the exogenous variables (IQ, number of siblings, and father's education and occupation). The other strategy is a search for an explanation of how the relationship between status of origin and expected or actual status comes about. The role of "school experience" is considered in the fourth chapter, while chapter 5 focuses on the boy's "beliefs about opportunity." In chapters 6 and 7 Kerckhoff considers the roles of parental and peer influence, respectively, while the final "summary, synthesis, and interpretation" chapter presents synthetic cohort models for which observed and implied correlations are compared.

In summarizing the findings, Kerckhoff points out that a significant proportion of the variance in educational and occupational outcomes is explained and that academic grades, participation in school activities, fatalism, and characteristics of friends help to explain the relationship

between the background variables, particularly IQ, and educational and occupational outcomes, expected or attained. Further, moving from the youngest to the oldest cohort, the models become increasingly more powerful in explaining the dependent variables. Nevertheless, "Although the whole design of the study was based on a social-psychological conceptualization, perhaps the most disappointing thing about the outcome is the limited direct support it lent to that orientation" (p. 102).

What the book does not do is cause for disappointment. No reason is given for excluding females from the samples, and, although nonwhites were included, the author states that there were too few to permit an entirely comparable analysis. Only the results for white males are reported; mention is made that some analysis of the data collected from black males leads to "strikingly different" (p. 19) outcomes, but there is no indication of what these differences are.

Further, although one of the principal foci of the study was to view changes from the youngest to the oldest cohort, no systematic comparisons of the fit of the models for the four cohorts are provided. The models are also not developed incrementally: variables entered in the models in one chapter are not necessarily retained when factors discussed in the next chapter are included. In addition, the path analyses are limited to recursive systems even though there were several points where the temporal sequence of the variables are an issue and one-way causation a dubious assumption. The author is sensitive to this problem and does provide some rationale for analyzing asymmetric causal effects. The study would have benefited from a treatment of feedback or symmetric causal effects which, in conjunction with an incremental modeling strategy, could possibly have provided the convincing interpretation of the status attainment process that the book set out to develop.

This is an excellent book in spite of my criticisms. Its value lies in the fact that the author continually juxtaposes clearly stated theory and his data. He accomplishes this difficult task using an analysis strategy that is presented in a clear and not too technically complex manner. In this respect Kerckhoff's book is a very good example of the sociological research process and one that should stimulate others to explore the unanswered questions about the process of status attainment.

Socioeconomic Background and Educational Performance. By Robert Mason Hauser. ASA Rose Monograph Series. Washington, D.C.: American Sociological Association, 1971. Pp. x+166. \$5.00 (nonmembers); \$2.75 (members).

Alan C. Kerckhoff

Duke University

This volume reports a careful secondary analysis of data collected in 1957 from whites in the junior and senior high schools of Nashville, Tennessee, and surrounding Davidson County. This analysis, dealing with the explanation of levels of school performance, is particularly concerned with

the identification and explanation of contextual effects—differences in performance between schools. Robert M. Hauser is one of the more sophisticated data analysts working in this area, and this monograph exhibits much of his skill. At the same time, the work has important flaws which weaken its substantive contribution.

Hauser presents an involved discussion of the logic and algebra needed to specify the degree to which variations among groups can be attributed to the characteristics of those groups. The discussion of his covariance approach to this problem is sometimes very difficult to follow, but his main point is that the total variance can be separated into within-group and between-group portions, and, more important, that the latter can be further divided into variance directly attributable to characteristics of the groups (what he calls "compositional effects") and a "residual effect." He treats the latter tentatively as an indication of the effect of the quality of the school, though no specific school measures are available.

The data include measures of SES, ability, membership in school organizations, perceived parents' goals for the student, school grades, achievement test scores, and (for county students) the student's educational and occupational aspirations. Grades, test scores, and aspirations are the dependent variables. Severe problems were encountered with missing data, casting some doubt on the results.

Four of the eight chapters are devoted to the analysis of within- and between-school effects and their explanation. Achievement test scores have the largest proportions of total variance between schools, sometimes more than one-fourth. The "compositional effects" (those attributable to the SES and ability characteristics of others in the same grade in a given school) are generally less than half of the between-school effects. Most between-school variation is "residual," which means it is unexplained. Hauser concludes that between-school variation must be generally rather small and not all of it can be attributed to school quality.

Within-school variation is analyzed using familiar path models, but Hauser also conducts analyses using hypothetical variables and the reciprocal causation of two variables. Although he emphasizes that the use of hypothetical variables is "quantitative speculation," he does not make full use of its potential. The method requires one to estimate the correlations of the hypothetical variable with others in the model and to hypothesize the "causes" of that variable within the model. Unfortunately, Hauser does not consider multiple estimates or varied models for a given hypothetical variable. To test the limits of such "quantitative speculation" with varying models and correlations would help us to narrow the range of our verbal speculation, and that would be a significant gain.

It is impossible to summarize here the results of Hauser's highly complex analyses. A great deal of hypothesis testing and comparison with other studies and other theories is included in these few pages. In fact, it can be argued that too much is compressed into this small volume. It is often difficult to follow Hauser's line of reasoning because he moves so rapidly from a brief question to an involved analysis and seldom presents

a summary statement of the results as they relate to the original question. Thus, the reader might be well advised to read the summary chapter first to obtain a general view of the major questions and findings discussed in the earlier chapters.

The entire analysis involves the separation of within- and between-school variance. At first reading, this seems a very commendable approach, but the data are not wholly appropriate for such an analysis. The most important dependent variables, achievement test scores and school grades, were measured when the students were in the eighth grade, except that for the seventh graders the measures were made in seventh grade. Thus, except for seventh and eighth graders, dependent variables were measured prior to the independent variables, sometimes four years earlier.

Even ignoring problems of causal ordering, this fact erodes the validity of the definition of "school" or "context." Students were grouped according to the schools they were attending in 1957, when the questionnaire data were collected. Thus, for instance, twelfth graders were grouped into 32 clusters of male or female students in the 16 high schools' senior classes. However, the students in any one of these groups were attending several *different* schools when they were eighth graders. Even if each junior high school fed only a single senior high school, the fact remains that there were twice as many eighth grades as twelfth grades (32 versus 16). Also, the "context" those twelfth graders had experienced in eighth grade included many students no longer in school. Those from schools with high attrition rates were in a different school building in twelfth grade, they were mixed in with students from other junior high schools, and they were among a much more select set of fellow students than they had known in eighth grade. There is no way to estimate the effects of these changes of context on the results Hauser reports, but it seems reasonable to be skeptical about his findings for such groups of students. This skepticism must apply equally to the within-school findings since they are computed net of the between-school effects. The substantive findings are adequate to the extent that they deal with the seventh and eighth graders, and it is important to state that the general between-school conclusions do hold for these two early grades. Unfortunately, however, the within-school analysis is conducted for all of the grades combined, so it is not possible to say the same about all of his findings.

The monograph is thus primarily an object lesson in analytic logic and techniques, its substantive findings being cast in doubt by both the large amount of missing data and the awkward definition of school context. As an object lesson, it has much to recommend it. It is difficult to read, partly because of the algebra, partly because of the use of meaningless letters to represent variables (even the author gets confused on page 36), partly because of the terse writing style, and partly because the logic of the analysis is often quite complex. But it is worth the effort. It should provide a good basis for in-depth discussions in graduate methodology classes as well as an analytic guide for those who would pursue further the issues with which it deals.

The Social Grading of Occupations: A New Approach and Scale. By John H. Goldthorpe and Keith Hope. Oxford: Clarendon Press, 1974. Pp. viii+188. \$5.70.

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For at least 50 years sociologists have labored to determine the best ways to conceive of and to measure similarities and differences among occupations so that the results would be useful to researchers interested in social stratification. The basic idea is that in any society occupational roles are so important that they structure almost everything objectively required by or subjectively valued by their incumbents. They are thought to be the bases of the distribution of personal resources and of rewards. A hierarchical ordering of occupations would thus have a wide range of consequences.

Almost all, if not all, sociological conceptions of hierarchical orders in society can be construed as one or more of the status dimensions called "power," "wealth," "prestige," and "information"; or if not they themselves, then subdivisions or logically inseparable combinations of them. For the most part American sociologists have taken what they call "occupational prestige" to provide the most fundamental hierarchical order, and have gone on to map similarities (occasionally, differences) in occupational hierarchies at various times and places and recently to describe the effects of antecedent variables on the occupational prestige of individuals.

A great deal of effort has been devoted to attempts to establish these "occupational prestige hierarchies." The usual assumption is that occupations are differentially evaluated by the population at large and that the evaluation of an occupation constitutes its prestige. An individual is thought to be prestigious to the degree that his occupation is so defined. Typically, these hierarchies are determined by asking representative samples of adults to rate each of a sample of occupational titles according to some pertinent criterion, such as prestige or social standing, either as the rater himself sees it or as he thinks "most people" see it. Sometimes these data are combined with others so as to order larger numbers of occupations which were not directly evaluated.

Yet quite a few sociologists have been dissatisfied with this approach, often at the conceptual level, occasionally at the operational level. Regarding the former or theoretical level, there seem to have been two completely different lines of thought attacking the use of popular evaluations as bases for ordering occupations hierarchically. One would greatly simplify the categories, perhaps to a dichotomy of occupational roles, and would order them on a single dimension composed of both wealth and power. Followers of this line tend to be concerned with the differences in the resources available to each person in competitive struggles; they generally believe that these resources are essentially matters of control over the means of production; they regard popular evaluations and indeed prestige as epi-

phenomena of no serious consequence. Followers of the other line appear to see wealth, power, and prestige as different words for a single analytically irreducible dimension, and they often use the word "prestige" to denote it. Unlike the former, they do not view prestige as epiphenomenal, but they do view popular evaluations as a rather poor reflection of it.

The recent work of Goldthorpe and Hope grows out of the latter line of thought. Some years ago they set out to elaborate the concept of prestige in such a way as to make it useful for studies of stratification in which the fundamental dimension, as they see it, would be measured more appropriately. This book, the latest of a series of works, is addressed to the problem of measurement.

The authors agree that occupations are somehow the key units to be observed in the study of stratification, and they allow for a finely graded hierarchical order. They hold that the main empirical evidence of prestige differences is the order of deference, acceptance, and derogation. They deny that popular evaluations and prestige are necessarily related to each other and give two reasons for their denial. First, they say that when respondents rank or score occupations they really have in mind the rewards that incumbents obtain. They doubt that deference weighs greatly in such estimates. Second, in their own research they have found considerable disagreement among respondents regarding the order of occupations, and they think that if prestige were being measured validly, the interrater correlations would be much higher. Although this argument is not wholly clear to me I think they mean to imply that a unique, clear, and constant order of deference (in matter of honor, power, and wealth) exists among incumbents of the different occupations, and that questions validly eliciting this variable would show very high agreement among pairs of respondents.

Clearly, the writers were displeased with the results of their own attempts to measure occupational prestige. Yet they have produced quite an impressive rank order of occupations, apparently along the same evaluative dimension most others have called "prestige." Since they have quite obviously scaled something of interest to a great many sociologists, and since they apparently need it for their own research, they have made the resulting scoring system available, despite their own apparent dissatisfaction with it on theoretical grounds. They call it "social desirability." As an index of occupational standing, it has the same theoretical virtues as the other indexes based upon popular evaluations but usually called "occupational prestige" scales. In a preliminary study reported in their book, they demonstrate that respondents' ratings of occupations according to "standard of living," "power and influence," "level of qualification," and "value to society" may be described adequately by one underlying hypothetical variable, which at that point they call "social standing."

In developing the scale they were especially careful about sources of error in the scaling of occupations and in the scaling of people according to their occupations. They discuss inconsistency among raters and of the same rater at different times. They treat unreliability due to coding:

problems of establishing homogeneous classes of occupations, of identifying a particular occupational title as a member of a class, and of scoring a title. Some of these are problems of developing a valid and reliable scale, and some are problems of applying it to determine the social standing of individuals. In all, 860 occupational titles were rated. These are based firmly upon the system the British census uses to classify occupations. Ultimately they were formed into three scales of different levels of specificity. The most useful seem to be those given in tables 6.4 (pp. 96-108) and 6.6 (pp. 134-43). Detailed instructions are provided for users.

Technically, this is a commendable and interesting piece of research, and considering its complexity the report is well written. If sociologists normally paid as much attention to both theoretical and procedural detail, the conceptual and empirical bases of the field would be more solidly founded than they are today. Even so, a few critical comments may be in order.

1. We do not yet know just how the various status stratification dimensions and variables hang together. Present evidence does not permit us to conclude that one basic dimension accounts for all others. (Respondents' evaluations of different aspects of jobs are just that—evaluations. They are not measurements of objective status or status behaviors.) Deference behavior surely exists. But the extent of correlation of a deference-precedence hierarchy based upon observations of such behavior, with other variables describing influence as well as those describing wealth differences, etc., has yet to be determined. Thus it is also premature to conclude that there is but one status hierarchy, whether one calls "it" prestige or gives it any other name.

2. Evidently, the writers were convinced of the existence of a unique order of deference among occupations, such that those in any specific occupation would always take the same deference stance with respect to those in any other. The method they used, of course, failed to reflect such a variable. I think the search for it is doomed to fail, because it does not exist. A well-developed deference-precedence order would require an elaborate etiquette of interoccupational relationships, reinforced by a powerful and equally elaborate set of sanctions. Probably such a system exists in rudimentary form, but the occupational role structures of urban and, especially, modern societies are extremely complex. True, one probably has a good knowledge of the total stock of occupational roles or at least those of the people with whom he might normally interact. For strangers to enact an occupational deference-precedence etiquette would require that each identify the other's occupation, and this requires occupationally specific status symbols. We rarely provide those outside our workplace, and even then often only to persons who simply must know who we are in order to get their own jobs done.

3. In this research an unusual method was used to obtain ratings of occupations. Obviously, the ultimate purpose of the authors is to rate people. It would have been helpful if the scales had been applied to one or more samples so as to obtain data on their reliability and validity (as

distinct from the methods by which they were constructed). Surely, test-retest correlations and correlations with previous British occupational status scales, as well as with measures of the other main dimensions of status stratification, are required. The authors' disclaimer in the introduction, to the effect that different populations would yield different results, is not a strong justification for this serious omission. Such data should be provided before the scales can be used with confidence.

Because this work is apparently one of the foundations upon which future research on the British stratification system is to be built, it doubtless will be influential for a long time to come. Researchers working on comparative stratification will also find it useful. This is one more product of a long and serious research effort. I, for one, have found this and the previous writings of Goldthorpe and Hope enlightening, and I look forward to seeing their writings on the subsequent stages of the project.

I, Pierre Rivière, Having Slaughtered My Mother, My Sister, and My Brother . . . : A Case of Parricide in the 19th Century. Edited by Michel Foucault. New York: Pantheon Books, 1975. Pp. xiv+288. \$10.00.

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The last execution of capital punishment in the United States was carried out when Luis Jose Monge was put to death by the State of Colorado on July 2, 1967. Since then, many persons have been condemned to death by the courts, and some of them are still awaiting their fate. The moratorium is due to the fact that the U.S. Supreme Court placed impediments upon the administration of the death penalty which have raised questions concerning its legality, at least as far as its present forms are concerned. The Court has agreed to review some further cases, but it is quite possible that forthcoming opinions will be limited to the consideration of matters pertaining to particular cases. In that event, abolitionists will continue struggling to save lives, while advocates of the death penalty will seek to frame norms than can be squared with binding judicial interpretations. Those who hope for it could await the arrival of total abolition by letting things go on as they have. But the outcome is uncertain. And it does not seem fitting for a civilization that has assumed responsibility for defining its own character to avoid the problem.

Though the death penalty is even now applicable to a number of crimes—among them, rape, treason, and kidnapping—the scope of its problematics is not substantially diminished by limiting discussion to the question whether a person who has killed deliberately may himself be killed deliberately by the state. At least, it seems plausible that if the state were restrained from putting murderers to death it would, a fortiori, be so restrained in punishing other crimes. More important, the concentration on murder as the definitive capital crime opens the possibility that

in reflexive juxtaposition the acts of illegal and legal homicide will somehow illuminate one another, helping thereby to show a way out of the present impasse.

I believe the portrayal and interpretation of the case of parricide contained in the volume produced by Foucault and his collaborators ought to be considered with this hope in mind. Approximately three-fifths of the book comprise documents, among which is a lengthy (66 pages), eloquent, and scrupulously detailed memoir by the parricide. The rest consists of seven essays written by Foucault and members of his seminar at the Collège de France. These materials contain a wealth of information about peasant life in Normandy during the first half of the 19th century, about a phase in the history of criminal procedure in France, and about the development of forensic psychiatry. Some of the interpretative essays are fairly straightforward. More often, the interpretations tend to be pregnant, if not pointed. Even when the argumentation gets strident, one is rarely told in so many words what this or that fact finally means, and one is often drawn into shadowy areas containing matters worth thinking about. To be led from mystery to mystery, to be tossed from one question to another, is not to everyone's taste; but this is, for better or for worse, Foucault's method, though one cannot be too sure even of that.

The case is said not to have been a "notable crime" in its own time. In the summer of 1835 a peasant youth, Pierre Rivière, killed his mother, sister, and brother with a pruning bill. He was tried and sentenced to hang. His appeal was unsuccessful, but he received a royal reprieve the terms of which provided that he be confined in an insane asylum, where he died by his own hand in 1840. The facts of the crime were never in doubt. Further, young Rivière was commonly known as a secluded, bizarre, troubled, and troublesome person. People who knew him considered him to be deranged in the ways in which village idiots are often perceived to be. Finally, it was generally known that Rivière's mother was an insufferable shrew who made his father's life abjectly miserable through the duration of their unhappy marriage. Information concerning all this comes from witnesses, but the principal source of it is Rivière's memoir. Thus what we know about the crime comes mainly from the murderer's eager and uncontradicted confession. He admits to being an odd fellow, to amusing himself in strange ways; he describes the strong aversion he felt toward women, which combined with the feeling that his mere presence might somehow pollute them; he states that he tended to be quarrelsome and intimidating, that he abused animals, and that he liked to frighten children. All this is presented in an entirely matter-of-fact manner, together with remarks concerning markets, mortgages, plowing, animal husbandry, and other worldly concerns. He does not feel called upon to justify his life and seems perfectly content to let strange conduct or feeling remain strange. The only thing Rivière feels compelled to explain is his crime, and he undertakes to explain it as a crime. He had to deliver his father from the misery which his mother and his sister created with calculating viciousness, and this could be accomplished only by killing them.

He was fully aware of the monstrosity of his enterprise, and he expected to hang for it. But he seemed more troubled by a peculiar difficulty inhering in his task. Though the murder was an act of filial piety, his father must not appreciate it as such; for if he did the familiar torment would only be replaced by the perhaps even greater pain of seeing his devoted son die on the scaffold. Now, the scaffold could not be avoided, but the character of the crime could be transformed, making it appear to be a senseless aberration committed by an immoral monster, a crime from which the senior Rivière would recoil in horror and which would cause him to see his son's just fate as part of the general relief he so much deserved. To this end the parricide included his younger brother in the carnage, an innocent child he and his father loved dearly.

One could say that the case illustrates the circuitous route some take toward self-destruction, a diagnosis confirmed by the eventual suicide. This, or some such diagnosis, might possibly help to update the psychiatric assessment the case received in its own time. One might treat the case as an exhibit in the history of forensic psychiatry, a lesson concerning the union of the concepts of crime and illness and about the ways this union did and does mediate between the condition of human suffering and the powers of the state. Or one could find in the act a cry of pain, an expression of rage felt by an oppressed peasantry unable to attack its real oppressors, blindly turning violence upon itself. These and similar interpretations discussed in the essays are important and interesting, and their possibility amply justifies the publication of the volume. But after viewing the case in its generalized and broadly historical sense, I found an aspect which is unique to it, at least as far as I know.

I believe there is no doubt that Rivière was insane. In fact, as is stated in one of the psychiatric evaluations, if he had been encountered in a mental hospital no one could have dreamed of considering him sane. But we do not find him in a hospital; in fact, apart from some passing glimpses, we do not find him in the courtroom either, nor, for that matter, in any other real place. Instead, we find him in a deposition he made himself. The murders did take place, but the murderer is presented as a literary character, a character in a fable. I do not know whether there exist any other cases of this sort on record, but I am certain that if there are they cannot be many. The facts told are clearly abnormal, but the manner of telling them is exemplary reasonable storytelling. In characterizing the account as reasonable, I do not mean that kind of rational coherence one expects to find in the confessions of clinical paranoids. Instead, I mean that when Rivière speaks about ordinary matters he is practical and realistic; that he tells how one thing leads to another through connections of recognizable contingency; that he finds some things to be arbitrary and baffling and portrays them as such. Further, when in his memoir he reaches the point of telling about the crime, his expressions become solemn and grave, as befits the realization of a tragic but unavoidable necessity.

But if Rivière is a talking fox, and his tale is a fable, what is it a fable

of? It seems to me a fable of the death that almost eluded him and, by extension, of the death penalty itself. In Rivière's own tale, his madness and the madness of his act are plainly revealed in the sanity of his account, that is, he clearly does make sense out of non-sense. In real life the two do not coexist: foxes do whatever foxes do, but they do not talk!

Let us consider what is said and known about the death penalty. The killing that follows judicial sentence is tortuously put together in an institutionalized process in which a large number of functionaries ply their trades as part of a day's work. Every one of those activities is, above all, a standard occupational practice. While they become united into a single and coherent process when someone at long last pushes a lever, generally speaking the actors' doings and intentions are not analyzable by reference to the ultimate aim of a physical act of killing. Since the killing is an extended activity, its sense can be readily composed and made plausible under the aegis of notions of duty and workmanship. This is so because the madness that starts with the manhunt and continues by gradually tightening the noose around the neck of the victim is kept from sight. That is, what we understand and debate about the death penalty stands to what actually happens in the same kind of glossing relationship as Rivière's memoir stands to his life and act. Thus the monster's account mocks the accounts of his judges.

We have no trouble repudiating the murderer's explanations because our interest is with what he did. But we can live with the death penalty because we are not concerned with what is actually done. And when concern for it is raised, it can be set aside as impertinent or sentimental. Yet if detectives could not speak of "clearing cases," and district attorneys could not refer to what they do as "successful prosecution," and judges could not label their part as "sad duty"; if instead they had to be aware of their part in the physical killing of a person, if they had to know the nature of their action as the murderer has to know his act, would we still have a death penalty? And all the rest of us, including those who oppose the death penalty: what if we had to think of it as something that we actually do, rather than as something that just happens?

Not all the abolitionists need the lesson, of course. Anthony Amsterdam, his colleagues in the Legal Defense Fund, and many others, know the realities of legal killing. I believe their ceaseless struggle draws on this knowledge rather than on mere devotion to principles. But the bulk of the abolitionist arguments concern only principles and are thereby emasculated. Most people oppose the death penalty because its effectiveness has not been demonstrated, as if they would abide having it if it were shown to be effective. Or they oppose it because it is administered with disregard for principles of equal justice, not knowing that the death penalty purged of arbitrariness is the greater danger. One never knows, of course, but I believe that the death penalty survives because its madness and evil have been dismembered and hidden—hidden not behind walls but in explanations. The murderer has taught us that he can formulate explanation as eloquently as his judges, and we should not

presume too readily to be able to tell the difference between them. Rivière's sanely reasoned account of his act of madness compels us to look *through* our sanely reasoned conceptions of the death penalty to the appalling realities they conceal. This, I believe, is the chief and very great merit justifying the publication of the book. And for making the case available to us we owe Foucault and his coworkers a debt of gratitude.

Bringing the War Home: The American Soldier in Vietnam and After. By John Helmer. New York: Free Press, 1974. Pp. xv+346. \$12.95.

Norma J. Wikler and Alan Sable

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Bringing the War Home is a study of the political consciousness among American servicemen who fought in Vietnam. A more important topic can scarcely be imagined. The war challenged many Americans' assumptions about our political system; the doubts and confusions that ensued (reinforced by recent political scandals) have still to run their course, and their fullest impact is still to be felt. Among the most deeply affected by the war were the more than 3 million men who fought it. Consequently, a study of their political consciousness is of great interest to all who seek to understand American society and its future. As the first scholarly, book-length treatment of this topic, John Helmer's work is potentially of great importance. Unfortunately, it failed to do full justice to its topic.

The study is based on structured interviews with 90 working-class Vietnam veterans from the Boston area divided into three subsamples: 30 members of the Veterans of Foreign Wars (VFW), designated "straights"; 30 opiate addicts (identified through Veterans Administration programs, methadone treatment centers, and personal contacts), labeled "addicts"; and 30 members of the Vietnam Veterans against the War (VVAW), called "radicals." The book's first chapter describes the military, political, and class nature of the war and the tactics employed in it. The following four chapters contain the bulk of the book's argument and data: chapter 2 sets forth the design of the study; chapter 3 presents the veterans' pre-service characteristics and attitudes and describes their passage into military life. Their war experiences are analyzed in chapter 4 and their "homecoming" in chapter 5. A final theoretical chapter seeks to develop the relationship between alienation, rebellion, and class consciousness.

This is an informative and interesting book. It contains an extensive amount of socioeconomic and attitudinal data. In addition, Helmer has done a thorough job of combing the literature and makes good use of relevant material from other studies. Finally, interspersed throughout the book are passages of genuine insight and/or provocative analysis. Particularly impressive are Helmer's analysis of the class nature of the war and his effective discrediting of Lipset's notion of working-class authoritarianism. In spite of these strengths, however, the book as a whole is disappointing, for the author never succeeds in making his main argu-

ment clear or convincing. Indeed, it is exceedingly difficult to tease the argument out of the massive amount of material and the frequent digressions. When the diligent reader does so, what emerges is generally facile and unconvincing.

Helmer's primary concern is to explain the soldiers' differential responses to their Vietnam experiences—why some men become straights, others addicts, and still others radicals. The appropriateness of this tripartite classification is hence of prime importance to Helmer's argument. But a careful reading of the text and tables calls its validity into question. First of all, the three categories appear not to be mutually exclusive. For example, table 5-38 reveals that more addicts than radicals identify their political ideology as revolutionary. Next, too much is inferred simply from group membership. Uniform political ideologies and social attitudes are attributed to the straights and radicals on the basis of their membership in the VFW and VVAW. This contradicts the well-established sociological fact that the opinions of rank-and-file members of organizations often differ markedly from those of the leadership. And Helmer neglects even his own data which reveal that the ideological self-identification of his straights varies from "liberal" to "strongly conservative" while that of his radicals varies from "moderately conservative" to "revolutionary." Thus, the careful reader is left wondering if Helmer's tripartite schema is a valid representation of the range and content of the veterans' ideological responses to the war. We suspect that it is not.

Even if one accepts Helmer's categorization scheme, his explanation of how and why these men ended up as straights, addicts, or radicals is unimpressive. The author states that nothing in the preservice backgrounds and experiences of his subjects accounts for their differing responses to the war, and the data he presents support this assertion. What does make a difference, he argues, is the primary groups to which men attached themselves while in Vietnam and their subsequent experiences in the United States. Helmer holds that all combat soldiers in Vietnam joined one of two mutually exclusive groups: the "juicers" (beer or liquor drinkers) or the "heads" (marijuana or opiate users). In Helmer's model, membership in these groups is the all-important determinant of later ideological development. The juicers, whose primary groups allegedly supported the legitimacy of the war, experienced little or no personal or ideological alienation or conflict upon their return to the United States. They became the straights. By contrast, the heads, whose primary groups Helmer holds were the source of questioning the war's legitimacy, became transformed into either addicts or radicals when they returned home, depending upon whether or not they were addicted to heroin or joined the VVAW. Addicts and radicals shared a deep disaffection with the war and American society, but they expressed their disaffection in different ways.

Although plausible when stated in this summary fashion, the model is unconvincing in the book because its basic assertions are unsubstantiated by data and/or adequate analysis. Indeed, Helmer's argument is flawed at its very core. Because he could find no explanation in the data

he collected, Helmer argues that the original all-important bifurcation of soldiers into juicers and heads was arbitrary. While it is possible that the choice of primary group was random with respect to the variables that Helmer investigated, (e.g., socioeconomic background, etc.), it is improbable that factors such as personality, tastes, and life-styles (which Helmer did not investigate) played no part in determining who became a juicer or a head. Further, Helmer imputes to these primary groups an intensity, exclusiveness, and degree of social solidarity that is not supported by other descriptions of primary-group ties among American soldiers in Vietnam. Moreover, the author offers no detailed description of the interpersonal, ideological, and social dynamics of these primary groups. Hence, the mechanisms by which political consciousness developed are never explained.

A frustrating characteristic of this book is that Helmer takes the reader through elaborate convolutions of thought and data, often in pursuit of very simple points. This is particularly irritating because the author himself frequently overlooks some obvious implications contained in the data. Indeed, he may have missed the most parsimonious explanation for differing ideological outcomes of the war. Tables 5-1 and 5-2 imply important differences between the straights, addicts, and radicals with regard to both the dates of their service in Vietnam and the amount of time they spent in the armed forces after returning to the United States. Not surprisingly, the straights served earlier in Vietnam than either the addicts or the radicals, and they had a higher proportion of men released from service immediately after returning from Vietnam. Is it not possible that the differences between Helmer's groups reflect to a great extent the fact that massive drug use and political disaffection greatly increased during the later years of the war? Further, it is unwise to ignore the possible effects of such things as G.I. coffeehouses and antiwar newspapers upon soldiers—primarily the addicts and radicals, according to Helmer's data—stationed on American military bases after service in Vietnam.

No review of this book can be complete without comment on its theoretical pretentiousness. The author employs all the currently fashionable "Marxist" terms: alienation, class consciousness, even Rosa Luxemburg's concept of "spontaneity." Yet a careful reading once again reveals little substantive analysis behind the author's all too facile flow of words, and one even wonders whether he properly understands some of the concepts he is using. (Rosa Luxemburg's notion of spontaneous mobilization, for example, seems light years away from Helmer's "spontaneous" groupings of juicers and heads.) The final chapter, entitled "Alienation and Rebellion," is particularly disappointing with respect to the strength and substance of its argument. The reader gains little insight into the important questions of why alienation did not lead to rebellion among the men who fought in Vietnam and why they did not bring the war home.

Condorcet: From Natural Philosophy to Social Mathematics. By Keith Michael Baker. Chicago: University of Chicago Press, 1975. Pp. xiv+538. \$22.00.

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Nineteenth-century social thought owes much to Condorcet, whose rationalist optimism provided a foil for Comte, Malthus, Marx, and Saint-Simon, among others. Condorcet is best known for his *Sketch for a Historical Picture of the Progress of the Human Mind*, which is a kind of epitome of Enlightenment thought. In it, the earlier stages of human social and intellectual development are mapped out and the tenth—still to come—predicted: scientific and technological progress were indisputable, and social and moral progress appeared ineluctable. Though he wrote it in hiding from the Committee of Public Safety, after his death the *Sketch* was widely promoted by the Convention and became an apologia for the Revolution.

Keith Michael Baker undertook to recapitulate Condorcet's intellectual development, from his years as a mathematician and rising star in the Royal technostructure through his shift to "social mathematics" to his experience as an actor on the stage of the Revolution. The product is neither biography nor psychohistory, nor is it social history—there are passing references to social and political events of the time but they are not central to the account. It is, rather, a sort of chronological distillation of Condorcet's thought and that of many thinkers which is roughly contemporaneous or topically related. For those who do not wish to read Condorcet for themselves, Baker's intellectual biography serves as a set of *présis* that interpret and explain these works in considerable detail.

The book has two main parts: "The Scientific Model," in which Baker describes the development of Condorcet's idea of social science, and "The Social Field," Condorcet's attempts to apply these principles to the social problems of the Revolution, particularly in his capacity as a member of the Assembly charged with studying the French educational system and recommending a plan for its reorganization. In the concluding chapter of the second part, Baker provides a novel interpretation of the *Sketch*:

Treatment of Condorcet in the standard histories of sociology—such as it has been—has almost invariably rested on a brief characterization of the doctrine of progress found in the *Esquisse* [the *Sketch*] as a powerful (if flawed) anticipation of the historical sociology established by his successors. . . . To read the *Esquisse* in the light of what Saint-Simon and Comte made of it, rather than in the context of the whole body of Condorcet's writings, is to risk a fundamental misinterpretation. . . . Condorcet's fundamental purpose was not to present the laws of a historical sociology that would threaten to swallow up action but to demonstrate man's freedom to advance human progress through the conduct of the rational social art. [P. 344]

In short, the book is a compendium of Baker's interpretations of Condorcet. Parts of it, particularly the chapter on Condorcet's tutelage under d'Alembert in the Académie des sciences and the chapter on the *Sketch*, are interesting if not easy to read.

The major weakness of the work is simply that it is not clear why we ought to read Baker rather than Condorcet. While Condorcet's writings are voluminous, it would seem that there are representative writings—the *Sketch* and the *Essai sur l'application de l'analyse à la probabilité des décisions rendues à la pluralité des voix*, for example—which will provide the interested reader with an exposure to Condorcet's ideas. If, in order to refute those who have simply seen him as a precursor of Saint-Simon and Comte, a reinterpretation of Condorcet's intellectual development is necessary, then surely an article or two would have sufficed. A biography would have been invaluable as an adjunct to reading Condorcet, but this is not biography. It is a compilation of interpretations that is indisputably scholarly but whose necessity is questionable.

The documentation is staggering. There are 86 pages of notes and a 38-page bibliography whose primary sources include Condorcet's correspondence with Turgot and with his mistress, the archives of the Académie des sciences, and the vast mathematical and scientific work—published and unpublished—of Condorcet himself. One drawback of the bibliography is that it contains no listing of Condorcet's works that are available in translation. The only translation listed is of the *Sketch*; it would be useful to know of others.

Baker appears to have allowed this documentation to get the upper hand. His chapter on Condorcet's prerevolutionary years presents observations about his personal life, intellectual development, and relationship with d'Alembert, and about political developments and academic intrigues in the Académie des sciences, all tied together in the same bundle. The subdivisions are chronological but do not separate the various levels of history into analytically homologous subdivisions. It is not that Baker has tried to do too much; rather, the oceanic source material has overflowed and obscured the logical boundaries that would make its absorption manageable.

The discussion of Condorcet's development is often accompanied by long and detailed analyses of positions taken by contemporaries and predecessors. Though these examinations provide a kind of microhistory of ideas, it is not always clear that they are related to Condorcet's intellectual maturation. For instance, in describing the genesis of Condorcet's ideas on probabilities and their relation to positivism, the positions of Locke, 'sGravesande, and Hume are recounted in sufficient detail that the discussion serves as a guide to these thinkers all by itself. Is this really necessary? Baker seems to have some doubts as he says, "Apart from the difficulties involved, the idea of devoting to the Scottish philosopher a substantial section of a study of Condorcet will doubtless appear gratuitous to some readers" (p. 139). Indeed it does, particularly since in the para-

graphs which immediately follow Baker questions whether Hume exercised any significant influence on Condorcet.

It is an erudite work into which a huge amount of scholarly effort has been poured. The chapter entitled "The Esquisse: History and Social Science" is a valuable companion to reading Condorcet. Yet in the end I am not convinced that the manifold digressions are necessary, nor that a reinterpretation of Condorcet's thought could not have been presented in a tightly organized article.

Of Time, Passion, and Knowledge: Reflections on the Strategy of Existence. By J. T. Fraser. New York: George Braziller, Inc., 1975. Pp. xiii+529. \$20.00.

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For the social sciences, time has been a problem of enduring, if not central, concern. However, as our earlier preoccupations with time conception and reckoning give place to a growing interest in the way social activities are temporally lodged and organized, the dimension of time comes into more central focus in the analysis of social structure and process. The strategy of treating time as at once a resource and principle of social organization is exemplified (though certainly not exhausted) in economics by the work of Staffan B. Linder, and, in sociology, by Wilber Moore, Barney Glaser and Anselm Strauss, and many others. In addition, recent and representative statements by Paul Fraisse and Leonard Doob reflect a renewed interest in the psychological dimensions of temporality. Given such developments, it is proper that a sociology journal should now welcome the appearance of a new book by J. T. Fraser on the philosophy of time.

In this long, dense, highly abstruse work, Fraser argues that each level of the integration of matter displays its own unique temporality. Time comes into view as an aspect of the "causalities" which animate different types of organization. The substance of this argument, which is evolutionary in character, is that "what we ordinarily call 'time' comprises hierarchically organized temporalities, each contributing something different to the temporal experience of man . . ." (p. 5). The lowest point in this hierarchy, says Fraser, is the *atemporal*, which characterizes those "acausal" universes, like the electromagnetic, wherein no known process can identify the temporal separation of two events. In such a universe everything appears to occur simultaneously. *Prototemporality* is defined by the emergence of indiscernible particles which display patterns in the aggregate but which, singly, bear no evident causal relation to one another. *Eotemporality* appears with the development of more complex physical entities which, though connected to one another in a deterministic before-after sequence, lack a preferred direction. Eotemporality thus involves

"pure succession" and is instanced in the timeless (as opposed to atemporal) "steady states" of repetitive and cyclical phenomena.

At length there is a radical break in the evolution of matter as biogenesis ushers in living forms, constituted in part by "physiological clocks" variably keyed to environmental *Zeitgebers* (cues), and developing (non-repetitively and unpredictably) in the direction of manifold and ever more complicated structures which in turn give rise to aging and death. All of these properties are facets of the final causality of the *biotemporal* plane. Finally, with the emergence of the human brain ("the most complicated structure in the universe") and mind (a "process description" of the brain) *nootemporality* makes its appearance. This most advanced form of temporality finds expression in the "mental present" of symbolically mediated experience, long-term memory and expectation, personal identity, the distinction between causation and intention, and, above all, the knowledge and dread of death, which, in matters temporal, is the uncertain measure of all things. Death as knowledge felt (with passion) as well as understood (through reason) is at the core of the unresolvable conflict between being and becoming and so represents the existential source of man's sense of time.

In its developmental aspect, says Fraser, the successive levels of this temporal hierarchy are built up around what is undetermined by previous levels; therefore, while each phase exhibits unique properties, each must also be consistent with those of the less advanced levels of integration. Functionally, the emergence of a temporal level is a way of resolving conflict built into the next lower level. In this sense, temporality is an instrument in the "strategy of existence." But lower-level conflicts, for example, the tension between increasing and decreasing entropy, are only resolved into dilemmas of a higher order, like the expected and encountered of life forms generally, and the noological dilemma of being and becoming. Thus, as the evolutionary scale is ascended, existential conflicts regarding time become increasingly intense and pervasive.

On the other hand, Fraser argues that temporal conflict may be resolved through regression as well as emergence. Thus, the individual can be transported to the eotemporal realm by obsessive repetition compulsions, which transform life into a timeless and directionless ritual. Prototemporality is lodged in the unconscious itself, whose contents defy chronological arrangement (they are discernible, says Freud, but not ordered). On the collective level, existential conflict may be reduced by regression into the eotemporal ecstasy of the dance or the prototemporal world of the absurd which, as depicted by Beckett and Kafka, is populated by men without memory or intention, men whose mental presents exceed no more than a few seconds, and whose acts admit of no coherence or causal connection. Similar regressive forms, Fraser shows, are to be found in other contemporary arts and in the worlds which they depict.

A useful summary chapter brings this book to its conclusion.

Fraser's work succeeds in showing that the problem of time is not exhausted by issues relating to its conception, reckoning, or measurement.

And in the process of linking temporality to organization in general, it convincingly demonstrates the centrality of time in the particular makeup and capacities of the human mind. The book also highlights the necessity of bringing multiple methodological perspectives to bear in the study of time. This is one of Fraser's major concerns. While the physicist's concepts and related mathematical tools fully capture the temporal dimension of subbiological realms of matter (and even their "residual" presence in the bio- and noological realms), there is no one-to-one correspondence between the properties of number systems and what is unique (and, therefore, most important) about the more complex, higher integrative levels of life and mind. Mathematics is queen, says Fraser, but only in her own realm—and that is the lower, "cruder" realm of the mindless and non-living.

These points embody some of the merits of the book—and they are in my opinion very substantial merits. However, there are limitations too. The main weakness of Fraser's book is that while it recognizes social and cultural systems to be emergent with respect to the individual mind it posits no level of temporality specific to them. Indeed, the temporality of the social is said to consist of aggregated individual temporalities. Social time is no more than individual time writ big (Fraser thus refutes his own thesis that new temporalities are constituent features of emerging organizations). The author should, and probably does, know better. For if nootemporality relies upon the unquestionably social fact of language, as Fraser says it does, then it must presuppose the organization, as well as the existence, of mind.

A rude, pre-Meadian conception of mind and society, and a resulting failure to distinguish systematically what is unique about their respective temporalities, grossly detracts from the sociological worth of this book. To be sure, Fraser comments now and then on how the specific aims and organization of society presume certain temporal strategies, that is to say, ways of mapping and structuring activity in terms of time. But he skirts the theoretical issue of whether these collective strategies are emergent from and independent of the nootemporal features of the individual strategist. Comments on this question are never more than comments in passing; but the answers they contain are always stated in the negative. What we are faced with, then, is a hierarchy of temporality whose terminus is in the individual. From a sociological vantage point, this conception ends where it should begin.

Despite this shortcoming, Fraser ought not to be ignored. He is worth reading for many reasons: for his unique ability to combine analytic rigor with imaginative speculation, his varied store of insights, and, above all, the enormous sweep of his scholarship, which positively humbles those of us who have selected the more narrow path. And if his book is something less than a direct source of insight into the social aspects of time, it is also much more than that. It is an impressive attempt to formulate a general theory of time. This grand enterprise succeeds in reaching, but then tauntingly falters at, the borders of sociology.

The Reality of Ethnomethodology. By Hugh Mehan and Houston Wood. New York: John Wiley & Sons, 1975. Pp. x+259. \$11.50.

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This book has three great strengths: it provides rich and full examples of what ethnomethodologists do, it is written well, and it avoids a distracting critique of traditional sociology. By presenting a straightforward account organized within a coherent structure, Mehan and Wood create the first "textbook" in ethnomethodology.

The authors' concern is to display the current state of the field. They do this by describing the studies of ethnomethodologists within a framework which helps both to clarify the material and to underscore the central problems it addresses. They also describe some of the conflicts and debates among ethnomethodologists over each other's solutions to common problems. Mehan and Wood do not present their book as a definitive account nor, primarily, as a critique of these studies. Rather, they are concerned to describe the multiplicity of approaches within ethnomethodology.

Described are studies of the realities of children (and their examiners), social science, and logic; studies of the nature and construction of social rules in bureaucratic, educational, legal, medical, and everyday settings; studies of naturally occurring language practices of everyday speakers, such as categorizing and describing; studies of conversational structure, of social order, and of reflexivity; studies of social knowledge, interpretive procedures, and reality construction; and analyses of the grounds of speakers' and writers' statements. Mehan and Wood make clear that this work is largely empirical, using ethnographies, conversation analysis, interviews, laboratory work, and demonstrations.

One study, that of children's reality, is both simple and revealing. The fact that children take standardized tests is used to gain insight into their world. Children are interviewed after taking such tests and asked why they answered questions as they did. Their responses provide an insight into the processes by which children reason which are neither tapped by the standardized tests nor otherwise discussed in the literature. The authors indicate something of their approach to presenting this material when one states, "Whenever feasible I have presented the researcher's own materials along with my interpretations. Readers are encouraged to create their own interpretations and to contrast these with mine and with those of the researchers discussed" (p. 6).

While part 1, "A Display of Ethnomethodology," and part 2, "Studies in Ethnomethodology," are clear and comprehensible, this is not true of part 3, "A Prospectus for Ethnomethodology." Here the authors delineate their own position within ethnomethodology by reference to established theories and philosophies. Unfortunately they adumbrate these viewpoints to such an extent that the reader cannot adequately comprehend them or the relationships among them. It is a disappointing section, too often

confusing, opaque, and glib. This discussion should either have been dropped or greatly expanded.

Methodologically, the dominant impression conveyed by the book is one of enormous concern on the part of all the ethnomethodologists discussed with the procedures they use to reveal the social world. This self-consciousness about procedure is a theme which unifies studies otherwise diverse in style, technique, and topic. This concern also represents a main source of the power of the ethnomethodological studies, because the detailed explication of procedure is carried out *for its own sake, for what it reveals*, and not in order to legitimate the work in terms of established, authoritative canons such as "the rules of scientific method." Ethnomethodological work is justified in terms of what it reveals about the social world, rather than by its conformity with "correct" procedure. This, of course, represents a major divergence from much of sociology and suggests in what way ethnomethodology is committed to objectivity. By demonstrating *in detail*, and *in each case*, how the analysis is done, ethnomethodologists strive to make explicit their own assumptions and procedures and those of the people they study. To reveal assumptions and procedures is to reveal the social world.

By showing what has been done by ethnomethodologists, rather than by writing programmatically, Mehan and Wood demonstrate what for me is the central import of the book: the work of ethnomethodologists is enormously rich and varied. While this work deals with a wide range of problems, in many different and often opposed styles, it is unified by the concern for the explication of assumption, procedure, and method. The *tone* of ethnomethodology has often suggested a closed, cultish, sectlike quality. It has sometimes alienated readers. But this book demonstrates that the *work* of ethnomethodology—which is, after all, what really matters—is open, comprehensible, and inviting.

Phenomenology, Role and Reason: Essays on the Coherence and Deformation of Social Reality. By Maurice Natanson. Springfield, Ill.: Charles C. Thomas, 1974. Pp. ix+357. \$12.75.

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"Redundant" is the main, and almost overwhelming, response to this collection of 16 essays, all except one of which were previously published. The exception is a paper commissioned by the Russell Sage Foundation through the offices of the Society for Health and Human Values. The essays (most read like they were papers prepared for oral presentation) are grouped into three parts. Though the parts are not labeled they presumably relate to the main title and deal, respectively, with phenomenology, role, and reason.

Part 1 consists of five papers on the basic ideas and concepts of phenomenology found in the work of Edmund Husserl and Alfred Schutz. One

wonders for whom these essays were resurrected from the professional journals in which they were originally published. They are too technical for the beginner in that they assume some understanding of concepts like "eidetic structures," "transcendental consciousness," "noetic-noematic unities," "intentional consciousness," "reduction," "typification," and a host of other very difficult concepts. It is apparent that this first part is somehow a prerequisite for the two parts which follow, but it is difficult to see how an understanding could be achieved with these particular papers. On the other hand, someone already familiar with these concepts would probably find these presentations uninteresting.

Nevertheless, there are some fundamental and important concerns that run through part 1: the problematicity of mundane reality, the transcendental structures that make that reality possible, the difficulties of understanding and pursuing the ever-elusive and ubiquitous process of typification. Central to any discussion of typification are questions about the nature of abstraction and ideation as the basis for constituting social reality. Arguments presented concerning the primacy of the abstract in the process of typification are too brief and uninformed about current developments in psychology, especially psycholinguistics. More closely reasoned and powerful arguments for the validity of the *a priori* (transcendental?) have emerged during the last 15 years as a result of the work of ethologists like Lorenz, linguists like Chomsky, and anthropologists like Lévi-Strauss. The work of Piaget would also have to be included as of great importance with respect to elucidating the nature of our abstractive powers, powers which Natanson rightly persists in pointing to as the basis for our modes of sociality. Perhaps it is unfair to expect these developments to be included in a recently published book by a phenomenological philosopher, but their absence is felt nevertheless.

Presuming fundamental concepts have been grasped, the six essays of part 2 attempt to weave these concepts into some interesting patterns with respect to the notion of social role. Whether or not the concepts of part 1 have been fully grasped, most sociologists will find the essays of part 2 at least provocative and occasionally unexpectedly insightful. They have an existential orientation with respect to phenomenological concerns. This orientation is especially evident in the treatment of anonymity and recognition and alienation. Again, typification is the touchstone by which roles are possible and necessarily generate the intersubjective world or reality with its fundamental characteristic of anonymity which, paradoxically, is the basis for the recognition of self and other in their individualized uniqueness. It is in these essays, as one would expect, that G. H. Mead's thinking is hybridized with that of Husserl and Schutz, with some not uninteresting results. Natanson is careful to stress that his concern is not so much with the varied actual manifestations of social phenomena as with the conditions that make them possible. In this, of course, he is offering a knowledge, or at least a realm of inquiry, that is scientific but which does not grotesquely distort that which is to be known. This, one must suppose, is the perennial quest of phenomenology.

"The career of reason, traced out in irregular outline," is how the book cover describes part 3. It consists of five essays having mainly to do with esthetics and ethics or, more generally, problems of valuation. Because each of these chapters focuses on a particular valuational problem, the common characteristic that provides the rationale for putting them together is their treatment of the specific concerns from a phenomenological framework of thought. Perhaps the titles of the chapters can convey the topics of concern: "The Fabric of Expression," "Philosophy and Psychiatry," "Benefit and Experimentation," "Disenchantment and Transcendence," and "On Conceptual Nihilism."

The repeated treatment of certain themes (mundanity, typification, etc.) in these papers gives them, as a whole, the unfortunate characteristic of redundancy. This is, of course, due to the fact that they were all separately written. One might question seriously the rationale for putting them together: the book as a whole is probably too elusive for beginners and not substantial enough for the specialized professional. But perhaps there is a type of person, resembling a tourist in Chinatown, interested in what he sees but not sure of what it is, for whom the book might be useful. And, like a tourist, he might be brave enough to plunge in and try a "dinner" in which interesting, but not fully comprehended, entities are consumed with the feeling that one is on the verge of an illuminating discovery.

Doing Field Research. By John M. Johnson. New York: Free Press, 1975. Pp. xiii+225. \$9.95.

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In the course of my graduate training, apart from learning such indispensable tricks of my future trade as how to correct undergraduate papers, run the Xerox machine, and apply the principles of survey research, I had occasion to do a fair amount of house painting. This was done as an early exercise in what has also become a continuing professional concern of mine, known in some quarters as "making a living." While it did not occur to me at the time, house painting was to provide me with a useful framework for viewing the sociology of sociology.

During my apprenticeship, all of house painting, like all of Gaul, was divided into three parts. The three main coverings were "high gloss," "semigloss," and "basic flat." It seems to me that all of sociological inquiry might well be categorized in this way. Indeed, I might even say that the paint metaphor is truer to the phenomenon than other more fashionable characterizations such as "micro" or "macro." In any case, these three kinds of paints had certain characteristics. High gloss was heavy duty and wore well; it was good at covering blemishes, best at weathering abuse, initially expensive (but cheapest in the long run), and relatively easy to apply. Semigloss glared less, covered well, and was also quite durable. Flat was cheapest initially (but more expensive in the long

run), did not cover as well (blemishes showed through), and, worst of all, did not wear well.

These three coverings, applied to sociology, offer, I think, a pretty fair assessment of the field. High gloss theories (1) wear best. That is, whether micro or macro, such formulations are presented so as to explain everything and nothing at the same time, and they are not easily given to "empirical disproof." Because there is no way to refute (or validate) them, they have great staying power. High gloss (2) covers best. That is, the higher the gloss, the less the underlying blemishes show through. And high gloss (3) offers for some customers the greatest visual appeal—the most glitter and the highest blinding effect. Because of the above, high gloss is (4) easy to apply, and (5) it offers for the sociologist in all of his more pragmatic pursuits the least-expensive covering in the long run.

Notwithstanding all of these professional advantages of high gloss, there has recently been a trend in sociology back to the use of basic flat. The use of flat, while it was at first embarrassing—the blemishes showed through, it did not wear well and was in need of constant repair, and was difficult to apply—proved to have certain assets, given that a major goal of sociology was to become a "social science." The first guiding principle of science is that one ought not hide from the truth, or hide it from others. In short, while it is discouraging for the rhetoric of science, it is useful for the progress of science to admit what you do not know. In this regard, coverings that reveal most by covering least, and best allow for "what you see is what you've got," are best in the long run. With this in mind it is not surprising to find that "flats" are coming back, in the world of sociological fashions.

One very good flat book is John Johnson's *Doing Field Research*. It uses the old-fashioned flat (not to be confused with the better covering, cheaper, and easier to apply miracle synthetics). What it gains by being flat-honest, it does not lose by being flat-dull. The work is well written. It is a book about doing field research and, much to my amazement, actually describes in some detail how it gets done. While many books on fieldwork describe "how to do it" in principle, abstractly, or how it ought to go ideally (subject to certain constraints presented, again, in the abstract), few actually describe in any detail how a real piece of research gets done. At first this may seem peculiar, since nearly everyone concerned with ethnographic research pays lip service to the need for such disclosures as a basis for assessing the work in the first place.

A second glance is all that is needed to recognize why such revelations are scarce in the literature, not only for those doing field studies, but for those applying the more "scientific" methods one associates with survey research or controlled laboratory experiments. After all, detailed descriptions of this kind are embarrassing, since as anyone who has ever conducted social research of any kind well knows, nothing ever seems to go quite as it should. This was true of Johnson's study of social welfare agencies. In fact, while the study offered some interesting revelations about the inner workings of welfare agencies, it is not what was studied

or what was found that is of prime interest: it is the uncommonly honest description of how the research proceeded and how the findings came to be revealed, that makes this an important book.

The book is divided into eight chapters with conventional-sounding titles such as "Gaining and Managing Entree in Field Research," "Developing Trust," "Observing, Recording, and Analyzing in Field Research." The unconventional nature of the book may be surmised from the titles to the first and last chapters: "Participant Observation, Field Research, and Objectivity in Sociology" and "Reconsidering Objectivity in Sociology." In fact, the *Leitmotif* of the entire book is to compare and contrast "how it's supposed to go," with "how it actually went" when Johnson conducted field research in five social welfare agencies. Most of the study concerns his observations and interactions in one of them.

This contrasting of real and ideal working practices of the fieldworker confronted with real situations is not presented as it usually is in "cook books" on doing field studies, that is, in the high gloss or semigloss mode. Instead, the descriptions are rendered in basic flat with all their irregularities clearly visible. However, the blemishes are not presented as something in need of, or capable of, being repaired. The recommendation is not for the researcher to work on making the real fit the ideal in order to achieve greater objectivity and scientific rigor. Rather, the reader is asked to recognize that the blemishes are not blemishes at all, but a part of the natural texture of the work. It is the current model of how field research ought to go that is in need of repair, not the way the research goes. In fact, the research goes as it must, subject to certain natural constraints that stem from the context of the inquiry itself. They cannot be "sanded away" in order to achieve greater cosmetic appeal. It is the recognition and display of these blemishes and irregularities that give the work an artistic quality and charm not found in other smooth, machine-turned works of this kind.

I highly recommend this book for courses in methods, field studies, and qualitative sociology.

Socio-Technical Design: Strategies in Multidisciplinary Research. By P. G. Herbst. New York: Barnes & Noble, 1974. Pp. x+242. \$16.00.

Michael S. Flynn

University of Michigan

The message of this book is quite clear: as humans, we need new organizational forms within which to pursue our individual and collective purposes; as scientists, we require new perspectives and methods for the integrated analysis of the influence of social and technological environments upon human behavior. These two concerns are by no means unrelated for, at least in part, it is in designing solutions to problems implied by variables of analytic concern that these new organizational forms will emerge.

Part 1 briefly reviews the development of sociotechnical research and

design and then exemplifies this approach in an extended analysis and formulation of new designs for shipboard organization. The major points made are that the technological imperative is dead, that organizational designs must jointly optimize responses to technical, social, and economic constraints, and that resulting organizations must have internal capacities for change and development. Designs which allow this typically turn out to be ones which specify minimal necessary structures rather than fully elaborated systems. These will normally involve autonomous work groups within a matrix organization, allowing fluid formation of groups for specific tasks. Factors ranging from the rate of technological change to the loneliness of the long-distance sailor give rise to a number of design imperatives for shipboard organizations. These include, among others, a matrix structure, a better-educated crew, briefer periods at sea, and more involving work. Herbst's analysis is interesting in its details, but the solution he offers—a mixture of remote shore-based control and the recruitment of sailors who are professionals and officers and thus, hopefully, gentlemen—is symptomatic of what seems to me to be a major flaw in his analysis of existing organizations and his suggestions for their redesign.

The analysis underplays the extent to which typical organizational design may indeed be a response to more than a technological imperative. The hierarchical coordination and control one may find in a work group, for example, may reflect constraints of an available work force as much as a technological imperative. Herbst's own solutions to design problems, on the other hand, frequently involve a "professionalized" work force—either its recognition within or its recruitment into the organization. This comes close to an individual imperative which, I suspect, most sociologists will find even less palatable than the technological imperative it replaces. Moreover, the fate of an organization which does not have and cannot recruit such a work force is left unclear. Will it of necessity maintain the traditional organizational form, or will redesign produce a "professional" work force sufficiently large to go around? This comes close to an organizational imperative, perhaps no more satisfying than its technological or individual alternatives. I suspect most readers will find this portion of the book useful and of interest for its insights and evaluations, rather than for its theoretical clarity, coherence, and originality.

A behavioral scientist who is committed to a multidisciplinary view of the world must eventually consider how to combine fragmented perspectives, valued for their divergences, into a theoretically satisfying unit. In part 2, Herbst argues that no event is uniquely tied to a particular mode or level of analysis. The differences we find in events which lead us to label some "psychological," others "sociological," and so on, are largely a function of our initial choices of operational units (defined in terms of operations producing system changes). But if any event is analyzable from many perspectives (or operational units), with each such analysis resulting in a partial theory of the event, how are we to integrate these partial theories into a meaningful whole? Herbst maintains that we need to develop measurement techniques which allow us to treat simultaneously

variables within different operational units, and he discusses a psycho-physical transformation which he feels offers promise for integrating physical and psychological response variables. His discussion is interesting, but not compelling. The image he wishes to convey of science as it ought to be is unclear at some crucial points, and his arguments might have been better served by the inclusion of more empirical materials on the psycho-physical transformation.

Part 3 applies the concepts and imperatives developed in earlier chapters to a variety of work settings: production, research, and education. Herbst presents a nicely detailed examination of the interplay of technical requirements and design features and continuously reminds us of the need for matrix organizations with a capacity to learn. However, exactly how we might design these organizations is still not clearly revealed—we see the problematic aspects of current forms and the utopian aspects of the new, but the operation of getting there from here is not adequately described. The epilogue of the book reasserts that we have choices to make with regard to how we structure our work lives, and these choices will affect what we become.

This is a useful book, and I would recommend it both to organizational specialists and to sociologists who find themselves hitting against disciplinary walls in their attempts to explain behavior. It may not provide solutions, but the multidisciplinary emphasis, with clear attention paid to problems of cross-disciplinary analysis, is thoughtful and welcome. The book's major failings are ones it shares with many. Tied to an image of what ought to be, the analyses of what is and what can be are frequently flawed. The design suggestions are theory based, but the exact derivation from the theory or perspective is unclear. How these design suggestions differ from those derived from other theories is not made clear, nor am I persuaded that that can be done.

Toward Social Hope. By Theodore Caplow. New York: Basic Books, 1975. Pp. viii+229. \$11.95.

William H. Form

University of Illinois at Urbana-Champaign

The main point of this book is that an effective social technology is currently available to solve societal ills. In Theodore Caplow's words, "Social improvement—that is, social change in a desired direction—is entirely feasible under contemporary conditions . . . provided that the social inventor respects the requirements of technology by specifying the existing conditions, the means to be used in getting from one to the other, and the intermediate conditions to be traversed in the process" (p. 10). In the first five chapters, Caplow briefly traces the history of ideas on social improvement from Hobbes, Locke, and especially Rousseau, who first suggested how people are shaped by society. The French Revolution is interpreted as the first major attempt to reshape society's institutions;

the Congress of Vienna as the mechanism to reassert the legitimacy of traditional institutions. The 19th century is seen as an era of increasing human control over the physical environment and increasing belief, by the end of the century, in the beneficial effects of modernization on all institutions, a belief exemplified in the Spencerian and Marxist notions of inevitable progress. The ideas of three late-19th-century futurologists are reviewed. (Bellamy's predictions about societal institutions were completely erroneous, Durkheim's prediction about the inevitability of societal integration was incorrect, and only Kautsky provided a blueprint for communism as a single gigantic firm.) Then Caplow interprets three dominant 20th-century trends (growing nationalism, war, and revolution) as attempts at societal transformation or control.

But such macroscopic views of events, Caplow asserts, do not help us understand how social change occurs unless we analyze it from the standpoint of those who organize social movements. If these movements are viewed as projects on all levels—from the world community to the individual—they can be readily classified as purposeful, accidental, or inexplicable, on the one hand, and as varying in degree of success, on the other. The succeeding eight chapters describe and evaluate a wide range of projects. Rural-urban migration is evaluated as a successful project organized by individual families, while modernization of nations, the Russian communist revolution, Western imperialism, and the conduct of war are seen as successful national projects. Russian efforts to promote world revolution, the attempts of Napoleon, Hitler, and the United States to dominate world politics, and the efforts at organizing parliaments of nations are evaluated as unsuccessful projects. Three vintage American movements (abolition, the settlement house, and prohibition) are analyzed as citizen-initiated projects whose technical deficiencies actually created new social problems. The entire range of New Deal projects is reviewed (regulatory commissions; direct subsidies to industries, farmers, and the aged; housing; public employment; social services; and community development) for lessons to be learned about successful and unsuccessful government projects. The War on Poverty, social improvements bestowed by the courts (civil rights, electoral reform, and protection of the rights of criminals, children, and women) are similarly reviewed.

In the final chapter, Caplow urges Americans to undertake projects at all levels (local and national, private and public) which conform to the requirements of successful projects, namely, "An accurate description of conditions to be changed, a careful and honest description of the end-conditions to be achieved, a division of the project into successive stages, a description of conditions to be achieved at each stage, practical methods for getting from each stage to the next, estimates of the time, personnel, material resources and information required to get from each stage to the next; procedures for measuring goal attainment at each stage, and procedures for detecting unanticipated results at each stage. Plus a measure of humility, an awareness of mortality and abundant good will" (p. 215).

For the undergraduate student who has little knowledge of social move-

ments and no systematic framework to evaluate them, this book fills a needed gap. The profiles of the projects are too truncated to go beyond newspaper descriptions (most movements are described in less than five pages). But Caplow does not intend to provide complete summaries; he sees his contribution in the evaluation of conscious projects to change society. Occasionally, as in the case of the student rebellion, the description and evaluation of the movement are brilliant. Occasionally, as in the case of the Works Progress Administration and the War on Poverty, both description and evaluation are inadequate. Readers will probably evaluate the treatment of projects in terms of their knowledge about them; the more extensive their knowledge, the more critical they will tend to be. Caplow may admit that no one can know all social movements in detail; the task is to grasp their essence and to locate the main reasons for their success or failure. He succeeds more in grasping the essence of movements than in analyzing their success or failure. Therefore, the contribution of the book must finally rest on the adequacy of the frame of reference for evaluating project success or failure.

Caplow has two frameworks: a formally acknowledged one, summarized in the second quotation above, and an unacknowledged one, adapted to individual projects. The acknowledged framework is the familiar means-end scheme so successfully used by managers when they control the variables which affect their planning. Its application to social projects is of limited utility because it is too general to apply to situations where sponsors do not control the variables affecting outcomes. The unacknowledged framework does take into account sociological variables such as the values of project sponsors, their organizational weapons, the local culture, and unanticipated consequences of social action. But the framework does not add up to a theory of successful planning because an essential ingredient is missing: an organizational analysis of project opponents. In his optimism concerning the possibility of societal change, Caplow fails to consider that the shortcomings of various projects may constitute successful projects on the part of the opponents.

Sisters in Crime: The Rise of the New Female Criminal. By Freda Adler. New York: McGraw-Hill Book Co., 1975. Pp. 287. \$9.95.

Rita J. Simon

University of Illinois

In recent months, the topic "women in crime" has probably received more attention from the mass media than it has at any previous period, certainly since the end of the Second World War. It is helpful, therefore, to have a solid piece of work on a topic so newsworthy; *Sisters in Crime* is such a work. At present it is also one of the few books devoted to this topic. The main message in Professor Adler's work is that more women are involved in more types of criminal activity than ever before; and that it looks as if, at least for the next few years, this pattern will continue.

The crime rate for adult and juvenile women is rising faster than it is for men in all crimes except murder and aggravated assault and especially in property and white-collar offenses. As Adler notes, "Liberated female criminals, like their male counterparts, are chiefly interested in improving their financial circumstances and only secondarily in committing violence." Her findings, and those reported in a volume I authored, make that same point; but both of our arguments run counter to mass-media accounts and popular impressions. That is, the Leanette Fromms and the Emily Harris notwithstand, women in large numbers are not becoming involved in crimes of violence.

Sisters in Crime begins with two chapters which review the changing patterns of women's role and the changing literature about women's propensities for crime. They also discuss the differences—biological, cultural, psychological—between men and women and assess their implications for women's participation in deviant as well as conformist activities.

The next five chapters describe various types of deviant activities, with special emphasis on prostitution and drugs. The author anticipates and already sees evidence of big changes in the types of women who work as prostitutes and the status they enjoy in the larger society. She sees educated, middle-class women entering the profession either on a part- or full-time basis and functioning primarily as entrepreneurs. She sees the role of the pimp diminishing and the presence of madams and houses disappearing. These women will be more emotionally independent and economically self-sufficient than prostitutes have been heretofore.

There are two chapters that try to assess the relationship among race, sex, and crime and among class, sex, and crime. These are the two weakest chapters, perhaps because there are so few data available to substantiate the author's expectations or theories. I think it would have strengthened her analysis to indicate the absence of data on those relationships and the need, therefore, to be very wary of any speculations.

The last two chapters discuss how women are treated and are likely to be in the future by the courts and the types of commitment they receive. One major change that Adler anticipates is that women will receive more equal treatment at the hands of the courts. But, on the whole, relatively little space is devoted to considering the legal implications of the changing pattern in women's criminal behavior. For example, Adler does not explore the likely responses of law-enforcement officials to the increasing proportion of women involved in serious property offenses, nor the possible changes that are likely to occur with respect to conviction and sentencing as a result of the attitudes of prosecutors and judges toward increases in the number of those accused.

There are some issues that Adler does not discuss that I wish she had. One is the relationship that women offenders are likely to have vis-à-vis organized crime. To what extent are women criminals already connected with organized crime and what is the likelihood that women will acquire executive and managerial positions in that network? or, asking the question another way, how formidable are the barriers to entry into the upper

echelons of organized crime? Another topic that I wished she had addressed is the situation of the woman who is sent to prison and faces loss of her children. Since the majority of women who are in prisons have young children, this is no small matter. How do women offenders react to this problem and what changes in the law, if any, should be made concerning it?

Adler writes clearly and lucidly, interspersing her observations and statistics with long quotes from interviews she has had with women offenders. These first-person accounts provide depth and balance to the more abstract and impersonal discussion of the forces that influence women's participation in crime.

Careers and Contingencies: How College Women Juggle with Gender. By Shirley S. Angrist and Elizabeth M. Almquist. New York: Dunellen Publishing Co., 1975. Pp. xvii+269. \$15.00.

Anne Foner

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When this study of college women's career choices was started in 1964, it promised to fill a serious gap in the literature on occupational choice. Until that time studies had focused primarily on men's career orientations. Moreover, because the researchers were following a group of women through their college years, the study presented an opportunity to get at processes of growth and change as these women wrestled with choices of career and family.

The specific goals of the study were to assess the effect of college on women's career orientations, pinpoint the factors that distinguished careerists from traditionalists, and obtain a detailed view of the role development of these students. The sample for the study consisted of members of the class of '68 at a small women's college (the findings reported in the book are based on a sample of 87, 85% of those who remained in college continuously for four years). Using questionnaires and interviews, the researchers collected data from these students for each of the four years of college. A great deal of information was accumulated about the course of these women's school experiences and the way they shaped their future plans.

The story that is told centers on the dilemma of educated women who want both family and career. The book describes the "contingency" approach which many of these college women adopted—they juggled and weighed alternatives while avoiding any fixed career commitment. Perhaps one of the most interesting findings (it should be noted that some of these have been previously published) concerns the great amount of change and the nature of the changes these women made in their plans for the future during their college years. Not only were there shifts in occupational choice, but there was also switching from traditional to

career aspirations or the reverse, with more women converting to careers than defecting from career orientations.

Can such a "preliberation" study have any relevance today? The authors' rationale for publishing data that might be outdated rests on their belief that most women's lives have not changed fundamentally since the time of their study and that college women today face the same problems that their sample did. This assumption seems debatable and clearly requires further research; but already there are several studies (e.g., A. Parelius, "Change and Stability in College Women's Orientations toward Education, Family, and Work," *Social Problems*, vol. 22 [February 1975]) which document considerable differences between "pre- and post-liberation" college women in attitudes toward careers. Of greater potential value is the panel design of the study which permits researchers not only to describe change but to discover the factors associated with these changes. Unfortunately, as it turns out, the authors did not capitalize on this aspect of their research design.

The book starts out with an excellent description of various patterns of change or stability in career orientations. Five types of students are identified: consistent careerists, consistent traditionalists, converts to careers, defectors from careers, and continual shifters. And capsule portraits put life into these types. Yet, although those who changed career orientations during their college years make up almost half the sample, in much of the analysis the authors collapse their interesting typology into two categories: careerists and noncareerists. They thereby lose the opportunity to explore fully the determinants of conversion to or defection from careers. For example, we learn that careerists were more likely to have working mothers than noncareerists, but we do not learn whether this is equally true for those who converted during college and those who maintained career aspirations from the beginning. Or, the career oriented were much less likely than the traditionalists to belong to sororities, but the direction of the relationship is not clear. The authors speculate that sororities may draw early career aspirers away from career ambitions, but it is puzzling that they did not pursue this further. There are other findings which begin to suggest the insights more sophisticated analysis can yield. For example, some of the women (we are not told how many) whose grades rose in their sophomore and junior years became converts to careers. Getting noticeably better grades, the authors suggest, seems to have boosted these women's self-confidence which, in turn, served to stimulate career orientations. It is regrettable that the authors did not search more thoroughly for other such clues about the nature of the college experience that propels women toward careers.

It is disappointing, then, that Angrist and Almquist did not exploit the wealth of data their longitudinal study surely must have produced. Indeed, much of their data could have been acquired by studying these women students just in their senior year. But the data are there. They provide a baseline for comparison not only to new cohorts of college women but to college men as well. The book also has useful reviews of

studies about factors influencing the supply and the demand for women workers and of theories of occupational choice. It raises provocative questions for the classroom and for further research.

Community and Occupations: An Exploration of Work/Leisure Relationships. By Graeme Salaman. Cambridge Papers in Sociology, no. 4. London: Cambridge University Press, 1974. Pp. viii+136. \$11.95 (cloth); \$5.95 (paper).

Craft and Consciousness: Occupational Technique and the Development of World Images. By Joseph Bensman and Robert Lilienfeld. New York: Wiley Interscience, 1973. Pp. x+369. \$13.95.

Walter M. Sprondel

University of Constance

Within the sociological framework observable variations in human consciousness have always been interpreted as the result of varying social experiences. The highly differentiated structures of modern societies have been more or less successfully studied with a view to their effect on images of reality which individuals develop and which, consequently, govern their behavior. The dimensions "class" and "occupation" are certainly the most prominent. Nearly all empirical investigations which use questionnaires as their main instrument treat the individual's occupation as one of the crucial independent variables. Nevertheless, detailed analyses of the relationship between a person's occupational experience, on the one hand, and his attitudes, goals, and relevance structures, on the other, are urgently needed. Both of the works reviewed here deal with this set of problems, but they differ in approach, in methods used, and, accordingly, in their results.

Everyone working in the field of occupational sociology has struggled with the unwieldy conceptual frames normally used to classify the tremendous number of jobs in modern societies. These have unfortunately proved to be rather inadequate for the sociological analysis of the world of work, at least where questions such as those indicated above are of central interest. The concept of "occupational community" seems to be somewhat more appropriate and could prove helpful in this area. Although this concept has hitherto been reserved primarily for the study of the established professions, Salaman sees it as applicable to a far wider range of occupations including, for example, those of fishermen, jazz musicians, police, and railwaymen. In spite of the highly flexible outlook of the "organization man" and the "instrumental orientation" of modern workers, there obviously still exist many occupations—whether manual or nonmanual, within large organizations or not, ranking high as well as low on prestige scales—which elicit a strong feeling of involvement in the work tasks themselves and where the members form separate and distinct groups in the strict sense. The typically resulting organizational

structure, including its social-psychological consequences for the practitioners, is called an "occupational community."

The author of the first book under review pursues three major goals. After a rather brief reference to the sociological classics focusing on such aspects as alienation (Marx), *Gemeinschaft* (Tönnies), bureaucracy (Weber), and professional ethics (Durkheim), he develops a systematic model for the study of occupational communities. Four defining components are identified: (a) the self-images of the members are centered on their occupational roles; (b) the occupation supplies the most important reference group for them; (c) they prefer to select their personal friends from among their colleagues; (d) the nonwork life is strongly influenced by the values and interests of the occupation. Occupational communities of this kind are most likely to emerge where a strong involvement in the work tasks prevails among the members, where a marginal status or class situation leads them to regard their "colleagues as the only people who matter," and where the work or organizational context is highly inclusive. Next, in a secondary analysis of known materials on different occupations, Salaman tries to demonstrate the applicability of his model. And finally, he presents data from his own investigation of railwaymen and architects and interprets them within the described framework.

Although the author is quite successful in his theoretical task, that is, in bringing some order and coherence into the ongoing debate on occupational communities, his empirical investigation of railwaymen and architects is much less convincing. Even taking into account that this is meant as a sort of pilot study, I wonder whether the use of questionnaires or the computation of simple percentages can be accepted as suitable procedures for uncovering the underlying structure of occupational value systems, especially since alternative research tools are available. Salaman is content to show that, in both cases, the components of the model are present or absent. Thus, the significance of the ascertained facts is not demonstrated by reference to the reality construction of the respondents, but only by reference to the inner logic of the model. The question how far the individual's identity is shaped by the kind of work he does is a crucial one, but we need much more elaborate instruments to deal with it empirically.

Whereas Salaman focuses on the social-organizational aspects of occupations, Bensman and Lilienfeld concentrate on the occupational conduct itself. Their general thesis is that, in professionals' handling specific materials with suitable methods and techniques, there emerge particular "habits of thought" which deeply influence the more general world views or attitudes toward everyday life of the professionals in question. Moreover, the authors believe that the culture of a whole society, the "knowledge" of the day-to-day routines, comprises disseminated yet vulgarized versions of "highly articulated systems of thought" created by professionals practicing their crafts. Thus, occupational experience is seen as an independent source of socially relevant world views, distinguishable

from class and religious or ethnic experience. Though the authors deal exclusively with such professions as arts, science, or politics, they consider their approach to be applicable to the analysis of every occupation in which a substantive level of skill is reached.

Part 1 of *Craft and Consciousness* investigates the main concepts. Expanding A. Schutz's distinction between the attitude of everyday life and the scientific attitude toward the world, the authors introduce a "critical," an "artistic," and a "planning" attitude located somewhere between the two. Empirically, these more general attitudes occur in combination with more specialized approaches which, for example, constitute the different branches of science. This is exemplified in brief sketches on religion, philosophy, history, economics, sociology, psychology, and literature which make up the second part of the book. This analysis culminates in the insight that all kinds of scientific work reduce the world in its total reality according to a specific framework and a set of acknowledged methods. Part 3 deals with the second thesis, namely, that the approaches to the world created for limited (occupational) purposes in the arts and sciences are disseminated throughout the society by journalists, intellectuals, politicians, or bureaucrats and, by this very diffusion process, are transformed into general world views available to everyone who may acquire an interest in them.

To use the jargon offered by the authors, one can read the book under at least two perspectives: if one takes the "intellectual attitude," one certainly will enjoy the great number of intelligent and inventive commentaries on the *Zeitgeist* or on specific social and cultural institutions. This is especially true of their descriptions of the "journalistic treatment" (p. 214), the relationship between artists and their critics (p. 20), and the change of perspectives within modern universities (p. 237), to give just a few examples. But the authors' claims suggest a reading primarily with the "scientific attitude." From a systematic point of view, the "new approach to the sociology of knowledge" proclaimed here turns out to be rather disappointing. The all too frequent and indeterminate use of the term "attitude" eliminates all the conceptual precision it had in the work of A. Schutz. Similarly, the key term of phenomenological methodology, "reduction," completely loses its particular meaning since it is used as a synonym for "occupational specialization." Thus, the reader is confronted with as many reductions as there are occupations. On the other hand, the authors conceive of the basic methodological principles and frameworks of the different sciences as occupations in the same sense as they speak of the occupations of physicians, lawyers, or bureaucratic employees. This leads to serious confusions in the analyses which follow. Because of this conceptual weakness, the terms "attitudes," "consciousness," "world view," "craft," etc., all used in formulating the theses of the book, can often be interchanged without altering the meaning of sentences. Thus, it seems to me rather unlikely that as many sociologists will be as fascinated by this book as the publisher's advertisement promises.

Toward a Political Sociology of Science. By Stuart S. Blume. New York: Free Press, 1974. Pp. xii+288. \$9.55.

Jerry Gaston

Southern Illinois University

This book is an attempt to redirect the major thrusts of contemporary sociology of science toward explicit consideration of social conditions that Blume believes generally do not get much attention from sociologists. As the title suggests, these conditions relate to the *political* dimensions of the social institution of science. Blume states that "the social institution of modern science is essentially political and that, moreover, the scientific role is an integral part of the political system of the modern state" (p. 1). In trying to show us the errors and shortcomings of our intellectual ways, Blume challenges a kind of hidden "enemy," the Mertonian brand of sociology of science which, he believes, is mostly and erroneously concerned with an internalist approach, specifically studying interaction of scientists with each other. Blume states: "Hagstrom, for example, on the very first page of his deservedly influential study of the scientific community, dismisses scientists' political activities as not relevant for the sociologist of science" (p. 26).

Toward a Political Sociology of Science is both a success and a failure. It is a success in what it does not set out to do intentionally and a failure in what it does set out to do. The author writes clearly and, in some instances, rather beautifully. He has a relaxed and smooth way of drawing on sources and data that does not get the reader bogged down. He has excellent sections in which he summarizes briefly the history of social movements of scientists dealing with the organization of professional and union groups. His chapter "Science for the Citizen" provides an especially interesting discussion of the dilemma between, on the one hand, the assumed necessity of involving citizens in making decisions affecting science and, on the other hand, the general ignorance of the population regarding science, the reliance on media as the main sources of information, the lack of good science writers, and the lack of uniform interest in reading about science. The best informed tend to read; the majority are not informed and are uninterested. The book is valuable because of such excellent chapters and sections.

The book fails, however, to convert me, at least, to the political sociology of science. Of course, Blume is correct that there are political dimensions to the sociology of science. He is correct also that these are important and interesting aspects of science. Where he errs is in believing that these should become the major concerns of the sociology of science (perhaps they should for the political scientists who study science). One reason Blume errs is because he views scientists quite differently from the way others do. For him, scientists seem to include all people who have studied and earned some qualifications in science; engineers may be scientists to him. It is only natural, thus defined, that great numbers of scientists—

certainly the majority—are interested in political attitudes such as unionization and preservation of wage levels and working conditions. It is hard to imagine that scientists who pursue discovery or whose main energies are expended toward creative work are so concerned with the same kind of questions.

An additional interpretation of Blume's that causes him to want sociology to view as problematic all the social conditions affecting science is his error in believing that the "public" has rejected science strongly. First, he accepts as correct any interpretation from whatever source that sees reduction in the rate of increase of support, or real reduction caused by inflation, as an indication of the lack of public support for, or public disillusionment with, science. Furthermore, after dealing at length with Marcuse, Habermas, Roszak, and Norman O. Brown, one may be seduced into believing that their view of science is shared by most people. I suspect it is much closer to reality to say that most people, even if they were aware, would not care what those scholars think about science. Thus, if it is argued that an important part of the political sociology of science is to study the response that science must make to continuously increasing public disillusionment, one may counter that the sociology of science may be without a subject. In spite of the quartet named above, real evidence of public rejection of science is not cause for concern (see the National Science Board's *Science Indicators 1972* [Washington, D.C.: Government Printing Office, 1973]).

Social movements, union organization, elite government advisers, and the like are subjects for the sociology of science. One could even study their origins and locations in the social system of the scientific community. But in spite of Blume's ideas, they are not the main or only appropriate focus of attention for the *sociology* of science.

Theories and Theory Groups in Contemporary American Sociology. By Nicholas C. Mullins. New York: Harper & Row, 1973. Pp. ix+337. \$8.95.

Arthur W. Frank III

University of Calgary

The title of Nicholas C. Mullins's book may prove misleading; he does not summarize the content of various theories but rather presents a theory of how theory groups develop. His stated objectives are to provide both a text for contemporary-theory courses and original research in the sociology of knowledge. He manages not only to achieve these objectives, but to provide insights into the state of sociology which crystallize the reorientation of the discipline during the last decade.

Mullins's theory seeks to examine "how, over time, groups of social theorists form, grow, and then cease to exist" (p. 13). This progression is divided into four successive stages; these overlap in practice, but any successful theory group must pass through each. They are: (1) a normal stage, at which the theorists are unorganized, work at scattered institu-

tions, and have no student groups; (2) a network stage, marked by "the appearance of one or more exciting intellectual products" (p. 21), frequent communication among theorists, and a few students; (3) a cluster stage, in which communication becomes increasingly ingrown, "training centers" emerge, specific problems are defined and the style of research delimited, the divergence from other theory groups is revealed, and secondary and critical materials appear; and (4) a specialty stage, at which journals are organized, faculty positions designated for representatives of the group, and a textbook(s) appears.

Movement through these stages depends on the emergence, at appropriate times, of certain "social and intellectual properties" (p. 25). Mullins does not maintain that a theory group must develop these properties in an exact sequence in order to progress, but he is successful in showing how the lack of certain properties by a certain stage can halt the development of the group. For example, structuralist theory lacks, at present, both a "social organization leader" and sufficient graduate students, and it must develop both in order to progress beyond the network stage.

This theory of theory development is applied to three "past" theories and five contemporary ones. The past-theory groups, which have either broken up or ceased effective development, are "standard American sociology" (primarily structural-functionalism but *including* conflict and exchange theories), symbolic interactionism ("the loyal opposition"), and small-group theory ("the light that failed"). The last group illustrates how the lack of certain properties, specifically an "intellectual leader" and a "program statement," can prohibit development into the specialty stage despite a legitimate field of inquiry and capable practitioners. Again, Mullins is not concerned with evaluating the quality of intellectual content, except as this content is perceived by other professionals; his concern is with academic acceptance and growth.

The contemporary-theory groups, and the stages to which they presently are developed (1973), are the social forecasters (network stage), ethnomethodology (specialty since 1971), new causal theory (characterized by the work of H. M. Blalock and O. D. Duncan; specialty stage since 1970), the structuralists (intellectually led by Harrison White; still at the network stage), and radical-critical theory (which Mullins presents as a theory group failing to conform to his model).

Mullins achieves his two objectives with considerable success. As a text, *Theories and Theory Groups* should have its greatest use in guiding preprofessional sociologists committed to learning the organization of the profession. Tables listing the important researchers in theory groups; charts illustrating patterns of coauthorship, student-teacher relationships, and colleague relations; and the bibliographic designation of the "program statement" and "intellectual successes" of different groups add to the value of the book as a reference. It is, in fact, so valuable in this capacity that the attention of some may be drawn from its theoretical contribution.

The book is limited in its usefulness as a text by the brevity of its discussions of the contents of theories; although extremely lucid, these

are perhaps too concise for even the beginning graduate student. The book would make excellent collateral reading for courses in theory content or construction; it could be used as primary-source reading in sociologies of knowledge and science courses.

As a contribution to the sociology of knowledge, the book provides more than sufficient argument to warrant further work on both Mullins's thesis that "major developments in an area require not only intellectual content but social support which must be generated in a specific way" (p. 316) and his model suggesting what this "specific way" is.

Whatever the shortcomings of Mullins's theory—and he himself enumerates them—his discussions of the breakup of small-group theory, the decline of symbolic interactionism, and the rise of new causal theory and ethnomethodology illustrate the explanatory power of his model. His discussion of the beginning of the structuralist-theory group with which he identifies himself is a rare example of a scholarly discussion which captures the excitement of group intellectual innovation.

Beyond his stated objectives, Mullins has described the changing shape of sociological work, with particular regard to the traditional divisions of theory, methods, and substantive areas. In the sociology of past-theory groups, still reflected in departmental course offerings and requirements as well as on most *vitae*, theory stands apart from methods and from substantive areas, although certain theory groups have traditionally appropriated certain methods and areas. This division is a relic of past theories: "Contemporary theory," Mullins writes, "has organized itself mainly around *how* theory is to be done; thus far, the theory itself has not been separable from methodology and content" (p. 305). The groups which "constitute the core of contemporary theory," the new causal theorists and ethnomethodologists (p. 299), are distinguished by their merging of theory and methods and their application of this mixture to a wide range of substantive areas.

In summary, Mullins not only provides a text on theory and a model for research in sociology of knowledge; he suggests with delightful coherence where sociology is in its work and presents a conceptual framework for thinking about its future.

Domestic Slavery in West Africa: With Particular Reference to the Sierra Leone Protectorate, 1896-1927. By John Grace. New York: Barnes & Noble, 1975. Pp. ix+294. \$23.50.

Bernard Magubane

University of Connecticut

The book is divided into six chapters: "Domestic Slavery in West Africa during the Nineteenth Century," "The Development of British Policy towards West African Domestic Slavery after 1833," "The Events Leading Up to the Proclamation of Sierra Leone Protectorate 1896," "The Imposition of British Authority over the Protectorate of Sierra Leone, the

Wars of 1898, and Slavery," "Domestic Slavery in the Sierra Leone Protectorate during the Twentieth Century," and "The Abolition of Slavery and Its Aftermath."

In the preface, the author tells us that he has preferred to let the facts about West African slavery speak for themselves. As a historian, John Grace seems to be more concerned with how British policy developed toward West African domestic slavery and with the motives of particular policymakers. There is no attempt to deal with the developments ushered in by imperialism and how these developments determined the various moral positions of those who were responsible for dealing with the issue of domestic slavery.

The period dating from 1870 is characterized by the imperial expansion of Europe to Africa on a more formal basis. There was in the 1880s the "scramble" and the partition of Africa. The change in the British attitude toward domestic slavery was first and foremost a response to the changing activities of European capital in Africa, which demanded the use of cheap African labor. Because of these developments, British policy demanded a crusade against domestic slavery as a means of establishing British economic and political hegemony. For example, Grace quotes Ross, who objected to compulsory labor because it was unpaid and gave the laborers no money to spend to boost trade with Britain (p. 236).

For Grace, the campaign against domestic slavery, though dominated by economic considerations, also revealed a great deal of moral expediency on the part of the abolitionists. Thus, at one time it was believed that Britain must get rid of slavery and other obstacles to progress so that the peoples of West Africa could be free to realize their great potential for development. Then, in the 1860s, there were the conservationist arguments that British and West African interests would be harmed by such interference with the institutions (including slavery) that West Africans had evolved to suit their needs.

According to the author, the latter position could be described by a cynic as a British attempt to find a moral justification for the triumph of practical considerations over moral principles (p. 22). It seems to me that whatever Britain's motives for the campaign against domestic slavery may have been, the campaign was ultimately to enhance Britain's imperial designs. True, British policy walked a thin line, both rejecting slavery and fearing to alienate those classes in Sierra Leone whose livelihood was based on the exploiting of domestic slaves.

Grace's study of the British antislavery movement in Sierra Leone is an important correction to two current positions. There is a view that holds that purely internal British humanitarian, religious, and political motives led to the campaign against all forms of slavery. This position denies any connection between the campaign and the objectives and tactics of British imperial policy. The other position holds that the antislavery campaign was merely an excuse for crass imperial objectives. The evidence presented in this book regarding the evolution of British policy toward Sierra Leone shows that one cannot separate easily the moral-humanitarian

motives from the objectives and tactics to further British imperial ambitions. Only a study of the total dynamics of the era would permit a proper assessment of the relative weights of the economic versus the humanitarian motives.

The campaign for the abolition of slavery in Sierra Leone, like the campaign to abolish the Atlantic slave trade, forms a chapter in the economic history of British imperialism in Africa. Just as the abolition of the Atlantic slave trade led to the partition and dismemberment of Africa, the campaign against domestic slavery in Sierra Leone led to the installation of Britain as an imperial protector of Sierra Leone. The antislavers, though sincere, never questioned the right of Britain to appropriate for herself African lands and resources. They only objected to the crude manifestations of this appropriation.

Ethnic Diversity in Catholic America. By Harold J. Abramson. New York: Wiley Interscience, 1973. Pp. xvi+207. \$11.95.

Joseph H. Fichter

Loyola University of the South

It is hard for a sociologist to take seriously the "persistence of ethnicity" in the growing middle class of urban and suburban American Catholics. Where ethnic diversity exists and persists, it is the exception to the ineluctable Americanization of Catholics in the third and subsequent generations. The exceptions, as implied throughout by the author of this book, exhibit three characteristics (singly or in combination): lower socioeconomic status, residential clustering, and recent immigration. Everyone knows about Catholics who are recent immigrants like Puerto Ricans, segregated groups like French-Canadians in the northeast, and those who are poorly educated like the Mexican-Americans in the southwest. But unless one holds that there is a "permanent proletariat" to which low-income, foreign-speaking Catholics are eternally doomed, it is little more than a popular fad to speak of "unmeltable" ethnic Catholics.

The persistence of Catholicism, and of other large-scale organized religions, fits into the mosaic of American religious pluralism. This is quite different from the continuing existence of ethnic pluralism, and the picture becomes even more confused when one subsumes ethnic pluralism within religious pluralism. Yet this is the highly complex sociocultural phenomenon which Abramson investigates in a competent scholarly manner.

The key statistical data concern the incidence of interethnic marriages of Catholics over succeeding generations. Abramson's intergenerational comparisons (p. 53) tend to confirm a generational shift in endogamy down from 80% to 55%. The data of this study were collected 10 years ago for Catholics from 23 to 57 years of age; the trend to exogamy is shown (p. 83) in that the younger respondents are significantly more likely than the older to be in interethnic marriages. The younger are also

more likely to have attended a Catholic high school (p. 91), but this appears to be a variable of rising SES.

Other things being equal, people tend to retain their cultural heritage when they remain clustered in a separate ethnic community, but the present study does not have any information about this (p. 74). A rough count of Catholic parish listings in six of the largest archdioceses in the United States shows that about 11% of the city parishes are still labeled by one or another nationality. If these ethnic enclaves are made up of lower-class, first-generation immigrants, they fulfill the conditions of continuing Catholic ethnic identity. It is unfortunate that the data from the 1964 NORC survey on which this study is based do not cover this key sociological question.

Nevertheless, Abramson has wrung a remarkably detailed interpretation out of available data, an excellent example of what can be done in secondary analysis of data gathered for other purposes. He has captured (chapter 5) the diversity of religious practices among nine identifiable ethnic categories of American Catholics. He has also sketched (chapter 7) the extent to which these practices change when people from diverse ethnic backgrounds get married. He caught this phenomenon at a significant historical moment, just as the post-Vatican II innovations in religious behavior and beliefs began to shift among American Catholics. There is not included in these changes any grass-roots movement in opposition to the Anglo-Irish version of Catholicism that predominates in the United States despite the popularization of an ideology of cultural pluralism to replace the earlier ideologies of Anglo-conformity and the Melting Pot. There is a difference between ethnic diversity and cultural pluralism, with the former gradually being submerged into the latter.

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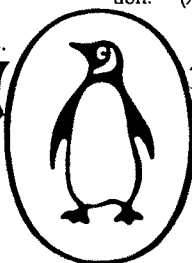
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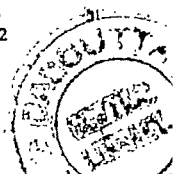
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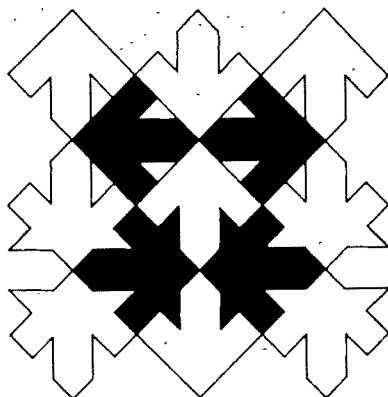
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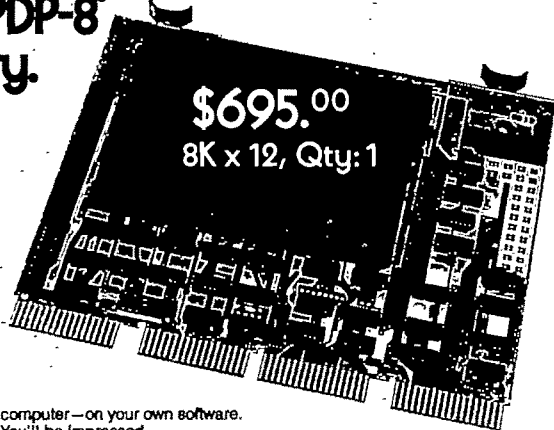
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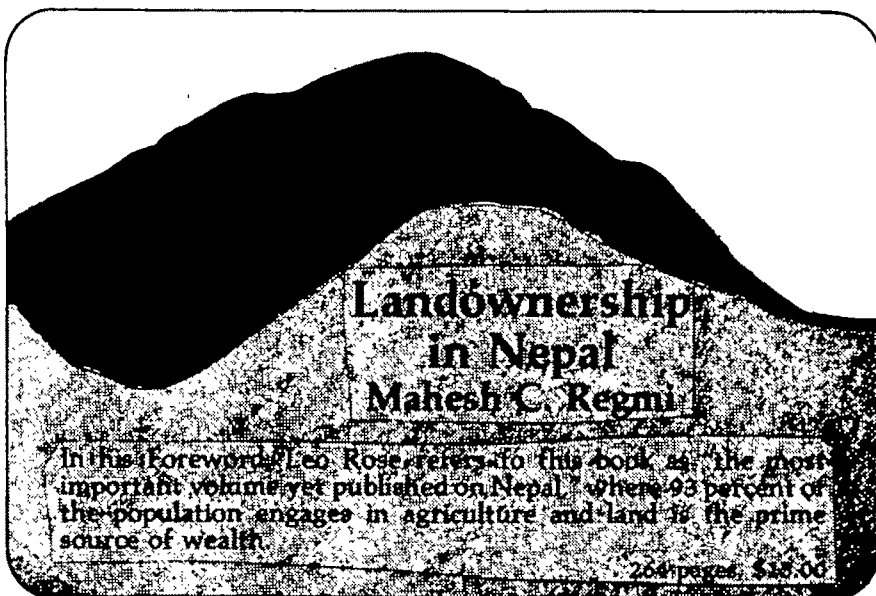
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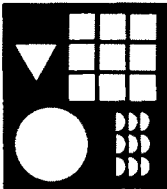
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FROM THE EDITORS

Perhaps our readers and contributors would like to know more about the way we review manuscripts than we can fit into our one page of Information for Contributors.

Any submission that the editors think is worth serious attention is sent routinely to two referees. One referee is almost always a member of the Editorial Board or a Consulting Editor. This helps to insure some consistency of evaluation across manuscripts and an evaluation that is sensitive to our editorial policy. The second referee is selected *ad hoc* with a view to specific technical and/or substantive competence. In cases of substantial disagreement between these readers, the opinion of a third referee is usually obtained. In selecting referees, both *ad hoc* and from our boards, we try to find persons who will be sympathetic to the genre of the paper in question but nevertheless likely to provide a dispassionate judgment.

The editor and associate editors meet weekly to discuss manuscripts. In these discussions we take the referees' evaluations seriously, but as advisory. We always give manuscripts our own independent reading and come to a decision only after we have argued for our own judgments and reviewed the referees' comments carefully. At this point we attend not only to the nature of the comments but also to their quality. In difficult cases the discussions may extend over two editorial meetings. On occasion we may finally overturn the referees. When we accept their judgment, it is because our own evaluation of both the manuscript and the referees' comments convinces us that it is correct.

IN THIS ISSUE

JAN SMITH is assistant professor of sociology at the University of Pennsylvania. He is completing a monograph about the effects of social structure on collective action.

HARVEY MOLOTCH is returning to the University of California, Santa Barbara, after spending the 1975-76 academic year in England as visiting professor at the University of Essex. In addition to the matters raised in the article contained in this issue (which is being expanded into a book to be published by Harcourt Brace Jovanovich), he is interested in domination as it is achieved through face-to-face talk and through mass media.

JOHN R. LOGAN is assistant professor of sociology at the State University of New York at Stony Brook. He is engaged in a continuing program of research on suburban political economy, in addition to a study of dependency, class consciousness, and authoritarian politics in contemporary Spain and Portugal.

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GARY F. JENSEN is assistant professor of sociology at the University of Arizona. His current research interests are the social psychology of sanctions, norms of retributive justice, and correlates of attitudes toward the law. His recent publications deal with women in prison, gender differences in delinquency, and the personal relevance of infraction.



Information for Contributors

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1. When author's name is in the text: Duncan (1959). When author's name is not in the text: (Gouldner 1963).
2. Pagination follows year of publication: (Lipset 1964, pp. 61-65).
3. For more than three authors, use "et al." For institutional authorship, supply minimum identification from the beginning of the complete citation: (U.S. Bureau of Census 1963, p. 117).
4. With more than one reference to an author in the same year, distinguish them by use of letters (*a*, *b*) attached to the year of publication: (1965*a*).
5. Enclose a series of references within a single pair of parentheses, separated by semicolons.

Format of References: List all items alphabetically by author (providing the full list of multiple authors) and, within author(s), by year of publication, in an appendix titled "References." Examples follow:

- Davis, K. 1963*a*. "The Theory of Change and Response in Modern Demographic History." *Population Index* 29 (October): 345-66.
- . 1963*b*. "Social Demography." Pp. 124-37 in *The Behavioral Sciences Today*, edited by Bernard Berelson. New York: Basic Books.
- Goode, W. J. 1967. "The Protection of the Inept." *American Sociological Review* 32 (February): 5-19.
- Moore, Wilbert E., and Arnold S. Feldman. 1960. *Labor Commitment and Social Change in Developing Areas*. New York: Social Science Research Council.
- Sanford, Nevitt, ed. 1962. *The American College*. New York: Wiley.
- Weber, Max. (1921) 1968. "Society's Problems." Pp. 12-16 in *Economy and Society*, edited by Guenther Ross and Claus Wittich. New York: Bedminster.

Communities, Associations, and the Supply of Collective Goods

Jan Smith

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Two principal conclusions are drawn from an examination of how the supply of collective goods to a group is affected by five communal-associational properties of the group. First, Mancur Olson has not completely explained exceptions to the size argument; other explanations do exist. Second, it is not true that communities invariably have a greater capacity to supply themselves with collective goods than do associations.

The debate over the relative social health of communities and associations (e.g., Tönnies 1957; Durkheim 1938; Park 1952; Redfield 1941) encompasses so much that it must be dissected, and its parts carefully identified, before it can be put to empirical test. This paper focuses on one kind of social health: the capacity of a group to supply itself with collective goods. Social health in this sense is sometimes incorporated into the very meaning of "community." But since "community" has other meanings as well, this usage could result in the awkward appearance of claiming that community is exhibited by something that is not a community. I shall show that it would do so. Communities do not invariably have a greater capacity to supply themselves with collective goods than do associations. The focus on the old debate does not resolve it, but instead dissolves it into a complex web of hypotheses.

Typological contrasts between communities and associations implicitly involve a set of ordinal variables. The extreme values taken by the variables denote, respectively, two ideal types, one of which is called communal, the other, associational. Each typology in the existing family (e.g., Tönnies 1957; Durkheim 1938; Redfield 1941; Levy 1966) overlaps with some of the others, but each also has its own idiosyncrasies. The variables I have selected take advantage of this variegated history while remaining firmly within the family tradition. They are group size, density of settlement, turnover in membership, access to markets, and access to inexpensive technology. An idealized community is a small and densely settled group with a stable membership and little access to markets or to inexpensive technology. An idealized association is a large and dispersed group with an unstable membership and much access to markets and to inexpensive technology. But a simple contrast between these idealizations would not take us very far. It is necessary to ask how variation in each of the five

communal-associational properties of a group affects the supply of collective goods to the group.

By a collective good, I mean a good which is made available for consumption by all members of a group whenever it is made available for consumption by any one of them (cf. Olson 1965, p. 14; Buchanan 1968, pp. 49-56; Frohlich, Oppenheimer, and Young 1971, p. 3). If schooling is made available to all children in a school district whenever it is made available to any one of them, it is a collective good for the group of children. If welfare is made available to all persons who qualify according to governmental criteria whenever it is made available to any one of them, it is a collective good for the group of people who qualify.

Collective goods are supplied if actors contribute resources—time, money, land, food, and so on—to supply them. The focus of the argument therefore shifts to the amount of some resource an actor contributes to a collective good, X , the valuation he (she, it) derives from contributing that amount, $V(X)$, and the opportunity cost he incurs by contributing that amount, $C(X)$. The opportunity cost is the maximum valuation the actor could derive from using X in some other way. The actor's marginal valuation of contributing is the rate of change of $V(X)$, $\Delta V(X)$; his marginal cost is the rate of change of $C(X)$, $\Delta C(X)$. The actor will contribute as long as $\Delta V(X) > \Delta C(X)$.¹

An actor can be said to belong to the beneficiary group of a collective good when his marginal valuation is greater than zero. The objective of this paper can now be stated more precisely. It is to discover how variation in each of the five communal-associational properties of the beneficiary group affects the marginal valuation and marginal cost of contributing to the collective good for any actor who belongs to the beneficiary group.

To keep this objective within manageable limits, three simplifying assumptions must be made. First, the actor's contribution is *unilateral*; he does not take into account the effect of his contribution on contributions by other actors. Second, the actor is *purely self-interested*; he derives valuation from his own consumption but is indifferent to all other effects of his actions. Finally, with one exception, I shall assume that the actor is *perfectly informed*; he knows precisely how his actions will affect his own consumption. The important question about these assumptions is not whether they are intuitively plausible, but whether they can be employed to arrive at empirically accurate hypotheses about the effects of the communal-associational variables on the supply of collective goods. Although

¹ The assumption that the actor maximizes valuation does not imply that he has any particular goals or any particular cognitive orientation (e.g., a *swachkrational* orientation). It does imply that his choices conform to certain rules for internal consistency (see Lee 1971, pp. 70-84). If they are appropriately consistent, saints, sybarites, and sociopaths will all maximize valuation.

I adduce observations which suggest empirical accuracy, they do not settle the matter; their purpose is to establish grounds for research.

THE MARGINAL VALUATION OF CONTRIBUTING

The resource contributed to a collective good may be physically identical with the collective good itself. A household may contribute food, for example, which is then available for consumption by several households. More often, however, contributed resources are physically distinct from collective goods. A household may contribute labor to harvest a common field, and the harvest may then be available for consumption by several households. A peasant may contribute labor on the lord's demesne in return for the lord's collective supply of land to the peasant community. Or he may contribute immunity from rioting in return for the lord's collective supply of grain to the peasant community.

It is essential to distinguish between the supply and consumption dimensions of a collective good (see Buchanan 1968, pp. 49-56). A unit of supply is a unit made available to all members of the group. A unit of consumption is a unit actually consumed; thus, there are as many consumption units, all defined in the same way, as there are consumers. Suppose the government sets the price of some product. The unit of supply is the difference in price caused by the government. The units of consumption are the consequent differences in revenues for the several sellers of the product.

The supply unit of a collective good can often be best conceived as some physical and/or human facility, such as a fire department or army or court of law. The consumption units can then be conceived as things provided by those facilities, such as protection from fire damage or from invasion or from imprisonment without appeal. In such cases, both the supply unit and the consumption units are likely to be multidimensional. A unit of fire department will have such dimensions as number of fireman-hours and number of pump trucks. Consumption units provided by the fire department will have such dimensions as protection from fire damage and protection from drowning. Thus, a reference to variation in the amount of a collective good supplied or consumed is to be understood as a reference to variation in a single dimension, on the implicit assumption that the other dimensions remain constant.

The collective good an actor consumes may not be the only good he consumes as a consequence of his contribution to that collective good. There may be externalities in consumption of the collective good (Buchanan 1968, pp. 65-72). That is, consumption of the collective good by one actor may mediate goods to other actors which they do not pay for. The valuation an actor derives from his contribution will therefore depend on both the collective good he consumes and the mediated goods he con-

sumes. A peasant consumes some amount of collectively supplied grain, for example, and, in addition, the grain consumed by other peasants decreases the probability that whatever food he has will be stolen. An actor may belong to the beneficiary group of a collective good even though he does not consume any of that good, but solely because he consumes goods mediated by others' consumption of it. Even if the peasant does not consume collectively supplied grain, he may consume protection from theft as a consequence of the grain consumption by other peasants.

Just as there can be mediated goods, so there can be mediated bads. If a highway is constructed next to a person's house he may travel on it, but he will also consume the cacophony mediated by the travel of others.

These considerations can be summarized in a causal diagram (fig. 1). The diagram shows how the total effect of a contribution to a collective good on the valuation derived from that contribution can be broken down. A communal-associational variable which affects the rate of change of any variable in the diagram with respect to any causally prior variable will affect $\Delta V(X)$.

Size

The seminal work about the effect of a group property on the supply of collective goods is Mancur Olson, Jr., *The Logic of Collective Action*. Translated into the terminology used here, Olson's principal argument (1965, pp. 22-36) can be summarized succinctly as follows: There are no mediated goods. To belong to the beneficiary group is to belong to the group which is supplied with the collective good. An increase in the size of that group will tend to cause a decrease in the rate at which the amount of the collective good consumed by any member increases with increases in the amount supplied to the group. Thus, if this is the only effect of size, an increase in size will cause a decrease in $\Delta V(X)$. When the group becomes large enough, $\Delta V(X)$ will be smaller than $\Delta C(X)$ for each member, and no member will contribute to the collective good. Indeed, when the group becomes large enough, $\Delta V(X)$ will be imperceptibly different from zero.

Olson illustrates his argument by the example of a firm which can undertake some action to raise the market price of its product. If the number of sellers is small, and each has a relatively large share of the market, a given increase in the collectively supplied price will cause a relatively large increase in the revenue consumed by each one. But if the number of sellers is large, and each has a relatively small share of the market, a given increase in the price will cause a relatively small increase in the revenue consumed by each one.

The implication of the argument is that collective goods will rarely if

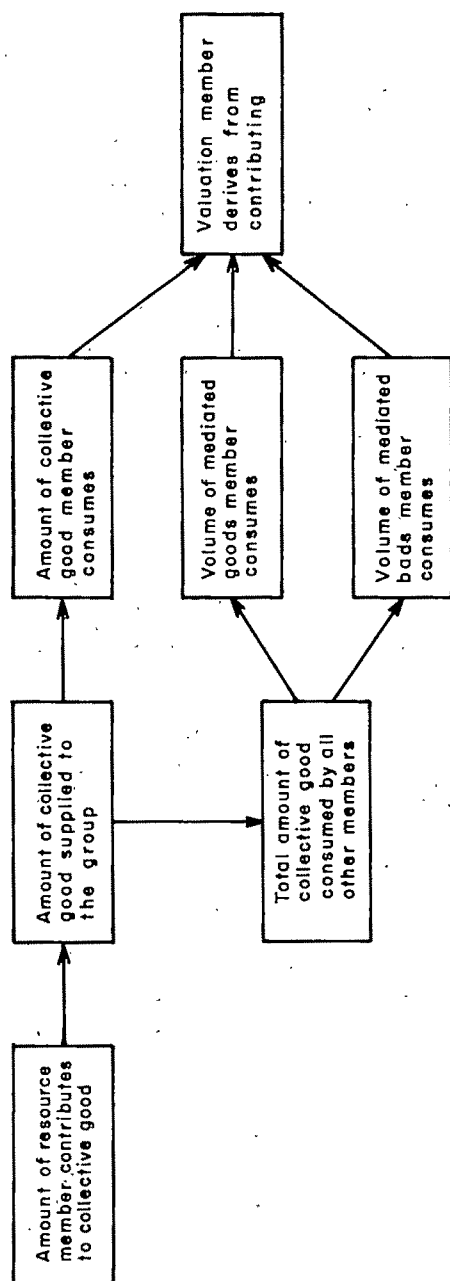


FIG. 1

ever be supplied to large groups. But collective goods are in fact supplied to very large groups every day. The size argument must be qualified. I consider three qualifications introduced by Olson and then two additional qualifications.

First, Olson (1965, pp. 159–62) takes the view that contributions to some collective goods, especially if they take the form of participation in “mass movements,” are better left to the province of social psychology. Taking his cues from the collective behavior tradition, he makes passing reference to such phenomena as “psychological disturbance” and “fanatic devotion.” Thus, while his own conceptual scheme is adequate to explain some collective goods, a fundamentally different scheme must be invented to explain the remainder. This is not a parsimonious strategy, and, appropriately, Olson invokes it only as a last resort, when other ways of accounting for exceptions to the size argument seem to have failed.²

Second, the explanation of exceptions most emphasized by Olson (1965, pp. 2, 44, 98–102, 132–59, et passim) is that organizations provide incentives which elicit members' contributions to collective goods. An organization may provide a good to a member in return for his contribution, thus increasing $\Delta V(X)$ for that member. Or it may provide a bad to a member who allocates his resource to any alternative action, thus decreasing $\Delta C(X)$ for that member. As a consequence, $\Delta V(X)$ may be larger than $\Delta C(X)$ regardless of group size.

This is not an adequate explanation of exceptions to the size argument. The organization which provides incentives is implicitly treated as an entity which behaves in accordance with unknown laws. If, in contrast, the organization is treated as an actor, it must use up some resource to provide an incentive which elicits a contribution to a collective good by some other actor. The amount of the resource allocated to providing the incentive, Y , is simply the amount indirectly contributed to the collective good by the organization. The organization will contribute to the collective good only as long as $\Delta V(Y)$ is larger than $\Delta C(Y)$. Thus, if the size argument is correct, an increase in size will cause a decrease in the rate at which the amount of the collective good consumed by the organization increases with increases in the amount supplied to the group. When the group becomes large enough, the organization will not provide incentives to elicit the direct contributions of other actors. Consider, for example, a household's contribution of food which is equally divided among several households. Suppose there is a chief's household which provides bads to other households when they fail to contribute. If the chief's household coerces M units

² The collective behavior tradition has recently been criticized by sociologists (Tilly 1973; Oberschall 1973; Gamson 1975) on the grounds that it is not parsimonious, that it is empirically inaccurate, and that its popularity has been partly due to its compatibility with an ideological defense of the status quo.

of food from the other households, then if there are N households in the group it will receive M/N units of food. Thus, when N becomes large enough, the chief's household will allocate its coercive resource to some other action, such as coercing the private contribution of food to itself alone.

To summarize to this point, one of Olson's qualifications of the size argument is not a genuine qualification, and another is a last resort. His third qualification, in contrast, is of great importance, but, unfortunately, he does not treat it explicitly as a qualification which can account for exceptions to the size argument. He (1965, pp. 36-43) distinguishes between exclusive and inclusive collective goods.³ In the terms used here, a collective good is defined as exclusive if the sum of the amounts consumed by the members of the group does not increase with increases in the size of the group when the amount supplied to the group is held constant. It is obvious, for example, that the sum of the amounts of revenue consumed by several sellers of a product does not increase with increases in the number of sellers when the price of the product is held constant. Olson points out that when a collective good is exclusive, the addition of a new member will cause a decrease in the sum of the amounts consumed by the old members, so that the old members will gain from excluding that new member.

A collective good is defined as inclusive if the sum of the amounts consumed by the members of the group increases with increases in group size when the amount supplied to the group is held constant. A collective good is defined as perfectly inclusive if this rate of increase is itself constant or increasing. Many collective goods are perfectly inclusive over some range of group size. For example, if a group of churchgoers is supplied with an hour of preaching, then N churchgoers will consume a sum of N hours of preaching up to the value of N at which all of the pews are full. If a lord supplies a group of serfs with freedom from a feudal obligation, N serfs will consume N freedoms for any value of N . More generally, when the supply unit of the collective good is best conceived as a human and/or physical facility, such as a police department or federal regulatory agency or city wall, perfect inclusiveness will ordinarily be closely approximated over some range of group size. Olson points out that when a collective good is perfectly inclusive the addition of a new member will not affect the sum of the amounts consumed by the old members, so that the old members will not gain from excluding the new member.

Hospitality is not, however, the most important consequence of perfect

³ This is an unfortunate choice of words, because no collective good is exclusive in the sense that members of the group can be excluded from consuming it. What Olson calls an inclusive collective good is sometimes called a pure collective good; what he calls an exclusive collective good is sometimes called a crowded collective good.

inclusiveness. When a collective good is perfectly inclusive over some range of group size, an increase in size within that range will usually not cause a decrease in the rate at which the amount of the good consumed by a member increases with increases in the amount supplied to the group. That is, Olson's size argument will not be valid. An increase in the number of churchgoers will not cause a decrease in the rate at which the amount of preaching each one hears increases with increases in the amount of preaching done until all of the pews are full. An increase in the number of serfs on the manor will not cause a decrease in the rate at which the number of freedoms each consumes increases with increases in the number of freedoms supplied by the lord. Over some range, an increase in the number of residents in a town will cause an imperceptible decrease in the rate at which the amount of protection from theft each consumes increases with increases in the number of police man-hours. In such cases, an increase in group size will not cause a decrease in $\Delta V(X)$ for any member.

A fourth qualification of the size argument, not considered by Olson, is that the conversion of resources into consumption units of a collective good may exhibit increasing returns to scale. That is, an increase in the total amount contributed to a collective good by all members of the beneficiary group may cause an increase in the rate at which the amount of collective good consumed by any one increases with increases in the amount he contributes (cf. Frohlich, Hunt, Oppenheimer, and Wagner 1975). In this circumstance, an increase in the total amount others are committed to contribute will cause an increase in $\Delta V(X)$ for any member.⁴

Scale, the total amount contributed to the collective good, is important here because of its dependence on group size. It is safe to assume that only members of the beneficiary group will contribute to a collective good. There is an upper limit on the amount any one of those members will contribute. Thus, an increase in group size may be a necessary condition for an increase in the total amount contributed to a collective good. If there are increasing returns to scale, then an increase in group size may cause an increase in $\Delta V(X)$ for any member of the group.

With this in mind, consider for a moment two of the other communal-associational variables: access to inexpensive technology and access to markets. By a technology, I mean either a nonhuman source of energy or a tool which multiplies the effects of energy. The employment of a particular technology in the production of a collective good may cause an increase in the rate at which the amount of the good consumed by any member increases with increases in the amount he contributes to production of that good. If farm machinery is inexpensive, its use may cause an increase in

⁴ To consider commitments in any depth would require consideration of agreements and trust. The effects of the communal-associational variables on agreements and trust is a substantial topic in itself and far beyond the scope of this paper.

the rate at which the amount of harvest from a common field consumed by any household increases with the amount of labor it contributes, including the labor required to build or buy the machinery. If preaching is done in a very large church, and if a sound amplifier is inexpensive, the use of an amplifier may cause an increase in the average proportion of each hour of preaching which any member is able to hear. Use of the amplifier may then permit any member to hear more preaching for his money, including that part of his money which is spent on the amplifier.

Technologies can be employed only by groups which have access to them, that is, by groups which know how to build them or where to buy them. But access in this sense may not be sufficient for the employment of a technology. In addition, the total amount contributed by all members may have to reach a certain level before it is possible to employ a technology. The amount of farm machinery used or the size of the amplifier will be limited by the total amount contributed. Thus, there is an interaction effect of scale and access to inexpensive technology. The conjunction of access to a particular technology with a large total contribution may be the condition that causes an increase in the rate at which the amount of the collective good consumed by any member increases with increases in the amount he contributes.

The effect of specialization is sometimes analogous to the effect of access to inexpensive technology. An increase in the specialization of labor and tools used to produce a collective good may cause an increase in the rate at which the amount consumed by any member increases with increases in the amount he contributes. The use of specialized labor to work a common field, for example, may cause an increase in the rate at which the amount of harvest consumed by any household increases with increases in the amount of labor it contributes. The specialization of an army into officers and soldiers may cause an increase in the rate at which the expected amount of protection from invasion received by any household increases with increases in the amount it contributes to the army's support. The degree of specialization which can be employed is limited, however, by the total amount all members contribute to the collective good. Thus, the conjunction of an opportunity for specialization with a large total contribution may be the condition that causes an increase in the rate at which the amount of the collective good consumed by any member increases with increases in the amount he contributes.

But this last assertion must be qualified. Even if the total contribution is large enough to permit some specialization, that specialization will have the presumed effect only if it is relatively inexpensive. An increase in the group's access to markets, that is, in the number of different goods and services which can be purchased in private exchange, will be likely to cause a decrease in the expense of specialization. It is often cheaper to buy

specialized tools than to make them, or to hire specialists than to train them. And it is almost always cheaper to pay specialists in money which they can use for market purchases than to oversee their subsistence directly. Thus, the conjunction of high access to markets with a large total contribution may be the condition that causes an increase in the rate at which the amount of the collective good consumed by any member of the group increases with increases in the amount he contributes.

Now reconsider the effect of group size. Since the total contribution by all members is dependent on group size, it may be the conjunction of either access to technology or access to markets with a large group that causes an increase in the rate at which the amount of the collective good consumed by any member increases with increases in the amount he contributes. If history will permit the generalization that the expansion of technology and markets is accompanied by an increase in the average number of beneficiaries of collective goods, this must certainly be one of the reasons.

A fifth and final qualification of Olson's size argument must be introduced because the argument does not countenance mediated goods and bads. The effect of group size on the volume of mediated goods and bads consumed will vary from one collective good to the next and, for the same collective good, from one member of the beneficiary group to the next. Indeed, for some collective goods, an increase in group size may cause such a large increase in the amount of mediated bad an actor consumes that it moves that particular actor out of the beneficiary group. For example, an increase in the number of travelers on a road may cause so much extra noise that a nearby resident comes to derive negative valuation from each dollar he contributes to maintain the road.

But sometimes an increase in group size will cause an increase in the volume of mediated goods each member consumes and will thus cause an increase in $\Delta V(X)$ for each member. Consider, for example, the use of social agitation to gain the incorporation of an ethnic language into the public school curriculum. An increase in the number of persons who consume schooling in the language will cause an increase in the number of opportunities any speaker of the language has to use it and will thus cause an increase in the valuation each speaker derives from extra social agitation. Analogous arguments could be developed for a variety of "cultural" collective goods.

The valuation derived from mediated goods may be so large that a member of the beneficiary group will contribute to provision of the collective good even though he does not consume any of it himself. A great variety of collective goods have been explained in precisely this way (e.g., Dorn 1940, pp. 1-17; Williams 1944; O'Connor 1973; Anderson 1974, pp. 93-94). When such explanations are correct, Olson's size argument

is irrelevant. And such explanations assume implicitly that over some range an increase in group size will cause an increase in the valuation derived from mediated goods.

In the 19th century, for example, some English manufacturers financed schools for the children of their employees (Stearns 1967, p. 178). Naturally, their own children did not attend these schools. But the schooling consumed by the future employees probably caused an increase in their tractability to factory discipline. It thus caused a decrease in the manufacturers' expenditure on labor and, since they sold their products in an imperfectly competitive market, caused an increase in their profits.

Density of Settlement

By density of settlement, I mean the reciprocal of the average distance between pairs of members of the beneficiary group. Density will change over time, and for some groups it may change over relatively short intervals. My argument deals implicitly with an interval during which density is stable.

Between contributions to and consumption of a collective good, physical things are moved from one place to another. Sometimes this movement is simple. A household moves food to a storehouse and later moves its members to a ceremony next to the storehouse, during which the stored food is consumed. Sometimes it is extremely complex. A corporation moves funds from bank to bank and finally into the pockets of legislators who vote a price support for its industry. This, in turn, changes the pattern of movement between the government's allocation of money to pay a tax collector and its allocation of tax revenues to an agency which enforces the price support.

Whatever the pattern of movement, an increase in the distance over which a physical thing is moved will cause an increase in the volume of resources required to move it. Since these resources are part of the total contribution to the collective good by all members, an increase in the volume required will cause a decrease in the average $\Delta V(X)$ for the members. Thus, other things being equal, the members will minimize the distance over which things are moved. An increase in density will tend to cause a decrease in the minimum distance and will therefore tend to cause a decrease in the average $\Delta V(X)$ for the members.

But this argument must be qualified. First, an increase in access to markets will cause an increase in the probability that a nonmonetary thing can be converted into money. The volume of resources required to move money will increase with increases in distance at a lower rate than the volume of resources required to move virtually anything that the money could buy. Thus, access to markets will cause a decrease in the average

$\Delta V(X)$. Second, the employment of inexpensive technologies of transportation may cause a decrease in the rate at which the volume of resources required to move a thing increases with increases in distance. Thus, access to such technologies will decrease the effect of density on the average $\Delta V(X)$.

Another way in which density affects the average $\Delta V(X)$ is by affecting the valuations derived from mediated goods and bads. A decrease in the distance between the member who consumes the collective good and the member who consequently consumes the mediated good or bad will often cause an increase in the amount of mediated good or bad consumed. If this occurs and if consumption of the collective good mediates both goods and bads, no conclusion can be made about the effect of density. But if consumption of the collective good mediates only goods, or a great preponderance of goods, an increase in density will cause an increase in the average $\Delta V(X)$. For example, an increase in density will cause an increase in the average protection from smells and disease each household consumes as a consequence of the garbage collection others consume. It will cause an increase in the average protection from theft each household consumes as a consequence of welfare payments to impoverished households. But the latter example shows that this effect of density is sometimes modified by access to inexpensive transportation technology, in this case, cheap automobile travel.

Of course, it is possible that consumption of a collective good will mediate only bads, or a great preponderance of bads, so that an increase in density will cause a decrease in the average $\Delta V(X)$. But it is difficult to find many examples of collective goods which conform to this condition. Thus, I offer the tentative generalization that consumption of collective goods is more likely to mediate goods than to mediate bads. If this is true, an increase in density will typically cause an increase in the average $\Delta V(X)$ for the members of the beneficiary group.

These two arguments help to account for several broad historical patterns. First, urban residents consume a larger volume of collective goods than do widely dispersed rural residents (Meier 1962, pp. 5, 35, 37). But the urban-rural difference should be reduced by access to money and to inexpensive transportation technologies. Second, the commercial and technological growth leading up to and composing the industrial revolution was accompanied by an increase in the volume of collective goods supplied to large territories through central governments (Barker 1966). Finally, this same growth saw a shift from local to nationwide political organizations (Tilly 1975b, p. 510).

A final argument about density applies specifically to the consumption of collectively supplied protection from invasion. An increase in density causes a decrease in the length of the perimeter which must be populated

by sentries, fortifications, and combat units, and may therefore cause an increase in the rate at which the amount of protection a member consumes increases with increases in the amount he contributes to defense of the perimeter. Together with the argument above concerning expenditures on transportation, this accounts for the importance of early cities as fortresses (Weber 1968, 3:1220–21) and for migration into dense settlements during periods of widespread warfare (e.g., Bloch 1961, p. 42; Elvin 1973, p. 42). But access to inexpensive technology modifies the effect of density. Before industrialization, an increase in the territorial extension of a group had a larger negative effect on the protection from invasion returned to a member by his contribution to support military organization. Thus, preindustrial empires which covered large territories were more vulnerable to invasion than are modern states of comparable extent (cf. Elvin 1973, pp. 17–22). Herz (1957) suggests that the territorial basis of military organization may even be about to disappear.

Turnover in Membership

In order to consider the effects of turnover in membership on $\Delta V(X)$, it is essential to consider how time enters into the decision to contribute to a collective good. The member does not estimate $\Delta V(X)$ for each instant in time. Instead, he estimates it for some interval of time by assessing how much he would contribute during that interval and the volume of collective good and mediated goods and bads he would consume during that same interval.

Turnover has two aspects—entry and exit—both of which affect group size. But it is possible to avoid confounding turnover with size by defining turnover as the number of members of the beneficiary group at the beginning of an interval who are replaced by other members during that interval. That is, turnover is defined for an idealized situation in which size is held constant. Turnover has two important effects on the interval $\Delta V(X)$ for the members.

First, the new member often must learn how to contribute to and consume the collective good. Sometimes the learning can be easily done by direct observation, and the newcomer can learn without help. A man who marries into a hunting band, for example, may learn by observation how to cooperate in a hunt and how to share in the division of the carcass. But sometimes learning by direct observation is unlikely. A newcomer to an urban neighborhood, for example, may never chance upon the opportunity to observe how to contribute to a neighborhood organization and how to consume the collective good supplied by that organization. In such cases, old members may allocate resources to instruct newcomers. Specialized facilities, such as neighborhood newspapers or the services of ward

leaders and social workers, may be employed to instruct newcomers in how to contribute and how to consume. Contributions to these facilities count as part of the total contribution to the collective goods they teach about. An increase in turnover will cause an increase in the volume of resources required for the facilities. And since group size is held constant, an increase in turnover will cause an increase in the volume of resources *per member* required for the facilities. Thus, an increase in turnover will cause a decrease in the average interval $\Delta V(X)$ for the members. The effect may, of course, be modified by access to inexpensive technologies of transportation and communication. A neighborhood organization, for example, may establish a newsletter or purchase space in a newspaper in order to instruct newcomers.

To understand the second effect of turnover, it is necessary to divide an interval of time into equal subintervals and distinguish the $\Delta V(X)$ for any member during the entire interval from the $\Delta V(X)$ s for that member during the subintervals. It is often possible to divide an interval into equal subintervals such that the $\Delta V(X)$ of each successive subinterval is larger than that of the preceding one for any member of the group. This situation can exist for at least three reasons. First, contributions to collective goods ordinarily precede consumption. Second, a large volume of resources is sometimes required to establish a facility and a much smaller volume to maintain it. Third, a large volume of resources is sometimes required to persuade an outside actor to supply a collective good in return for immunity from additional bads and a much smaller volume to reinforce the persuasion.

Recall the definition of the beneficiary group: it consists of all actors for whom $\Delta V(X)$ is greater than zero. Thus, an actor who belongs to the group at the beginning of an interval but is replaced during it will realize the relatively small subinterval $\Delta V(X)$ s before leaving but not the relatively large subinterval $\Delta V(X)$ s after leaving. Leaving means that his subsequent subinterval $\Delta V(X)$ s will be zero or less. It follows that an increase in turnover, in the number of initial members who leave during the interval, will cause a decrease in the average interval $\Delta V(X)$ for the initial members.

This effect may be modified by access to markets. In particular, if the group can obtain loans of resources from outsiders, its members may not have to concentrate their contributions at the beginning of an interval. The differences among subinterval $\Delta V(X)$ s can therefore be decreased.

Consider some of the *ceteris paribus* predictions which follow from these arguments about turnover effects. First, the smaller the turnover of employees in a particular firm, the more likely the employees will be to undertake actions which supply them with collective goods. The frequency of strikes is highest in industries where workers reside in their own, isolated

communities (Kerr and Siegel 1954), and migratory workers seem especially slow to unionize. Second, the larger the turnover of incumbents in a particular occupation, the smaller the probability that the incumbents will undertake actions to supply themselves with collective goods. Relatively little union organization should occur in occupations which have few skill requirements, or in occupations which are ordinarily rungs on a promotional ladder. Third, the larger the turnover of residents in a territorial neighborhood, the smaller the probability that the residents will undertake actions to supply themselves with collective goods. Neighborhoods with a large number of transients should be less likely than more stable neighborhoods to form organizations which autonomously produce, or bargain with outsiders for the supply of, public facilities.

THE MARGINAL COST OF CONTRIBUTING

The task here is to discover how variation in the communal-associational properties of the beneficiary group affects the marginal cost of contributing to the collective good. By making restrictive assumptions, one could discover a number of such effects.⁵ But I will consider just one, because that one seems to have had great historical importance.

Suppose an actor holds a pure consumption good, that is, a good which cannot be used as a factor in the production of any other good. The good can be (a) immediately consumed, (b) stored, (c) privately exchanged for some other good, or (d) collectively supplied to a group. The marginal cost of collectively supplying this good is the maximum marginal valuation of the first three alternatives.

Immediate consumption causes satiation: an increase in the amount of the good consumed during a short period of time will cause a decrease in the marginal valuation of additional consumption. Thus, once a sufficient amount of the good has been consumed, the marginal valuation of consumption will fall below the marginal valuation of one of the remaining three alternatives.

In consequence, it is necessary only to consider how the communal-associational variables affect the marginal valuation of storing or privately exchanging the good. First, access to inexpensive technology of storage will cause a decrease in the volume of resources required to store a given amount of the good for a given period of time and will thus cause

⁵ Suppose, for example, that members of the beneficiary group engage only in private exchange with other members. An increase in density will then tend to decrease the distance between partners to a private exchange and thereby decrease the amount of resources they must allocate to transportation. Thus, an increase in density will tend to increase the maximum marginal valuation a member could derive from privately exchanging the resource he would contribute to the collective good. A number of similar arguments about opportunity cost can be developed.

an increase in the marginal valuation of storage. Second, an increase in access to markets will permit private exchange of the good for a larger variety of other goods. An increase in the variety of goods purchased in private exchange will cause a decrease in satiation from consuming each one of them and will therefore cause an increase in the marginal valuation of private exchange. Third, access to markets will permit private exchange of the good for money. Since money is cheap to store, access to markets will cause an increase in the marginal valuation of private exchange (cf. Sahlins 1968, p. 87). To summarize, then, an increase in access to inexpensive technology of storage or to markets will tend to cause an increase in the marginal cost of collectively supplying a pure consumption good. And the argument can be extended. It applies to any *other* good which can be used only in one of the four ways mentioned above or as a factor in production of the pure consumption good.

The argument explains why exposure to technology and to markets tends to diminish and destroy collective supply of agricultural produce, collective rights in land, and the collective supply of their labor by people who are skilled only in agriculture. Collective and quasi-collective agricultural systems seem always to dissolve when exposed to markets (Wolf 1969). The dissolution has been thoroughly studied for the case of Western Europe (for example, Weber 1961, p. 82; Hill 1969; Davis 1973; Tilly 1975a) where, naturally enough, it has been understood as a consequence of urban demand for agricultural products.

But it does not follow that technology and markets inevitably and completely "privatize" a society. One of the goods a person consumes in return for consenting to collective rights in land, or in return for collectively supplying his private harvest, is immunity from the force and fraud of other people who would receive the land or the harvest. Markets for agricultural products may make the risk of this force and fraud tolerable for the agrarian capitalist. But the risk is not negligible. Peasant revolts and food riots are characteristic of the transition to market agriculture (Wolf 1969; Tilly 1975a).

Participation in such a rebellion is a contribution to a collective good. Insofar as such participation attempts to restore old collective rights or old arrangements for the collective distribution of agricultural products, it is symptomatic of the old order's last days. But insofar as it is an attempt to control market prices, it is a contribution to a distinctively "modern" kind of collective good; it is a close relative of striking for higher wages, lobbying for professional licensing, boycotting a retailer, and joining a cartel. Access to markets increases the marginal cost of contributions to some kinds of collective goods, but it also creates new opportunities for the supply of other kinds of collective goods.

SUMMARY

Olson has not explained adequately why collective goods are supplied to large groups. I have shown that a large group may be supplied with a collective good under any one of three conditions: (a) the collective good is inclusive, (b) an increase in the number of actors who consume the collective good causes an increase in the volume of mediated goods consumed by some actors, or (c) there are increasing returns to scale in the production of the collective good. Increasing returns to scale are more likely to exist if the group has access to markets and/or to inexpensive technology.

A group's capacity to supply itself with collective goods tends to be decreased by a decrease in density of settlement or by an increase in turnover of members. But both of these effects may be substantially diminished by access to markets or to inexpensive technology.

Access to inexpensive technology or to markets may increase the marginal cost of contributing to a collective good and thus decrease a group's capacity to supply itself with that collective good. But while markets may eliminate some kinds of collective goods, they also make new kinds possible.

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The City as a Growth Machine: Toward a Political Economy of Place¹

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A city and, more generally, any locality, is conceived as the areal expression of the interests of some land-based elite. Such an elite is seen to profit through the increasing intensification of the land use of the area in which its members hold a common interest. An elite competes with other land-based elites in an effort to have growth-inducing resources invested within its own area as opposed to that of another. Governmental authority, at the local and nonlocal levels, is utilized to assist in achieving this growth at the expense of competing localities. Conditions of community life are largely a consequence of the social, economic, and political forces embodied in this growth machine. The relevance of growth to the interests of various social groups is examined in this context, particularly with reference to the issue of unemployment. Recent social trends in opposition to growth are described and their potential consequences evaluated.

Conventional definitions of "city," "urban place," or "metropolis" have led to conventional analyses of urban systems and urban-based social problems. Usually traceable to Wirth's classic and highly plausible formulation of "numbers, density and heterogeneity" (1938), there has been a continuing tendency, even in more recent formulations (e.g., Davis 1965), to conceive of place quite apart from a crucial dimension of social structure: power and social class hierarchy. Consequently, sociological research based on the traditional definitions of what an urban place is has had very little relevance to the actual, day-to-day activities of those at the top of local power structure whose priorities set the limits within which decisions affecting land use, the public budget, and urban social life come to be made. It has not been very apparent from the scholarship of urban social science that land, the basic stuff of place, is a market commodity providing wealth and power, and that some very important people consequently take a keen interest in it. Thus, although there are extensive literatures on community power as well as on how to define and conceptualize a city or urban place, there are few notions available to link the two issues coherently, focusing on the urban settlement as a political economy.

This paper aims toward filling this need. I speculate that the political

¹ I have had the benefit of critical comments and assistance from Richard Appelbaum, Richard Baisden, Norman Bowers, Norton Long, Howard Newby, Anthony Shih, Tony Pepitone, Gerald Suttles, Gaye Tuchman, and Al Wyner.

and economic essence of virtually any given locality, in the present American context, is *growth*. I further argue that the desire for growth provides the key operative motivation toward consensus for members of politically mobilized local elites, however split they might be on other issues, and that a common interest in growth is the overriding commonality among important people in a given locale—at least insofar as they have any important local goals at all. Further, this growth imperative is the most important constraint upon available options for local initiative in social and economic reform. It is thus that I argue that the very essence of a locality is its operation as a growth machine.

The clearest indication of success at growth is a constantly rising urban-area population—a symptom of a pattern ordinarily comprising an initial expansion of basic industries followed by an expanded labor force, a rising scale of retail and wholesale commerce, more far-flung and increasingly intensive land development, higher population density, and increased levels of financial activity. Although throughout this paper I index growth by the variable population growth, it is this entire syndrome of associated events that is meant by the general term “growth.”² I argue that the means of achieving this growth, of setting off this chain of phenomena, constitute the central issue for those serious people who care about their locality and who have the resources to make their caring felt as a political force. The city is, for those who count, a growth machine.

THE HUMAN ECOLOGY: MAPS AS INTEREST MOSAICS

I have argued elsewhere (Molotch 1967, 1973) that any given parcel of land represents an interest and that any given locality is thus an aggregate of land-based interests. That is, each landowner (or person who otherwise has some interest in the prospective use of a given piece of land) has in mind a certain future for that parcel which is linked somehow with his or her own well-being. If there is a simple ownership, the relationship is

²This association of related phenomena is the common conceptualization which students of the economic development of cities ordinarily utilize in their analyses (see, e.g., Alonso 1964, pp. 79–81; Leven 1964, pp. 140–44; Brown 1974, pp. 48–51; and Durr 1971, pp. 174–80). As Sunquist remarks in the context of his study of population policies in Western Europe, “The key to population distribution is, of course, job availability. A few persons—retired, notably, and some independent professionals such as artists, writers and inventors—may be free to live in any locality they choose but, for the rest, people are compelled to distribute themselves in whatever pattern is dictated by the distribution of employment opportunities. Some investors may locate their investment in areas of surplus labour voluntarily, and so check the migration flow, and others may be induced by government assistance to do so. But if neither of these happens—if the jobs do not go where the workers are—the workers must go to the jobs, if they are not to accept welfare as a way of life. When population distribution is an end, then, job distribution is inevitably the means” (1975, p. 13).

straightforward: to the degree to which the land's profit potential is enhanced, one's own wealth is increased. In other cases, the relationship may be more subtle: one has interest in an adjacent parcel, and if a noxious use should appear, one's own parcel may be harmed. More subtle still is the emergence of concern for an aggregate of parcels: one sees that one's future is bound to the future of a larger area, that the future enjoyment of financial benefit flowing from a given parcel will derive from the general future of the proximate aggregate of parcels. When this occurs, there is that "we feeling" (McKenzie 1922) which bespeaks of community. We need to see each geographical map—whether of a small group of land parcels, a whole city, a region, or a nation—not merely as a demarcation of legal, political, or topographical features, but as a mosaic of competing land interests capable of strategic coalition and action.

Each unit of a community strives, at the expense of the others, to enhance the land-use potential of the parcels with which it is associated. Thus, for example, shopkeepers at both ends of a block may compete with one another to determine in front of which building the bus stop will be placed. Or, hotel owners on the north side of a city may compete with those on the south to get a convention center built nearby (see Banfield 1961). Likewise, area units fight over highway routes, airport locations, campus developments, defense contracts, traffic lights, one-way street designations, and park developments. The intensity of group consciousness and activity waxes and wanes as opportunities for and challenges to the collective good rise and fall; but when these coalitions are of sufficiently enduring quality, they constitute identifiable, ongoing communities. Each member of a community is simultaneously the member of a number of others; hence, communities exist in a nested fashion (e.g., neighborhood within city within region), with salience of community level varying both over time and circumstance. Because of this nested nature of communities, subunits which are competitive with one another at one level (e.g., in an interblock dispute over where the bus stop should go) will be in coalition at a higher level (e.g., in an intercity rivalry over where the new port should go). Obviously, the anticipation of potential coalition acts to constrain the intensity of conflict at more local loci of growth competition.

Hence, to the degree to which otherwise competing land-interest groups collude to achieve a common land-enhancement scheme, there is community—whether at the level of a residential block club, a neighborhood association, a city or metropolitan chamber of commerce, a state development agency, or a regional association. Such aggregates, whether constituted formally or informally, whether governmental political institutions or voluntary associations, typically operate in the following way: an attempt is made to use government to gain those resources which will enhance the growth potential of the area unit in question. Often, the

governmental level where action is needed is at least one level higher than the community from which the activism springs. Thus, individual land-owners aggregate to extract neighborhood gains from the city government; a cluster of cities may coalesce to have an effective impact on the state government, etc. Each locality, in striving to make these gains, is in competition with other localities because the degree of growth, at least at any given moment, is finite. The scarcity of developmental resources means that government becomes the arena in which land-use interest groups compete for public money and attempt to mold those decisions which will determine the land-use outcomes. Localities thus compete with one another to gain the *preconditions* of growth. Historically, U.S. cities were created and sustained largely through this process;³ it continues to be the significant dynamic of contemporary local political economy and is critical to the allocation of public resources and the ordering of local issue agendas.

Government decisions are not the only kinds of social activities which affect local growth chances; decisions made by private corporations also have major impact. When a national corporation decides to locate a branch plant in a given locale, it sets the conditions for the surrounding land-use pattern. But even here, government decisions are involved: plant-location decisions are made with reference to such issues as labor costs, tax rates, and the costs of obtaining raw materials and transporting goods to markets. It is government decisions (at whatever level) that help determine the cost of access to markets and raw materials. This is especially so in the present era of raw material subsidies (e.g., the mineral depletion allowance) and reliance on government approved or subsidized air transport, highways, railways, pipelines, and port developments. Government decisions influence the cost of overhead expenses (e.g., pollution abatement requirements, employee safety standards), and government decisions affect the costs of labor through indirect manipulation of unemployment rates, through the use of police to constrain or enhance union organizing, and through the legislation and administration of welfare laws (see Piven and Cloward 1972).

Localities are generally mindful of these governmental powers and, in addition to creating the sorts of physical conditions which can best serve industrial growth, also attempt to maintain the kind of "business climate" that attracts industry: for example, favorable taxation, vocational training, law enforcement, and "good" labor relations. To promote growth, taxes should be "reasonable," the police force should be oriented toward protection of property, and overt social conflict should be minimized (see

³ For accounts of how "boosterism" worked in this manner, see Wade (1969) and Harris (1976).

Rubin 1972, p. 123; Agger, Goldrich, and Swanson 1964, p. 649).⁴ Increased utility and government costs caused by new development should be borne (and they usually are—see, e.g., Ann Arbor City Planning Department [1972]) by the public at large, rather than by those responsible for the “excess” demand on the urban infrastructure. Virtually any issue of a major business magazine is replete with ads from localities of all types (including whole countries) trumpeting their virtues in just these terms to prospective industrial settlers.⁵ In addition, a key role of elected and appointed officials becomes that of “ambassador” to industry, to communicate, usually with appropriate ceremony, these advantages to potential investors (see Wyner 1967).⁶

I aim to make the extreme statement that this organized effort to affect the outcome of growth distribution is the essence of local government as a dynamic political force. It is not the only function of government, but it is the key one and, ironically, the one most ignored. Growth is not, in the present analysis, merely one among a number of equally important concerns of political process (cf. Adrian and Williams 1963). Among contemporary social scientists, perhaps only Murray Edelman (1964) has provided appropriate conceptual preparation for viewing government in such terms. Edelman contrasts two kinds of politics. First there is the “symbolic” politics which comprises the “big issues” of public morality and the symbolic reforms featured in the headlines and editorials of the daily press. The other politics is the process through which goods and services actually come to be distributed in the society. Largely unseen, and relegated to negotiations within committees (when it occurs at all within a formal government body), this is the politics which determines who, *in material terms*, gets what, where, and how (Lasswell 1936). This is the kind of politics we must talk about at the local level: it is the politics

⁴ Agger et al. remark, on the basis of their comparative study of four U.S. cities: “[Members of the local elites] value highly harmony and unity—‘pulling together.’ They regard local community affairs as essentially nonpolitical, and tend to associate controversy with ‘politics.’ An additional factor reinforcing the value of harmony in many communities . . . is the nationwide competition among communities for new industries. Conflict is thought to create a highly unfavourable image to outsiders, an image that might well repel any prospective industry” (1964, p. 649).

⁵ See, e.g., the May 19, 1974, issue of *Forbes*, which had the following ad placed by the State of Pennsylvania: “Q: [banner headline] What state could possibly cut taxes at a time like this? A: Pennsylvania [same large type]. Pennsylvania intends to keep showing businessmen that it means business. Pennsylvania. Where business has a lot growing for it. . . .” The state of Maryland ran this ad in the same issue: “Maryland Finances the Training. . . . In short, we can finance practically everything you need to establish a manufacturing plant. . . .”

⁶ The city of Los Angeles maintains an office, headed by a former key business executive, with this “liaison” role as its specific task (see “L.A.’s Business Envoy Speaks Softly and Sits at a Big Desk,” *Los Angeles Times* [August 26, 1974]).

of distribution, and land is the crucial (but not the only) variable in this system.

The people who participate with their energies, and particularly their fortunes, in local affairs are the sort of persons who—at least in vast disproportion to their representation in the population—have the most to gain or lose in land-use decisions. Prominent in terms of numbers have long been the local businessmen (see Walton 1970),⁷ particularly property owners and investors in locally oriented financial institutions (see, e.g., Spaulding 1951; Mumford 1961, p. 536), who *need* local government in their daily money-making routines. Also prominent are lawyers, syndicators, and realtors (see Bouma 1962) who need to put themselves in situations where they can be most useful to those with the land and property resources.⁸ Finally, there are those who, although not directly involved in land use, have their futures tied to growth of the metropolis as a whole. At least, when the local market becomes saturated one of the few possible avenues for business expansion is sometimes the expansion of the surrounding community itself (see Adrian and Williams 1963, p. 24).⁹

This is the general outline of the coalition that actively generates the community “we feeling” (or perhaps more aptly, the “our feeling”)¹⁰ that comes to be an influence in the politics of a given locality. It becomes manifest through a wide variety of techniques. Government funds support “boosterism” of various sorts: the Chamber of Commerce, locality-promotion ads in business journals and travel publications, city-sponsored parade

⁷ The literature on community power is vast and controversial but has been summarized by Walton: he indicates, on the basis of 39 studies of 61 communities, that “the proportion of businessmen found in the leadership group is high irrespective of the type of power structure found” (1970, p. 446). It is my argument, of course, that this high level of participation does indeed indicate the exercise of power on behalf of at least a portion of the elite. My analysis does not assume that this portion of the elite is necessarily always united with others of high status on the concrete issues of local land use and the uses of local government.

⁸ Descriptions of some tactics typically employed in land-use politics are contained in McConnell (1966), Tolchin and Tolchin (1971), and Makielski (1966), but a sophisticated relevant body of literature does not yet exist.

⁹ Thus the stance taken by civic business groups toward growth and land-use matters affecting growth is consistently positive, although the *intensity* of commitment to that goal varies. In his study of New York City zoning, Makielski indicates that “the general business groups . . . approached zoning from an economic viewpoint, although this often led them to share the Reformer’s ideology. Their economic interest in the city gave them a stake in a ‘healthy,’ ‘growing community’ where tax rates were not prohibitive, where city government was ‘efficient,’ and where some of the problems of the urban environment—a constricting labour force, congestion, and lack of space—were being attacked” (1966, p. 141). A similar dynamic has been observed in a medium-size Mexican city: “Despite many other differences, basic agreement on the primacy of stability and growth provides a basis for a dialogue between government and business” (Fagen and Tuohy 1972, p. 56).

¹⁰ Bruce Pringle suggested the latter phrase to me.

floats, and stadia and other forms of support for professional sports teams carrying the locality name. The athletic teams in particular are an extraordinary mechanism for instilling a spirit of civic jingoism regarding the "progress" of the locality. A stadium filled with thousands (joined by thousands more at home before the TV) screaming for Cleveland or Baltimore (or whatever) is a scene difficult to fashion otherwise. This enthusiasm can be drawn upon, with a glossy claim of creating a "greater Cleveland," "greater Baltimore," etc., in order to gain general acceptance for local growth-oriented programs. Similarly, public school curricula, children's essay contests, soapbox derbies, spelling contests, beauty pageants, etc., help build an ideological base for local boosterism and the acceptance of growth. My conception of the territorial bond among humans differs from those cast in terms of primordial instincts: instead, I see this bond as socially organized and sustained, at least in part, by those who have a use for it (cf. Suttles 1972, pp. 111-39). I do not claim that there are no other sources of civic jingoism and growth enthusiasm in American communities, only that the growth-machine coalition mobilizes what is there, legitimizes and sustains it, and channels it as a political force into particular kinds of policy decisions.

The local institution which seems to take prime responsibility for the sustenance of these civic resources—the metropolitan newspaper—is also the most important example of a business which has its interest anchored in the aggregate growth of the locality. Increasingly, American cities are one-newspaper (metropolitan daily) towns (or one-newspaper-company towns), and the newspaper business seems to be one kind of enterprise for which expansion to other locales is especially difficult. The financial loss suffered by the *New York Times* in its futile effort to establish a California edition is an important case in point. A paper's financial status (and that of other media to a lesser extent) tends to be wed to the size of the locality.¹¹ As the metropolis expands, a larger number of ad lines can be sold on the basis of the increasing circulation base. The local newspaper thus tends to occupy a rather unique position: like many other local businesses, it has an interest in growth, but unlike most, its critical interest is not in the specific geographical pattern of that growth. That is, the crucial matter to a newspaper is not whether the additional population comes to reside on the north side or south side, or whether the money is made through a new convention center or a new olive factory. The newspaper has no axe to grind, except the one axe which holds the community elite together: growth. It is for this reason that the newspaper tends to achieve

¹¹ Papers can expand into other industries, such as book publishing and wood harvesting. The point is that, compared with most other industries, they cannot easily replicate themselves across geographical boundaries through chains, branch plants, and franchises.

a statesman-like attitude in the community and is deferred to as something other than a special interest by the special interests. Competing interests often regard the publisher or editor as a general community leader, as an ombudsman and arbiter of internal bickering and, at times, as an enlightened third party who can restrain the short-term profiteers in the interest of more stable, long-term, and properly planned growth.¹² The paper becomes the reformist influence, the "voice of the community," restraining the competing subunits, especially the small-scale, arriviste "fast-buck artists" among them. The papers are variously successful in their continuous battle with the targeted special interests.¹³ The media attempt to attain these goals not only through the kind of coverage they develop and editorials they write but also through the kinds of candidates they support for local office. The present point is not that the papers control the politics of the city, but rather that one of the sources of their special influence is their commitment to growth per se, and growth is a goal around which all important groups can rally.

Thus it is that, although newspaper editorialists have typically been in the forefront expressing sentiment in favor of "the ecology," they tend nevertheless to support growth-inducing investments for their regions. The *New York Times* likes office towers and additional industrial installations in the city even more than it loves the environment. The *Los Angeles Times* editorializes against narrow-minded profiteering at the expense of the environment but has also favored the development of the supersonic transport because of the "jobs" it would lure to Southern California. The papers do tend to support "good planning principles" in some form because such good planning is a long-term force that makes for even more potential future growth. If the roads are not planned wide enough, their narrowness will eventually strangle the increasingly intense uses to which the land will be put. It just makes good sense to plan, and good planning for "sound growth" thus is the key "environmental policy" of the nation's local media and their statesmen allies. Such policies of "good planning" should not be confused with limited growth or conservation: they more typically represent the opposite sort of goal.

Often leaders of public or quasi-public agencies (e.g., universities, utilities) achieve a role similar to that of the newspaper publisher: they become growth "statesmen" rather than advocates for a certain type or

¹² In some cities (e.g., Chicago) it is the political machine that performs this function and thus can "get things done." Political scientists (e.g., Edward Banfield) often identify success in performing this function as evidence of effective local government.

¹³ In his study of the history of zoning in New York City, Makielski remarks: "While the newspapers in the city are large landholders, the role of the press was not quite like that of any of the other nongovernmental actors. The press was in part one of the referees of the rules of the game, especially the informal rules—calling attention to what it considered violations" (1966, p. 189).

intralocal distribution of growth. A university may require an increase in the local urban population pool to sustain its own expansion plans and, in addition, it may be induced to defer to others in the growth machine (bankers, newspapers) upon whom it depends for the favorable financial and public-opinion environment necessary for institutional enhancement.

There are certain persons, ordinarily conceived of as members of the elite, who have much less, if any, interest in local growth. Thus, for example, there are branch executives of corporations headquartered elsewhere who, although perhaps emotionally sympathetic with progrowth outlooks, work for corporations which have no vested interest in the growth of the locality in question. Their indirect interest is perhaps in the existence of the growth ideology rather than growth itself. It is that ideology which in fact helps make them revered people in the area (social worth is often defined in terms of number of people one employs) and which provides the rationale for the kind of local governmental policies most consistent with low business operating costs. Nonetheless, this interest is not nearly as strong as the direct growth interests of developers, mortgage bankers, etc., and thus we find, as Schulze (1961) has observed, that there is a tendency for such executives to play a lesser local role than the parochial, home-grown businessmen whom they often replace.

Thus, because the city is a growth machine, it draws a special sort of person into its politics. These people—whether acting on their own or on behalf of the constituency which financed their rise to power—tend to be businessmen and, among businessmen, the more parochial sort. Typically, they come to politics not to save or destroy the environment, not to repress or liberate the blacks, not to eliminate civil liberties or enhance them. They may end up doing any or all of these things once they have achieved access to authority, perhaps as an inadvertent consequence of making decisions in other realms. But these types of symbolic positions are derived from the fact of having power—they are typically not the dynamics which bring people to power in the first place. Thus, people often become “involved” in government, especially in the local party structure and fund raising, for reasons of land business and related processes of resource distribution. Some are “statesmen” who think in terms of the growth of the whole community rather than that of a more narrow geographical delimitation. But they are there to wheel and deal to affect resource distribution through local government. As a result of their position, and in part to develop the symbolic issues which will enable them (in lieu of one of their opponents or colleagues) to maintain that position of power, they get interested in such things as welfare cheating, busing, street crime, and the price of meat. This interest in the symbolic issues (see Edelman 1964) is thus substantially an aftereffect of a need for power for other purposes. This is not to say that such people don’t “feel strongly” about these matters—they do

sometimes. It is also the case that certain moral zealots and "concerned citizens" go into politics to right symbolic wrongs; but the money and other supports which make them viable as politicians is usually nonsymbolic money.

Those who come to the forefront of local government (and those to whom they are directly responsive), therefore, are not statistically representative of the local population as a whole, nor even representative of the social classes which produce them. The issues they introduce into public discourse are not representative either. As noted by Edelman, the distributive issues, the matters which bring people to power, are more or less deliberately dropped from public discourse (see Schattschneider 1960). The issues which are allowed to be discussed and the positions which the politicians take on them derive from the world views of those who come from certain sectors of the business and professional class and the need which they have to whip up public sentiment without allowing distributive issues to become part of public discussion. It follows that any political change which succeeded in replacing the land business as the key determinant of the local political dynamic would simultaneously weaken the power of one of the more reactionary political forces in the society, thereby affecting outcomes with respect to those other symbolic issues which manage to gain so much attention. Thus, should such a change occur, there would likely be more progressive positions taken on civil liberties, and less harassment of welfare recipients, social "deviants," and other defenseless victims.

LIABILITIES OF THE GROWTH MACHINE

Emerging trends are tending to enervate the locality growth machines. First is the increasing suspicion that in many areas, at many historical moments, growth benefits only a small proportion of local residents. Growth almost always brings with it the obvious problems of increased air and water pollution, traffic congestion, and overtaking of natural amenities. These dysfunctions become increasingly important and visible as increased consumer income fulfills people's other needs and as the natural cleansing capacities of the environment are progressively overcome with deleterious material. While it is by no means certain that growth and increased density inevitably bring about social pathologies (see Fischer, Baldassare, and Ofshe 1974), growth does make such pathologies more difficult to deal with. For example, the larger the jurisdiction, the more difficult it becomes to achieve the goal of school integration without massive busing schemes. As increasing experience with busing makes clear, small towns can more easily have interracial schools, whether fortuitously through spatial proximity or through managed programs.

In addition, the weight of research evidence is that growth often costs existing residents more money. Evidently, at various population levels, points of diminishing returns are crossed such that additional increments lead to net revenue losses. A 1970 study for the city of Palo Alto, California, indicated that it was substantially cheaper for that city to acquire at full market value its foothill open space than to allow it to become an "addition" to the tax base (Livingston and Blayney 1971). A study of Santa Barbara, California, demonstrated that additional population growth would require higher property taxes, as well as higher utility costs (Appelbaum et al. 1974). Similar results on the costs of growth have been obtained in studies of Boulder, Colorado (cited in Finkler 1972), and Ann Arbor, Michigan (Ann Arbor City Planning Department 1972).¹⁴ Systematic analyses of government costs as a function of city size and growth have been carried out under a number of methodologies, but the use of the units of analysis most appropriate for comparison (urban areas) yields the finding that the cost is directly related both to size of place and rate of growth, at least for middle-size cities (see Follett 1976; Appelbaum 1976). Especially significant are per capita police costs, which virtually all studies show to be positively related to both city size and rate of growth (see Appelbaum et al. 1974; Appelbaum 1976).

Although damage to the physical environment and costs of utilities and governmental services may rise with size of settlement, "optimal" size is obviously determined by the sorts of values which are to be maximized (see Duncan 1957). It may indeed be necessary to sacrifice clean air to accumulate a population base large enough to support a major opera company. But the essential point remains that growth is certainly less of a financial advantage to the taxpayer than is conventionally depicted, and that most people's values are, according to the survey evidence (Hoch 1972, p. 280; Finkler 1972, pp. 2, 23; Parke and Westoff 1972; Mazie and Rowlings 1973; Appelbaum et al. 1974, pp. 4.2-4.6) more consistent with small places than large. Indeed, it is rather clear that some substantial portion of the migrations to the great metropolitan areas of the last decade has been more in spite of people's values than because of them. In the recent words of Sundquist: "The notion commonly expressed that Americans have 'voted with their feet' in favor of the great cities is, on the basis of every available sampling, so much nonsense. . . . What is called 'freedom of choice' is, in sum, freedom of employer choice or, more precisely, freedom of choice for that segment of the corporate world that operates mobile

¹⁴ A useful bibliography of growth evaluation studies is Agelasto and Perry (undated). A study with findings contrary to those reported here (Gruen and Gruen Associates 1972) limits cost evaluation to only three municipal services and was carried out in a city which had already made major capital expenditures that provided it with huge unused capacities in water, schools, and sewage.

enterprises. The real question, then, is whether freedom of corporate choice should be automatically honored by government policy at the expense of freedom of individual choice where those conflict" (1975, p. 258).

Taking all the evidence together, it is certainly a rather conservative statement to make that under many circumstances growth is a liability financially and in quality of life for the majority of local residents. Under such circumstances, local growth is a transfer of quality of life and wealth from the local general public to a certain segment of the local elite. To raise the question of wisdom of growth in regard to any specific locality is hence potentially to threaten such a wealth transfer and the interests of those who profit by it.

THE PROBLEMS OF JOBS

Perhaps the key ideological prop for the growth machine, especially in terms of sustaining support from the working-class majority (Levison 1974), is the claim that growth "makes jobs." This claim is aggressively promulgated by developers, builders, and chambers of commerce; it becomes a part of the statesman talk of editorialists and political officials. Such people do not speak of growth as useful to profits—rather, they speak of it as necessary for making jobs. But local growth does not, of course, make jobs: it distributes jobs. The United States will see next year the construction of a certain number of new factories, office units, and highways—regardless of where they are put. Similarly, a given number of automobiles, missiles, and lampshades will be made, regardless of where they are manufactured. Thus, the number of jobs in this society, whether in the building trades or any other economic sector, will be determined by rates of investment return, federal decisions affecting the money supply, and other factors having very little to do with local decision making. All that a locality can do is to attempt to guarantee that a certain proportion of newly created jobs will be in the locality in question. Aggregate employment is thus unaffected by the outcome of this competition among localities to "make" jobs.

The labor force is essentially a single national pool; workers are mobile and generally capable of taking advantage of employment opportunities emerging at geographically distant points.¹⁵ As jobs develop in a fast-growing area, the unemployed will be attracted from other areas in sufficient numbers not only to fill those developing vacancies but also to form

¹⁵ I am not arguing that the labor force is perfectly mobile, as indeed there is strong evidence that mobility is limited by imperfect information, skill limitations, and cultural and family ties. The argument is rather that the essential mobility of the labor force is sufficiently pronounced to make programs of local job creation largely irrelevant to long-term rates of unemployment.

a work-force sector that is continuously unemployed. Thus, just as local growth does not affect aggregate employment, it likely has very little long-term impact upon the local rate of unemployment. Again, the systematic evidence fails to show any advantage to growth: there is no tendency for either larger places or more rapidly growing ones to have lower unemployment rates than other kinds of urban areas. In fact, the tendency is for rapid growth to be associated with higher rates of unemployment (for general documentation, see Follett 1976; Appelbaum 1976; Hadden and Borgatta 1965, p. 108; Samuelson 1942; Sierra Club of San Diego 1973).¹⁶

This pattern of findings is vividly illustrated through inspection of relevant data on the most extreme cases of urban growth: those SMSAs which experienced the most rapid rates of population increase over the last two intercensus decades. Tables 1 and 2 show a comparison of population growth and unemployment rates in the 25 areas which grew fastest during the 1950-60 and 1960-70 periods. In the case of both decade comparisons, half of the urban areas had unemployment rates above the national figure for all SMSAs.

Even the 25 slowest-growing (1960-70) SMSAs failed to experience particularly high rates of unemployment. Table 3 reveals that although all were places of net migration loss less than half of the SMSAs of this group had unemployment rates above the national mean at the decade's end.

Just as striking is the comparison of growth and unemployment rates for all SMSAs in California during the 1960-66 period—a time of general boom in the state. Table 4 reveals that among all California metropolitan areas there is no significant relationship ($r = -17$, $z = .569$) between 1960-66 growth rates and the 1966 unemployment rate. Table 4 is also instructive (and consistent with other tables) in revealing that while there is a wide divergence in growth rates across metropolitan areas, there is no comparable variation in the unemployment rates, all of which cluster within the relatively narrow range of 4.3%-6.5%. Consistent with my previous argument, I take this as evidence that the mobility of labor tends to flatten out cross-SMSA unemployment rates, regardless of widely diverging rates of locality growth. Taken together, the data indicate that local population growth is no solution to the problem of local unemployment.

It remains possible that for some reason certain specific rates of growth may be peculiarly related to lower rates of unemployment and that the measures used in this and cited studies are insensitive to these patterns.

¹⁶ This lack of relationship between local population change and unemployment has led others to conclusions similar to my own: "Economists unanimously have agreed that the only jurisdiction that should be concerned with the effects of its policies on the level of employment is the Federal government. Small jurisdictions do not have the power to effect significant changes in the level of unemployment" (Levy and Arnold 1972, p. 95).

TABLE 1

GROWTH AND UNEMPLOYMENT RATES FOR 25 FASTEST-GROWING SMSAs, 1950-60
(%)

Metropolitan Area	Rate of Growth	Unemployment Rate, 1960
1. Ft. Lauderdale-Hollywood, Fla.	297.9	4.7
2. Anaheim-Santa Ana-Garden Grove, Calif. ..	225.6	4.6
3. Las Vegas, Nev.	163.0	6.7*
4. Midland, Tex.	162.6	4.9
5. Orlando, Fla.	124.6	5.1
6. San Jose, Calif.	121.1	7.0*
7. Odessa, Tex.	116.1	5.6*
8. Phoenix, Ariz.	100.0	4.7
9. W. Palm Beach, Fla.	98.9	4.8
10. Colorado Springs, Colo.	92.9	6.1*
11. Miami, Fla.	88.9	7.3*
12. Tampa-St. Petersburg, Fla.	88.8	5.1
13. Tucson, Ariz.	88.1	5.9*
14. Albuquerque, N. Mex.	80.0	4.5
15. San Bernadino-Riverside-Ontario, Calif.	79.3	6.7*
16. Sacramento, Calif.	74.0	6.1*
17. Albany, Ga.	73.5	4.4
18. Santa Barbara, Calif.	72.0	3.6
19. Amarillo, Tex.	71.6	3.3
20. Reno, Nev.	68.8	6.1*
21. Lawton, Okla.	64.6	5.5*
22. Lake Charles, La.	62.3	7.8*
23. El Paso, Tex.	61.1	6.4*
24. Pensacola, Fla.	54.9	5.3*
25. Lubbock, Tex.	54.7	3.9
Total U.S.	18.5	5.2

SOURCE.—U.S. Bureau of the Census 1962, tables 33, 154.

* Unemployment rate above SMSA national mean.

Similarly, growth in certain types of industries may be more likely than growth in others to stimulate employment without attracting migrants. It may also be possible that certain population groups, by reason of cultural milieu, are less responsive to mobility options than others and thus provide bases for exceptions to the general argument I am advancing. The present analysis does not preclude such future findings but does assert, minimally, that the argument that growth makes jobs is contradicted by the weight of evidence that is available.¹⁷

I conclude that for the average worker in a fast-growing region job security has much the same status as for a worker in a slower-growing region: there is a surplus of workers over jobs, generating continuous

¹⁷ It is also true that this evidence is based on federal data, accumulated through the work of socially and geographically disparate persons who had purposes at hand different from mine. This important reservation can only be dealt with by noting that the findings were consistent with the author's theoretical expectations, rather than antecedents of them. At a minimum, the results throw the burden of proof on those who would argue the opposite hypothesis.

TABLE 2

GROWTH AND UNEMPLOYMENT RATES OF THE 25 FASTEST-GROWING SMSAs, 1960-70
(%)

Metropolitan Area	Rate of Growth	Unemployment Rate, 1970
1. Las Vegas, Nev.	115.2	5.2*
2. Anaheim-Santa Ana-Garden Grove, Calif. ..	101.8	5.4*
3. Oxnard-Ventura, Calif.	89.0	5.9*
4. Ft. Lauderdale-Hollywood, Fla.	85.7	3.4
5. San Jose, Calif.	65.8	5.8*
6. Colorado Springs, Colo.	64.2	5.5*
7. Santa Barbara, Calif.	56.4	6.4*
8. W. Palm Beach, Fla.	52.9	3.0
9. Nashua, N.H.	47.8	2.8
10. Huntsville, Ala.	46.6	4.4
11. Columbia, Mo.	45.8	2.4
12. Phoenix, Ariz.	45.8	3.9
13. Danbury, Conn.	44.3	4.2
14. Fayetteville, Ark.	42.9	5.2*
15. Reno, Nev.	42.9	6.2*
16. San Bernadino-Riverside-Ontario, Calif.	41.2	5.9*
17. Houston, Tex.	40.0	3.0
18. Austin, Tex.	39.3	3.1
19. Dallas, Tex.	39.0	3.0
20. Santa Rosa, Calif.	39.0	7.3*
21. Tallahassee, Fla.	38.8	3.0
22. Washington, D.C.	37.8	2.7
23. Atlanta, Ga.	36.7	3.0
24. Ann Arbor, Mich.	35.8	5.0*
25. Miami, Fla.	35.6	3.7
Total U.S.	16.6	4.3

SOURCE.—U.S. Bureau of the Census 1972, table 3, SMSAs.

* Unemployment rate above the SMSA national mean.

anxiety over unemployment¹⁸ and the effective depressant on wages which any lumpenproletariat of unemployed and marginally employed tends to exact (see, e.g., Bonacich 1975). Indigenous workers likely receive little benefit from the growth machine in terms of jobs; their "native" status gives them little edge over the "foreign" migrants seeking the additional jobs which may develop. Instead, they are interchangeable parts of the labor pool, and the degree of their job insecurity is expressed in the local unemployment rate, just as is the case for the nonnative worker. Ironically, it is probably this very anxiety which often leads workers, or at least their union spokespeople, to support enthusiastically employers' preferred policies of growth. It is the case that an actual decline in local job opportunities, or economic growth not in proportion to natural increase, might induce the hardship of migration. But this price is not the same as, and is less severe than, the price of simple unemployment. It could also rather

¹⁸ For an insightful treatment of joblessness with respect to the majority of the American work force, see Levison (1974).

TABLE 3.
GROWTH, UNEMPLOYMENT, AND NET MIGRATION RATES FOR THE 25 SLOWEST-GROWING
SMSAs, 1960-70
(%)

SMSA	Rate of Growth	Net Migration	Unemployment, 1970
1. Abilene, Tex.	-5.3	-19.7	3.6
2. Altoona, Pa.	-1.4	- 6.6	3.5
3. Amarillo, Tex.	-3.4	-19.5	3.4
4. Brownsville-Harlingen-San Benito, Tex. ...	-7.1	-32.1	6.6*
5. Charleston, W. Va.	-9.3	-19.0	4.1
6. Duluth-Superior, Minn.-Wis.	-4.1	-10.9	7.3*
7. Gadsden, Ala.	-2.9	-12.4	7.3*
8. Huntington-Ashland, W. Va.-Ky.-Ohio	-0.4	- 9.7	5.1*
9. Jersey City, N.J.	-0.5	- 7.5	4.7*
10. Johnstown, Pa.	-6.4	-11.8	4.9*
11. McAllen-Pharr-Edinburgh, Tex.	-0.3	-25.4	5.9*
12. Midland, Tex.	3.4	-19.1	3.5
13. Montgomery, Ala.	0.9	-11.1	3.8
14. Odessa, Tex.	0.9	-16.7	4.3
15. Pittsburgh, Pa.	-0.2	- 7.0	4.3
16. Pueblo, Colo.	-0.4	-12.3	5.9*
17. St. Joseph, Mo.	-4.0	- 9.2	3.9
18. Savannah, Ga.	-0.3	-13.3	4.3
19. Scranton, Pa.	-0.2	- 1.8	5.2*
20. Sioux City, Iowa	-3.2	-13.5	4.4*
21. Steubenville-Weirton, Ohio-W. Va.	-1.3	- 8.9	3.7
22. Utica-Rome, N.Y.	-1.7	-11.2	5.7*
23. Wheeling, W. Va.-Ohio	-4.0	- 8.3	4.2
24. Wichita Falls, Tex.	-2.6	-15.1	4.0
25. Wilkes-Barre-Hazleton, Pa.	-1.3	- 3.5	4.0
Total U.S.	16.6	...	4.3

SOURCE.—U.S. Bureau of the Census 1972, table 3, SMSAs.

* Unemployment rate above SMSA national mean.

easily be compensated through a relocation subsidy for mobile workers, as is now commonly provided for high-salaried executives by private corporations and in a limited way generally by the federal tax deduction for job-related moving expenses.

Workers' anxiety and its ideological consequences emerge from the larger fact that the United States is a society of constant substantial joblessness, with unemployment rates conservatively estimated by the Department of Commerce at 4%-8% of that portion of the work force defined as ordinarily active. There is thus a game of musical chairs being played at all times, with workers circulating around the country, hoping to land in an empty chair at the moment the music stops. Increasing the stock of jobs in any one place neither causes the music to stop more frequently nor increases the number of chairs relative to the number of players. The only way effectively to ameliorate this circumstance is to create a full-employment economy, a comprehensive system of drastically increased unemployment insurance, or some other device which breaks the connection between

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TABLE 4
GROWTH AND UNEMPLOYMENT RATES FOR ALL CALIFORNIA SMSAs, 1960-66
(%)

SMSA	Rate of Growth, 1960-66	Average Annual Change	Unemployment Rate, 1966
Anaheim-Santa Ana-Garden Grove	65.0	8.3	4.3
Bakersfield	11.1	1.7	5.2
Fresno	12.3	1.9	6.5
Los Angeles-Long Beach	11.9	1.9	4.5
Modesto
Oxnard-Ventura	68.8	8.7	6.0
Sacramento	20.0	3.0	5.2
Salinas-Monterey	15.9	2.4	6.1
San Bernadino-Riverside	27.9	4.0	6.2
San Diego	14.0	2.1	5.1
San Francisco-Oakland	11.1	1.7	4.4
San Jose	44.8	6.1	4.8
Santa Barbara	48.7	6.6	4.5
Santa Rosa
Stockton	12.5	1.9	6.3
Vallejo-Napa	20.6	3.0	4.4
California mean	27.47	3.80	5.25

SOURCES.—For average annual change and rate of growth, U.S. Bureau of the Census 1969, table 2; for unemployment rate, 1966, State of California 1970, table C-10.

a person's having a livelihood and the remote decisions of corporate executives. Without such a development, the fear of unemployment acts to make workers politically passive (if not downright supportive) with respect to land-use policies, taxation programs, and antipollution nonenforcement schemes which, in effect, represent income transfers from the general public to various sectors of the elite (see Whitt 1975). Thus, for many reasons, workers and their leaders should organize their political might more consistently not as part of the growth coalitions of the localities in which they are situated, but rather as part of national movements which aim to provide full employment, income security, and programs for taxation, land use, and the environment which benefit the vast majority of the population. They tend not to be doing this at present.

THE PROBLEM OF NATURAL INCREASE

Localities grow in population not simply as a function of migration but also because of the fecundity of the existing population. Some means are obviously needed to provide jobs and housing to accommodate such growth—either in the immediate area or at some distant location. There are ways of handling this without compounding the environmental and budgetary problems of existing settlements. First, there are some localities which are, by many criteria, not overpopulated. Their atmospheres are clean, water

supplies plentiful, and traffic congestion nonexistent. In fact, in certain places increased increments of population may spread the costs of existing road and sewer systems over a larger number of citizens or bring an increase in quality of public education by making rudimentary specialization possible. In the state of California, for example, the great bulk of the population lives on a narrow coastal belt in the southern two-thirds of the state. Thus the northern third of the state consists of a large unpopulated region rich in natural resources, including electric power and potable water. The option chosen in California, as evidenced by the state aqueduct, was to move the water from the uncrowded north to the dense, semiarid south, thus lowering the environmental qualities of both regions, and at a substantial long-term cost to the public budget. The opposite course of action was clearly an option.

The point is that there are relatively underpopulated areas in this country which do not have "natural" problems of inaccessibility, ugliness, or lack of population-support resources. Indeed, the nation's most severely depopulated areas, the towns of Appalachia, are in locales of sufficient resources and are widely regarded as aesthetically appealing; population out-migration likely decreased the aesthetic resources of both the migrants to and residents of Chicago and Detroit, while resulting in the desertion of a housing stock and utility infrastructure designed to serve a larger population. Following from my more general perspective, I see lack of population in a given area as resulting from the political economic decisions made to populate other areas instead. If the process were rendered more rational, the same investments in roads, airports, defense plants, etc., could be made to effect a very different land-use outcome. Indeed, utilization of such deliberate planning strategies is the practice in some other societies and shows some evidence of success (see Sundquist 1975); perhaps it could be made to work in the United States as well.

As a long-term problem, natural increase may well be phased out. American birth rates have been steadily decreasing for the last several years, and we are on the verge of a rate providing for zero population growth. If a stable population actually is achieved, a continuation of the present inter-local competitive system will result in the proliferation of ghost towns and unused capital stocks as the price paid for the growth of the successful competing units. This will be an even more clearly preposterous situation than the current one, which is given to produce ghost towns only on occasion.

THE EMERGING COUNTERCOALITION

Although growth has been the dominant ideology in most localities in the United States, there has always been a subversive thread of resistance.

Treated as romantic, or as somehow irrational (see White and White 1962), this minority long was ignored, even in the face of accumulating journalistic portrayals of the evils of bigness. But certainly it was an easy observation to make that increased size was related to high levels of pollution, traffic congestion, and other disadvantages. Similarly, it was easy enough to observe that tax rates in large places were not generally less than those in small places; although it received little attention, evidence that per capita government costs rise with population size was provided a generation ago (see Hawley 1951). But few took note, though the very rich, somehow sensing these facts to be the case, managed to reserve for themselves small, exclusive meccas of low density by tightly imposing population ceilings (e.g., Beverly Hills, Sands Point, West Palm Beach, Lake Forest).

In recent years, however, the base of the antigrowth movement has become much broader and in some localities has reached sufficient strength to achieve at least toeholds of political power. The most prominent cases seem to be certain university cities (Palo Alto, Santa Barbara, Boulder, Ann Arbor), all of which have sponsored impact studies documenting the costs of additional growth. Other localities which have imposed growth controls tend also to be places of high amenity value (e.g., Ramapo, N.Y.; Petaluma, Calif.; Boca Raton, Fla.). The antigrowth sentiment has become an important part of the politics of a few large cities (e.g., San Diego) and has been the basis of important political careers at the state level (including the governorship) in Oregon, Colorado, and Vermont. Given the objective importance of the issue and the evidence on the general costs of growth, there is nothing to prevent antigrowth coalitions from similarly gaining power elsewhere—including those areas of the country which are generally considered to possess lower levels of amenity. Nor is there any reason, based on the facts of the matter, for these coalitions not to further broaden their base to include the great majority of the working class in the localities in which they appear.

But, like all political movements which attempt to rely upon volunteer labor to supplant political powers institutionalized through a system of vested economic interest, antigrowth movements are probably more likely to succeed in those places where volunteer reform movements have a realistic constituency—a leisured and sophisticated middle class with a tradition of broad-based activism, free from an entrenched machine. At least, this appears to be an accurate profile of those places in which the antigrowth coalitions have already matured.

Systematic studies of the social make up of the antigrowth activists are only now in progress (e.g., Fitts 1976), but it seems that the emerging countercoalition is rooted in the recent environmental movements and relies on a mixture of young activists (some are veterans of the peace and

civil rights movements), middle-class professionals, and workers, all of whom see their own tax rates as well as life-styles in conflict with growth. Important in leadership roles are government employees and those who work for organizations not dependent on local expansion for profit, either directly or indirectly. In the Santa Barbara antigrowth movements, for example, much support is provided by professionals from research and electronics firms, as well as branch managers of small "high-technology" corporations. Cosmopolitan in outlook and pecuniary interest, they use the local community only as a setting for life and work, rather than as an exploitable resource. Related to this constituency are certain very wealthy people (particularly those whose wealth derives from the exploitation of nonlocal environments) who continue a tradition (with some modifications) of aristocratic conservation.¹⁹

Should it occur, the changes which the death of the growth machine will bring seem clear enough with respect to land-use policy. Local governments will establish holding capacities for their regions and then legislate, directly or indirectly, to limit population to those levels. The direction of any future development will tend to be planned to minimize negative environmental impacts. The so-called natural process (see Burgess 1925; Hoyt 1939) of land development which has given American cities their present shape will end as the political and economic foundations of such processes are undermined. Perhaps most important, industrial and business land users and their representatives will lose, at least to some extent, the effectiveness of their threat to locate elsewhere should public policies endanger the profitability they desire. As the growth machine is destroyed in many places, increasingly it will be the business interests who will be forced to make do with local policies, rather than the local populations having to bow to business wishes. New options for taxation, creative land-use programs, and new forms of urban services may thus emerge as city government comes to resemble an agency which asks what it can do for its people rather than what it can do to attract more people. More specifically, a given industrial project will perhaps be evaluated in terms of its social utility—the usefulness of the product manufactured—either to the locality or to the society at large. Production, merely for the sake of local expansion, will be less likely to occur. Hence, there will be some pressure to increase the use value of the country's production apparatus and for external costs of production to be borne internally.

¹⁹ Descriptions of the social makeup of American environmentalists (who coincide as a group only roughly with the no-growth activists) and of their increasing militancy are contained in Nash (1967), Bartell (1974), Dunlap and Gale (1972), Faich and Gale (1971). For a journalistic survey of no-growth activities, see Robert Cahn, "Mr. Developer, Someone Is Watching You" (*Christian Science Monitor* [May 21, 1973], p. 9). A more comprehensive description is contained in Reilly (1973).

When growth ceases to be an issue, some of the investments made in the political system to influence and enhance growth will no longer make sense, thus changing the basis upon which people get involved in government. We can expect that the local business elites—led by land developers and other growth-coalition forces—will tend to withdraw from local politics. This vacuum may then be filled by a more representative and, likely, less reactionary activist constituency. It is noteworthy that where antigrowth forces have established beachheads of power, their programs and policies have tended to be more progressive than their predecessors'—on all issues, not just on growth. In Colorado, for example, the environmentalist who led the successful fight against the Winter Olympics also successfully sponsored abortion reform and other important progressive causes. The environmentally based Santa Barbara "Citizens Coalition" (with city government majority control) represents a fusion of the city's traditional left and counterculture with other environmental activists. The result of the no-growth influence in localities may thus be a tendency for an increasing progressiveness in local politics. To whatever degree local politics is the bedrock upon which the national political structure rests (and there is much debate here), there may follow reforms at the national level as well. Perhaps it will then become possible to utilize national institutions to effect other policies which both solidify the death of the growth machine at the local level and create national priorities consistent with the new opportunities for urban civic life. These are speculations based upon the questionable thesis that a reform-oriented, issue-based citizens' politics can be sustained over a long period. The historical record is not consistent with this thesis; it is only emerging political trends in the most affected localities and the general irrationality of the present urban system that suggest the alternative possibility is an authentic future.

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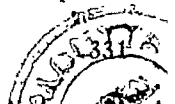
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Industrialization and the Stratification of Cities in Suburban Regions¹

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It is proposed that the process of suburban growth since 1950 has been strongly influenced by the suburbanization of industrial and commercial employment. Suburban communities have responded to the new conditions of growth in ways that create a new pattern of suburban differentiation, one in which there is stratification by both social class and fiscal wealth to the disadvantage of poorer residents. A theoretical model of this growth process is presented and illustrated by the case of cities in a recently growing, industrializing suburban SMSA.

The urban-suburban dichotomy has become a major theme of urban sociology. Extensive comparisons have been made of the socioeconomic characteristics of the two parts of the metropolis (see Martin 1956). These comparisons are often conceived in terms of stratification, defining the differences between city and suburb as social and economic disparities. According to this view, the "suburban wall" institutionalizes the segregation of classes and races as well as an inequitable distribution of public wealth relative to need between the two parts of the metropolis; the inequities are exacerbated by exclusionary suburban zoning policies and the recent movement of employment opportunities and the industrial tax base out of central cities (Long 1967; Advisory Commission 1965).

As suburban regions continue to expand it becomes increasingly important to understand the process of differentiation *within* suburbia. Indeed, the small size and multiplicity of suburban municipalities within a metropolitan region would seem to provide a favorable basis for specialized patterns of growth. Thus Danielson (1972, p. 145) concludes that "political fragmentation in the metropolis is primarily a suburban phenomenon, as is the institutionalization of socioeconomic differentiation along local political boundaries." Some research has already been reported on the implications of suburban differentiation for social inequality (Branfman, Cohen, and Trubek 1974; Hill 1974). It is the purpose of the present study to outline in more detail the contemporary pattern of stratification among

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suburban municipalities and to explore its sources in the conditions and processes of suburban growth.

I. SOCIOECONOMIC DIFFERENCES AMONG SUBURBS: 1950 AND 1970

Researchers have long recognized the existence of marked differences among suburbs. Douglass ([1925] 1970) distinguished and analyzed the characteristics of "residential" and "industrial" suburbs, classified according to the occupational distributions of residents. Later studies of "working-class" suburbs have re-emphasized the class heterogeneity of suburbanites (Berger 1960). Much of the systematic research has classified suburban cities according to their economic functions (employing vs. residential)² and explored differences in various characteristics of their populations, such as income, occupation, education, age, density, and rate of growth (Harris 1943; Dornbusch 1952; Jones 1953; Reiss 1956; Schnore 1956).

The typical suburban pattern of 1950 as reported by Collver (1958) is presented in table 1. With the exception of the most recently developed

TABLE 1
MEDIAN GROSS MONTHLY RENT OF SUBURBS BY CITY TYPE IN THE NINE LARGEST SMSAs, 1950 (\$)

REGION	CITY TYPE		
	Employing	Balanced	Residential
New York	52.91	52.27	60.45
Philadelphia	45.13	46.15	60.47
Boston	49.41	54.16	58.54
Pittsburgh	40.00	41.78	57.07
Chicago	46.89	54.81	58.35
Detroit	45.36	48.21	55.96
St. Louis	42.57 ^a	38.39	53.74
Los Angeles	53.39	47.47	53.00
San Francisco	47.15	47.53	51.39

SOURCE.—Collver 1958, p. 76.

^a Based on one case.

region, Los Angeles, employing suburbs had *lower-status* populations than residential or balanced suburbs. Other studies for that time showed that employing suburbs also were older, larger, and located farther from the central cities, had higher densities, and grew less rapidly in the previous decade. Indeed, employing suburbs were shown to have many of the characteristics of central cities.

² Economic function is often measured by the employment/residents ratio: the ratio of local employment in manufacturing and trade to the number of local residents employed in these areas. This measure is used in lieu of information on the location of place of work of residents of individual suburbs.

Although the differences among suburbs in 1950 are well documented, they have not been satisfactorily explained. That is, the research on suburban differentiation has provided little beyond the essentially *descriptive* concept of "areal specialization" as an explanation of the processes of growth which created the particular pattern of differences. According to the best evidence, the employing suburbs of 1950 were formerly independent industrial centers which predated the suburbanization of middle- and upper-income households in the 1940s (Schnore 1957). What then requires explanation is the pattern of the flow of the new population to undeveloped or unincorporated areas of the suburban ring, and the segregation of different income groups into different suburban cities during the 1940s.

The composition of suburban growth—and therefore possibly the pattern of differentiation among suburbs—has changed considerably since 1950. While growth prior to that time was primarily residential, more recent growth has been marked by a new movement of industry into suburbia. Between 1958 and 1963, while dropping 6% in central cities, manufacturing employment grew 16% in the suburbs (Douglass Commission 1969, pp. 413–14). During the decade of the 1960s, while the labor force living in suburban areas increased 40%, total suburban employment increased 48%. What is the impact of industrialization on suburban regions, and how has this development been distributed?

There is little published research concerning these questions. Schnore (1963) found that the employing suburbs of 1950 maintained in 1960 their original population characteristics, and Farley (1964) has shown that the socioeconomic composition of suburbs which existed in 1920 remained stable through 1960 despite population growth in the intervening years. But no study has taken into account the many new suburbs incorporated after 1950. As an initial step in exploring post-1950 growth, the 1970 characteristics of new Chicago suburbs are contrasted here with those of suburbs which were incorporated and had attained a population over 10,000 by 1950 (table 2).

The 27 original suburbs maintained their initial differences: while employment increased uniformly in each type of suburb, the employing suburbs still have the lowest average median household income. Among the 60 new suburbs, however, the pattern is reversed: the average median income in employing suburbs (\$14,016) is *higher* than in any other category, including older residential suburbs. This is the same pattern as previously found in Los Angeles (the exception reported in table 1).

In the Chicago region, new industrial and commercial growth at the present time tends to locate in suburban cities which already have or soon attain relatively high-income populations. Yet this is a purely descriptive statement. In order to understand why these suburbs develop

TABLE 2

Median Employment/Residents Ratio and Average Median Household Income
of Chicago Suburbs, by City Type, 1950 and 1970

City Group	Median Employment/ Residents Ratio	Average Median Household Income (\$)
I:		
27 original suburbs, 1950	0.60	4,258
8 residential (0-40)	0.275	4,753
12 balanced (41-100)	0.61	4,102
7 employing (over 100)	1.24	3,961
II:		
27 original suburbs, 1970 ^a	0.69	11,917
8 residential (0-40)	0.335	12,932
12 balanced (41-100)	0.735	12,023
7 employing (over 100)	1.42	10,577
III:		
60 new suburbs, 1970	0.40	13,314
22 residential (0-30)	0.09	11,458
20 balanced (31-85)	0.42	11,424
18 employing (over 85)	1.19	14,016

SOURCES.—Calculated from data published in *U.S. Census of Population*, 1950 and 1970, and *U.S. Census of Business and Manufacturers*, 1954 and 1972.

^a These original cities are categorized by their 1950 E/R ratio.

as they do, to project this growth under changing conditions, and to predict what other regions may share the same pattern of differentiation, it is necessary to explore the *process of growth*. What are the determinants of industrial and commercial location? What causes the segregation of income groups into different suburbs? Why are employing suburbs no longer the place of residence of the least affluent suburbanites?

II. TOWARD A THEORY OF SUBURBAN GROWTH

This section will develop a model of suburbanization during the post-1950 period. It is composed of two elements. The first is a modification of classical theories of land economics to explain new suburban industrial and commercial location. The second is a theory of the social organization of land development in suburbia.

Suburban Land Economics

The classical land-use models predicted that the greatest intensity of both residential and nonresidential activity would be at the region's center, as a result of competition for accessibility (Burgess 1925; Alonso 1961). Detailed analysis of the marginal utility of central location for various users of urban space provided a theoretical mapping which places employment and dense, low-income residences at the center and low-density,

higher-income residences at the periphery of the region. This mapping is consistent with much of the reported research on metropolitan regions (Berry, Simmons, and Tennant 1963).

There is no reason to challenge the principal assumption of those models—that choice of location is based upon competition as reflected by land values. However, two types of changes have taken place which call into question the value of centrality for industry. First, owing to increased public investment in transport systems, the importance of distance as a determinant of cost has declined for most industries (at least, within the range of a suburban region). Concurrently a new cost factor has become significant: the fragmentation of local governments in growing suburban regions has introduced a wide variety of tax rates and public services which directly affect profitability and among which businesses may choose. Within a suburban region where there is access to the same metropolitan market, work force, and transport facilities, it is reasonable to expect that the location of new employment (other than small-scale retail trade which must be distributed according to population) will be strongly influenced by fiscal differences among suburbs (Advisory Commission 1967; Wood 1958, pp. 206–25; Netzer 1966, pp. 124–36).

It cannot be supposed that fiscal considerations constitute the primary influence on industrial and commercial location. While industries, regional shopping centers, and large service facilities seek a low tax rate and a high level of public services, they also require good highway or rail access to and from other communities, availability of land in sufficient amounts, suitable topography, and an amenable social and political context. Many of these factors are subject to change. Perhaps one of the inducements for suburban location is the manipulability of the fiscal and geographic structure of relatively undeveloped fringe areas.

There exists a dynamic interaction between employment location and other characteristics of suburban cities. Business seeks the most favorable among available sites. Particularly since some suburbs are usually more attractive than others, leading to an early concentration of new industry and commercial uses, the choice of business location has profound influences on the ecological structure of the region:

a) In contrast to rapid residential expansion, which places large public service demands on suburban municipalities and school districts, industry has been thought to provide a net fiscal benefit. Business firms add substantially to taxable property value without raising school enrollments, as long as employees tend to find homes in other communities.

b) The fiscal benefit of industrial and commercial growth can be translated into lower taxes and/or improved public services. Low taxes in turn make the community attractive as a location for additional homes and businesses, and demand for building sites rises. Competition forces up the

market value of land, and as land prices rise, working-class households find it increasingly difficult to pay the price for homesites in the community. If a new plant opens in the community its workers will likely find it more feasible to live in other communities where land prices are lower. This might explain in part the relatively high income levels of new employing suburbs shown in table 2.

c) In response to the growth of industrial and commercial suburbs, neighboring communities may attempt to protect their schools and other services from excessive demand by sharply limiting new housing. Yet the new population must be housed in some community; meeting resistance in some areas, these families will find their way to one or more areas with low land values and with few geographic and political constraints on construction. Such growth in turn will cause deterioration of these communities' fiscal conditions, making them still less attractive to industry.

Industry's search for a favorable fiscal context thus implies a continuing cycle of advantage for some suburbs and disadvantage for others.

Social Organization of Suburban Growth

The discussion to this point has involved only economic theories—the economics of the firm, public budgets, and land value. It begins to suggest what is at stake in suburban growth for businesses, residents, and public agencies, but one cannot safely predict growth on the basis of economics without understanding the social forces which constrain development. It may be useful here to recall William Form's argument in favor of a sociological analysis of land-use patterns, published over twenty years ago: "The first step is to analyze the social forces operating in the land market. Obviously the image of a free and unorganized market in which individuals compete impersonally for land must be abandoned. The reason for this is that the land market is highly organized and dominated by a number of interacting organizations. Most of the latter are formally organized, highly self-conscious, and purposeful in character" (Form 1954, p. 317).

Of these organizations, the most identifiable is the local government which may, by its policies of zoning, subdivision control, expenditures, and taxation, attempt to induce a "desirable" pattern of development. This is often presumed to involve maximizing revenues and minimizing costs: "In the case of the residential suburb, the usual analysis is to estimate the city government's costs and revenues arising from each possible land use and then to use zoning ordinances and capital improvement programs to encourage the fiscally most 'profitable' uses" (Margolis 1957, pp. 225–27).

Researchers have discussed two kinds of growth strategies which fit the general category of "fiscal zoning" (Coke and Gargan 1969; Danielson

1972). One is to limit residential growth through zoning subdivision controls. The other is to attract new industry and commerce by stabilizing the tax rate and/or improving municipal services. In principle, if growth at the present time is dominated by competitive fiscal zoning, suburbs should attempt to combine both of these strategies, aspiring to the ideal of an exclusive residential community supported by a strong industrial and commercial tax base. In fact, suburbs have followed a variety of strategies, including even the promotion of extensive residential tract development or prohibition of industry. This may be explained only by taking into account the "profitability" of various growth policies to the interest groups which may influence local policy: real estate developers, big business, and residents.

1. *Resistance to industrial growth.*—If we consider first the case of suburbs which experience industrial and commercial growth, the interests of these groupings are clear. Real estate developers profit from the rapid inflation of land values and intensive construction. Industries and large commercial firms benefit from the combination of low taxes and quality public services. The impact on residents varies according to social class and tenure: lower-income residents, particularly renters, suffer under increasing housing costs, while higher-income homeowners benefit from improved property values. Residents derive some benefit from the suburb's fiscal wealth, but it should be recognized that they may consider the changing character of their community a counterbalancing cost of development.

The "quality of community" is a significant value among suburban residents, particularly in older, more established, or more rural communities. The issue of growth may therefore create a strong political cleavage in employing suburbs, so that the pursuit of intensive development depends on the ability of the real estate and big business coalition to dominate local politics. Even if growth is not challenged from the outset, "residentialist" opposition may grow steadily as the social costs of intensive development begin to outweigh increments in fiscal wealth.

2. *The politics of rapid residential growth.*—Residents of rapidly growing, relatively dense, low-income residential suburbs bear the public costs of the employment growth of industrial-commercial suburbs. It is for this reason that Wood (1958) refers to them as the "un-annointed." Yet their acquiescence in the growth pattern is not simply passive. The newer residents are content to have found homes at reasonable cost in the region. Local business, particularly retail trade, grows proportionately with population. And the real estate industry benefits from a quick turnover of large tracts of previously cheap, undeveloped land and from the construction and financing of new housing for a mass market. Under these conditions, what is in principle "profitable" for *municipal* budgets has little

influence upon local land-use decisions, and new subdivisions may be routinely approved.

3. *Restricting growth: exclusive residential suburbs.*—Growth is not inevitable in existing suburbs; municipal incorporation itself has in some communities the expressed purpose of obtaining land-use controls with which to restrict growth (see Warren 1966). Exclusion of both industry and dense (and lower-income) residential development can be seen as an effort to preserve a particular community life style. It requires that local residents be sufficiently organized to assume control of municipal politics. It also presupposes a sufficient residential tax base to support basic public services. Public expenditures may be maintained at very low levels in order to meet this condition, and new housing may be allowed only at low density and high value in order to maximize the ratio of property value to population. Thus the large-lot zoning typical of exclusive suburbs is a reflection of both community values and fiscal planning.

In principle any community could attempt to restrict industrial growth. The pressures for more intensive development, stemming from business or real estate developers and landowners, become less resistable, however, in suburbs which find it difficult to support the cost of services—especially schools. In practice, then, the exclusive residential strategy is available only to the most affluent suburbs.

Growth in a Regional System of Cities

The image presented here is one of a system of suburban cities in which the characteristics of any particular suburb depend upon its internal constellation of political influences and the conditions imposed upon it by the development of neighboring communities. The types of suburbs which evolve are functionally interdependent—the growth of employing suburbs depends upon the availability of new housing in residential suburbs, while many of the residents of exclusive residential suburbs work elsewhere in the region.

The particular pattern of differentiation hypothesized here depends upon certain assumptions which hold broadly across the United States, which may be specified as follows (see Netzer 1968, p. 28): (a) Public decisions affecting growth, including both zoning and taxation, are made by many relatively small local governments. (b) Local governments rely heavily on revenues which depend upon the land-use pattern, including property and sales taxes. (c) Growth of the region is stimulated by expansion of employment opportunities.

Some of these practices are currently under legal challenge. The California State Supreme Court has found the system of local property taxes to be an unconstitutionally inequitable method of school financing, and

this decision has been repeated in other states. Numerous attacks have been made against local zoning codes which prohibit multiple-unit housing, and the New Jersey courts have recently ruled that local zoning must be responsive to the general welfare of the surrounding communities. Although the effects of such decisions are still to be seen, it should be kept in mind what has been proposed here, that these institutions are at the heart of the stratification of suburban cities, conditioning the specialization of cities into three stable types (see Williams et al. 1965): (a) industrial-commercial cities with high local employment, a strong tax base, high expenditures and low tax rate, moderately wealthy population and high home values; (b) intensive residential cities with some local employment, a poor tax base and high tax rate, low home values, low income population, and rapid residential growth; and (c) exclusive residential cities with little local employment, strong residential tax base, low expenditures and tax rate, a wealthy population, high home values and low residential density.

III. AN EMPIRICAL TEST: THE CASE OF SANTA CLARA COUNTY

The competitive growth process described above may be explored in suburban regions which (1) assign zoning powers to local jurisdictions, (2) rely heavily on locally collected property taxes to support public services, and (3) have begun to balance population growth with expansion of employment centers. The first two conditions are found generally, although with some variation, throughout the United States. The third has become common: of the 10 largest metropolitan regions in 1970, six had experienced a greater percentage of growth in suburban employment than in suburban population during the 1960s (Cleveland, Detroit, Chicago, Boston, New York-Newark, and Philadelphia).

The present research focuses on the case of Santa Clara County, California (the San Jose SMSA). The county has developed principally since 1950 (from a population of 290,000 in 1950 to over 1 million in 1970) and is the fastest growing suburban region of the San Francisco Bay Area. Its share of Bay Area manufacturing employment rose from 27% in 1958 to 46% in 1967, while its share of new industrial building permits rose from 20% in 1955 to 38% in 1970.³ Owing to the continued growth of its electronics industry, originally established in the 1950s, Santa Clara County experienced the most rapid growth of manufacturing employment of any suburban county in the United States during 1967-72.⁴ The region is therefore a particularly interesting case for the study of growth in industrializing suburban regions.

³ Reported by the Research Department, Bay Area Council, San Francisco, California.

⁴ This and similar information on the region is compiled in Security Pacific Bank's "Special Report on the Economy of Santa Clara County," 1974.

All 15 cities in the region had been incorporated by 1960, and it is possible to study in detail the development pattern between the 1960 and 1970 census.

Methods and Results

1. *Patterns of differentiation: 1960 and 1970.*—Consistent with the prior literature, cities were classified for both 1960 and 1970 on the basis of their economic function as measured by the ratio of employees to residents (E/R ratio). The three categories of residential, balanced, and employing were formed for each year by dividing the interval scale at the natural cut-off points, the average residential city having an E/R ratio of approximately 0.10, the average balanced city about 0.60, and the average employing city between 1.50 and 2.00.⁵ Cities were then compared across these categories on the basis of several other relevant characteristics: average median family income, average median home value, proportion of housing units for single-family use, property assessed valuation per capita, tax rate, and municipal expenditures per capita. Table 3 summarizes an analysis of variance of these characteristics as predicted by city type.

The 1960 residential cities could be termed "exclusive residential." They had high levels of income and median home value, they were almost exclusively composed of single-family houses, they had a high per capita property tax base, low expenditures, and very low taxes. There were, however, only small differences between balanced and employing cities. As reported in previous research, the employing suburbs of 1960 had a lower average median home value, higher taxes, and higher municipal expenditures, although they were not more densely settled nor did they have a lower family income level than balanced cities.

By 1970 the pattern of differences was much more sharply defined. In five out of six comparisons the 1970 categorization of cities explains more variance in a given city characteristic than the 1960 categorization. The sixth case shows a slightly higher explained variance in 1960, but here there is a theoretically important reversal in the direction of differences. The employing cities, which in 1960 had a relatively high tax rate, had a lower tax rate in 1970 than did the balanced cities. They also had a very strong property tax base and a higher family income and house value than did balanced cities. Finally, they had by far the lowest proportion of single-

⁵ The actual ranges of E/R ratios in 1960 and 1970 are 0–0.33 for residential, 0.42–0.93 for balanced, and 1.12–3.10 for employing suburbs. The cut-off point for residential cities is somewhat lower than used in the Municipal Yearbook (around 0.85). The latter cut-off point, if applied to the Santa Clara data, would combine quite dissimilar suburbs in the residential category.

TABLE 3
ANALYSIS OF VARIANCE OF CITY CHARACTERISTICS PREDICTED BY CITY TYPE, SANTA CLARA COUNTY, 1960 AND 1970

	μ All Cities	μ Residential	μ Balanced	μ Employing	$F_{1,12}$	η
Median family income:						
1960	\$ 8,373	\$10,750	\$ 7,500	\$ 7,525	3.93*	.39
1970	\$14,540	\$18,320	\$11,657	\$13,300	14.00***	.70
Median home value:						
1960	\$19,813	\$26,350	\$17,628	\$17,100	5.67*	.48
1970	\$32,346	\$43,360	\$25,200	\$30,667	24.51***	.80
Single family:						
1960	88.8%	97.0%	86.7%	84.5%	3.02	.33
1970	75.1%	90.2%	72.7%	55.6%	10.39**	.63
Assessed value per capita:						
1959/60	\$ 2,218	\$ 2,507	\$ 2,092	\$ 2,150	0.49	.07
1969/70	\$ 2,680	\$ 2,872	\$ 2,172	\$ 3,543	6.08*	.50
Tax per \$100 assessed valuation:						
1959/60	\$ 0.95	\$ 0.17	\$ 1.19	\$ 1.32	21.11***	.77
1969/70	\$ 1.05	\$ 0.33	\$ 1.53	\$ 1.13	15.81***	.72
Expenditures per capita:						
1959/60	\$ 45.2	\$ 31.5	\$ 42.2	\$ 64.2	2.26	.27
1969/70	\$ 88.8	\$ 40.0	\$ 92.8	\$160.6	17.03***	.74

NOTE.—On the basis of their E/R ratios in 1960, there were 4 residential cities, 7 balanced cities, and 4 industrial cities. For 1970, there were 5 residential cities, 7 balanced cities, and 3 industrial cities, with several changes of category by individual cities.

* $P \leq .05$.
 ** $P \leq .01$.
 *** $P \leq .001$.

family residences, but this reflects the cost of land more than the poverty of residents.

In short, there were some differences among cities in 1960, primarily distinguishing the exclusive residential cities as a separate type. By 1970, after a decade of industrial growth in the region, the differentiation of cities was more marked and fit clearly the typology described in the previous section. Employing and balanced cities had exactly the characteristics attributed to the "industrial-commercial" and "intensive residential" types.

2. *Patterns of differentiation: factorial ecology.*—An alternative method of studying regional differentiation, not requiring a priori classification along a single dimension such as the E/R ratio, is provided by the numerous multivariate taxonomic techniques. The most commonly used in problems of this nature is factor analysis (Rees 1971; Schwirian 1974). Tables 4 and 5 present the results of a principal components factor analysis of

TABLE 4
PRINCIPAL COMPONENTS FACTOR STRUCTURE FOR SANTA CLARA COUNTY SUBURBS, 1960

VARIABLE	FACTOR LOADINGS		
	Factor 1	Factor 2	Factor 3
E/R ratio	-.80
Median household income77	.50	...
Median home value81	.44	...
% single-family residences77
Property tax base per capita43	.57	...
Municipal tax rate	-.84
Capital assets per capita	-.52	.69	...
Expenditures per capita	-.75	.45	...
Sales tax income per capita	-.41	-.41	.72
Population growth 1950-60	-.55	.68	...
Ratio residential/employment growth 1960-63	-.86
Proportion of variance explained42	.20	.13

NOTE.—Includes only weightings above .40.

selected economic, population, and housing characteristics of Santa Clara County cities for 1960 and 1970.⁶ A three-factor solution explains 75% of the variance in the 1960 characteristics and 85% in 1970.

The first factor in each year can be clearly identified as an "exclusive residential" factor: the variables of family income, home value, and proportion of single-family residences all have high positive loadings, while measures of industrial and commercial activity and scale of municipal government have high negative loadings.

⁶ The ratio of residential to employment growth is actually the ratio of declared value of new residential to nonresidential construction, based on building permits compiled by Security Pacific Bank.

Suburban Industrialization and Stratification

TABLE 5

PRINCIPAL COMPONENTS FACTOR STRUCTURE FOR SANTA CLARA COUNTY SUBURBS, 1970

VARIABLE	FACTOR LOADINGS		
	Factor 1	Factor 2	Factor 3
E/R ratio	-.73	.61	...
Median household income86	.46	...
Median home value86	.47	...
% single-family residences84
Property tax base per capita95	...
Municipal tax rate	-.73	-.48	...
Capital assets per capita	-.64	.67	...
Expenditures per capita	-.86	.45	...
Sales tax income per capita	-.43	-.83
Population growth 1950-6056
Ratio residential/employment growth 1970-7390
Proportion of variance explained42	.26	.17

NOTE.—Includes only weightings above .40.

The second and third factors in 1960 have no evident relationship to the typology developed above. However by 1970, the second factor can be identified as an "industrial-commercial" factor. While, like factor 1, it has positive loadings on median income and home value, it has a *positive* loading on the E/R ratio, property tax base, and municipal assets and expenditures. The third factor in 1970 can be considered an "intensive residential" factor: it has strong negative weighting on sales tax income (a measure of the retail sales volume) and high positive weightings on population growth and residential building activity. These results are consistent with the theoretical model which predicts a development over time of a particular three-factor structure corresponding to the competitive industrial growth process.

The changes in the region's factor structure can be summarized systematically through factor comparison techniques, in which the 1960 structure is rotated into the 1970 factor space, and differences in factor loadings of the rotated and target structures are computed.⁷ The results of such a procedure are presented in table 6. The greatest change between 1960 and 1970 has occurred in three variables: the employment/residents ratio, property tax base, and rate of population growth. The 1970 pattern represents a change toward a higher association of fiscal wealth and magnitude of local employment (factor 2), a lower association of exclusive residences and fiscal wealth (factor 1), and a higher association of population growth with both exclusive residences and nonindustrial growth (factors 1 and 3).

⁷ The method used here was developed by Ahmavaara and is available in the OSIRIS statistical package.

TABLE 6

DIFFERENCES IN FACTOR LOADINGS BETWEEN THE 1960 AND 1970 FACTOR STRUCTURES
FOR SANTA CLARA COUNTY SUBURBS

Variable	Factor 1	Factor 2	Factor 3	Sum of Squares
E/R ratio	-.53	.28	.36
Median household income	-.1313	.04
Median home value12	.02
% single-family residences	-.1904
Property tax base per capita42	-.3228
Municipal tax rate1503
Capital assets per capita2808
Expenditures per capita2518	.10
Sales tax income per capita1102
Population growth 1950-60	-.79	.40	-.37	.92
Ratio residential/employment growth 1960-63, 1970-731202

NOTE.—Includes only differences greater than .10; the 1960 structure has been rotated into the 1970 factor space for this comparison.

The emerging pattern of suburban *differentiation* represents also an increasingly inequitable social *stratification* (Advisory Commission 1969; Williams et al. 1965). This can be easily illustrated in the case of Santa Clara County suburbs. These suburbs have been classified according to their 1970 factor scores on the factors described above; table 7 compares the three types of cities in terms of their racial composition, proportion of low-income households, and financial characteristics of public school dis-

TABLE 7

LOW-INCOME AND MINORITY POPULATION, SCHOOL PROPERTY TAX BASE, SCHOOL TAX
RATE, AND SCHOOL EXPENDITURES PER STUDENT, BY CITY TYPE:
SANTA CLARA COUNTY, 1970

	CITY TYPE		
	Industrial Commercial	Exclusive Residential	Intensive Residential
Households with income less than \$6000 (%) ..	18	9	22
Minority households (%)	8	2	18
Elementary districts:			
Tax base per student (\$)	23,160	22,205	11,890
Expenditure per student (\$)	985	965	680
Tax per \$100 assessed valuation (\$)	3.55	3.45	3.70
Unified elementary high school districts:			
Tax base per student (\$)	19,190	...	10,440
Expenditure per student (\$)	1,305	...	8.45
Tax per \$100 assessed valuation (\$)	6.74	...	5.90

tricts.⁸ There is a clear segregation of minority and low-income residents into intensive residential cities, which have not only the highest municipal tax rates but also the poorest school tax base and the lowest school expenditures per student.

IV. CONCLUSION

The purpose of this article has been exploratory—to present some elements of a theoretical model of current suburban growth and to show that this model is consistent with the experience of at least one recently industrializing, rapidly growing suburban region. The main conclusion to be drawn is not that this particular model does or does not represent the growth process of American suburbs after 1950. To test the generality of the model and evaluate the relative importance of the variables which it emphasizes would require a longitudinal study of a broad range of suburban regions. But this study does suggest what *kind* of model might be required to explain suburban ecological patterns. Such a model should take into account the type of growth in the region as a whole, the interdependence among suburbs and the fact that growth in any community becomes a condition affecting growth in others, the importance of local government's land-use and budget decisions, the social composition and organization of each community, and the interests and influence of each social group upon local decision making.

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⁸ Data on school finances is from California Teachers Association, *California School District Financial Analyses, 1969-70*, research bulletin 253, December 1970. In cases where school district boundaries do not coincide with municipal boundaries, estimates for each city are weighted averages based upon enrollment in the elementary schools of different districts which are actually located in each municipality.

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Logan on Molotch and Molotch on Logan

NOTES ON THE GROWTH MACHINE—TOWARD A COMPARATIVE POLITICAL ECONOMY OF PLACE

For years the study of urban politics was burdened by the theoretically sterile debate over the degree of concentration of power in American cities. Researchers from both sides implicitly presumed that cities were relatively homogeneous in their pattern of politics, to the point that one analyst attributed variations in the results of case studies simply to methodological differences (Walton 1966). It was therefore an important breakthrough when Terry Clark (1968) initiated a program of comparative studies in which he took seriously the possibility of real political differences among communities.

From the functionalist perspective taken by Clark, the power structure is expected to be differentiated ("decentralized") according to the social differentiation of the urban community. Large, heterogeneous cities have less centralized power structures than small, homogeneous cities; urban growth is expected to create more pluralistic structures of power as new interests develop and are accommodated within the political system. Fundamental to this model (and to that of the pluralists with whom Clark's work is theoretically consistent) is the conception of power as influence over a series of loosely related discrete decisions in a variety of issue areas. Clark identified five such areas, ranging from mayoral elections to air pollution controls; Dahl (1961) studied three. "Decentralization" is operationally defined in terms of the number of types of interests which participate in decision making in these areas and the degree of overlap in influential persons and organizations among areas.

"The City as a Growth Machine" (Molotch 1976) challenges the premise and hence the conclusions of the functionalist model. Molotch asserts that "the political and economic essence of virtually any given locality, in the present American context, is *growth*," and therefore that "growth is not . . . merely one among a number of equally important concerns of political process" (pp. 309-10, 313). This same identification of one type of issue as the "big issue" of municipal politics can be found in Hunter (1954)—he also emphasized the issue of growth—and I propose that it is this conceptual step on which Hunter's interpretation of politics is based. From this position, the "decentralization" which Clark associates with growth is irrelevant to the essence of the urban political economy. Rather, growth is the result of the usurpation of political control by unrepresentative land-based local elites and is the source of their continuing coherence as a power bloc.

Many readers will contest Molotch's conception of what is at stake in urban politics. I believe that he is correct at least in the extent that the growth-related policies of a city affect its size, composition, fiscal resources and responsibilities, and relationship to other cities in the regional or national system. But his essay aims to move another step toward a theory of urban power structures: by identifying the substantive content of policy, it becomes possible to analyze the interests and coalitions in favor of and in opposition to growth and to examine sources of variation and change in the conflicts over growth policy. How does growth present itself as an issue in different communities? What are the interests represented in the growth machine and when does it break down? What are the social bases of the countercoalition and in what kinds of cities has it developed strength? My purpose here is to examine some elements of this emerging theory, to raise issues more than to settle them.

1. The Growth Coalition: Local and Extralocal Partners

Particularly important in Molotch's analysis of the local interests for and against growth is what he says about the working class and what he does not say about private corporations.

He argues (p. 325) that the working class is "politically passive (if not downright supportive)" with respect to growth policies because of a false belief that growth means jobs. It is true that the geographic distribution of growth has no necessary relationship to the number of jobs created by the national economy and that growing cities may have high rates of unemployment. There are nevertheless some types of jobs which depend directly on the growth machine: construction, transportation, government, and wholesale and retail trade all grow with the metropolis. Such employment growth may increase the job security of the existing work force, but in particular it increases the size and influence of the unions through which the latter is organized. Thus although growth may create public problems in the communities in which workers live, their unions are often strong advocates of regional development.

Molotch does not include the private corporation in the growth machine, but rather treats it as an outsider which, in its search for attractive location, is responsive to local government policies. I would argue that its role is considerably more active in several ways. First, once established in a community, the corporation (as a major property owner) has a substantial stake in local policy as this affects changes in the tax structure, service levels, and regulatory action—the same policies according to which it made its original decision to locate. Second, private corporations as well as unions and some other interests are organized and active in regional, state, and national politics to promote the preconditions of growth—not in any par-

ticular municipality but in the region or state generally. Such extralocal policies as investment tax credits or state-guaranteed loans for new industries, the federal assumption of welfare costs, and public investment in urban redevelopment, highways, mass transit, and sewers may critically affect the economics of growth. By its action on such issues, the corporation—in its effort to maximize the profitability of new investment or to guarantee old investments—is a senior partner in the growth machine but beyond the influence of local countercoalitions. One must question whether the political economy of growth can be studied as a strictly local process.

2. The Concept of Growth and the Conditions of Opposition

For the purposes of his essay, Molotch has defined growth as a unidimensional variable; his reference to growth as an “entire syndrome of associated events” (p. 310), encompasses both population and economic expansion. Studies of metropolitan growth, however, have found that these associated events are unequally distributed both between central city and suburb and among suburbs themselves. The concept of growth has to be elaborated to facilitate identification of the types or strategies of growth in different kinds of municipalities: the kinds of growth they have experienced and are trying to attract, the policies they may manipulate to achieve their aims, and their competitive positions in relation to one another. In my own research I have identified three types of suburbs emerging since the 1960s in which growth issues present themselves in quite different ways, just as the growth problems and interest coalitions are different in New York and Dallas, or Los Angeles and West Palm Beach.

The unidimensional conceptualization of growth clouds real differences in the nature of the oppositions to growth and the conditions under which the countercoalition develops. The point can be made with examples of antigrowth movements cited by Molotch:

a) The restriction of growth in Ramapo and Petaluma is difficult to distinguish from the exclusionary practices of “meccas” like Beverly Hills and Lake Forest, which attained high social status and a strong property-tax base through zoning and land-use controls. Paradoxically, many exclusive suburbs grew at a high rate in the 1950s; such suburbs were opposed not to growth itself but to a disruption of their social status by industry or lower-income, high-density housing. In such communities, “antigrowth” policies are open to criticism from the point of view of their effects on racial segregation and class stratification in the region.

b) The studies of the impact of growth in Palo Alto and Santa Barbara were concerned with population growth, and in the case of Palo Alto were closely associated with an environmental movement to preserve open lands in the foothills overlooking the city. Opposition to growth has not pre-

vented the continued expansion of employment which, it can be argued, provides a net fiscal benefit to these communities, helping to support excellent public services for those who can afford to live there. Expansion of employment in turn is the condition for population growth in other cities in the region.

c) Notably missing among cases of strong antigrowth activity are the working-class suburbs in which the bulk of population growth has occurred. Yet these are the communities in which growth problems are most severe—they are unable to attract the kinds of development favored by exclusive suburbs or employment centers, and so suffer the tax costs and public service burdens of extensive single-family subdivision development. We can only guess at the reasons for lack of organized opposition to the growth machine in such places: the absence of voluntary associations, the rapid turnover of population, the large proportion of renters, racial and ethnic cleavages, and the ambiguity and variability of people's perceptions of the causes of local problems. It is not clear that persons in these cities share the interests of or are benefited by the countercoalition in other areas.

The variety of contexts in which growth may be treated as the central political issue implies that it is misleading to analyze the growth machine and countercoalition in gross terms. We are certainly far from being able to extrapolate the national effects of a victory by the countercoalition. The challenge is to elaborate a *comparative* theory of the urban political economy, to reflect and to order this variety of American communities.

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VARIETIES OF GROWTH STRATEGY: SOME COMMENTS ON LOGAN

John Logan usefully draws attention to the fact that while it is indeed the case that urban development can be understood in terms of interlocal competition for land-use enhancement, the processes at work are not an undif-

ferentiated contest for growth. First there is the fact, as I also indicated in my article, that it is not quite the case that in all locations the members of the dominant land-use elite find growth the key method of enhancing their shared turf; there are and have always been small pockets of land where the rich make their homes. These are the large-lot, bucolic, strategic hamlets (e.g., Los Altos) to which the rich retreat with the money they make from the industrial and distributive processes which end up despoiling the environments in which the less economically powerful must dwell. While I believe these places are relatively rare, in the context of locality in general, it is indeed a real type, pointing to the validity of Logan's notion that there are different sorts of locality and hence different sorts of political economies guiding their development. Even so, it is remarkable how many of these pockets of privilege nonetheless succumb to other forms of development which decrease or actually eliminate their elite status and land-use pattern—when the need and the high-stake opportunity actually arise (e.g., Exxon in Santa Barbara; Pepsi in Purchase).

Also as Logan points out, among the localities which do have growth as their guiding reality—dominant in both number and significance, I would argue—there are variations in growth strategy dictated by opportunity. These growth strategies are hierarchical in nature, with localities having to settle for that growth strategy which has the potential to succeed. For example, one such strategy—quite a desirable one from certain standpoints—is to grow economically through industrial expansion but somehow induce the residents so attracted to locate elsewhere. This sort of strategy is relevant, of course, only when dealing with the small-unit locality; at the metropolitan or regional level of analysis, it is quite clear that any industrial growth must ordinarily be accompanied by the work force's taking up residence in the same area. But such fiscal arguments are ordinarily not constraining: virtually any sort of development, when it comes down to the wire, tends to be sold as fiscally beneficial on the argument that it will, without doubt, "add to the tax base." That it will also detract from the tax base through a demand for services tends to be ignored at such moments. In other words, growth of industry without people is nice work if you can get it; but if you can't, you take residents as well, or even the residents alone. The local growth coalitions are ordinarily sufficiently powerful to dictate such fiscally irresponsible programs.

Other hierarchical differences in growth strategy emerge in terms of the *types* of industry which are sought as well as the *types* of residents who are sought. These opportunities will be dictated by such factors as natural amenity, cultural milieu (e.g., historical tradition of elite uses, such as Beacon Hill or Palm Beach), or degree of access to those levers of public-investment decision making which can provide for the widest choice of development strategies. In general, I would argue, the most

sought-after kind of industry is the smokeless, research and development sort; the type of residents most desired are the rich, the highly educated, and the blonde.

The appearance of Logan's paper provides an opportunity to speculate on what may be other forms of differentiation in the means through which members of local elites act to enhance shared turf. As one example, tourist-oriented cities must closely guard their amenity-oriented growth strategy. As I implied in drawing attention to recent political developments in the high-amenity, university cities (Ann Arbor, Boulder, Santa Barbara, and Palo Alto—the latter noted also by Logan), the dominant local booster strategy has always been to achieve growth through the "clean" development of universities and a surrounding belt of Ph.D.-intensive R & D. Permitting just any sort of industry could in the end destroy that growth strategy and its constituent coalition, a strategy which was built on some sort of minimum amenity maintenance that could attract free floating industries for which classy labor supplies and pleasant working/living environments were important. Hence each growth coalition wages the best and most appropriate strategy it can: Gary, Indiana, is just not in a position to attract, say, General Electric Tempo's Santa Barbara-based research installation (however it and its employees may be fiscally or culturally preferred), while the Santa Barbara growth elite is mindful of the need to protect the city's current growth strategy and would hence resist a branch plant of, say, United States Steel on its sandy shores (however much it would add to the tax base).

Entire states may develop similarly differentiated strategies—Vermont must maintain a viable tourist and second-home economy; Ohio must adjust to a different reality. It was development-minded Ronald Reagan (even Ronald Reagan) who once implied he had some sensitivity to the fact that what he called "the magic of California" was the goose that was laying too many golden eggs to be sacrificed to totally unregulated industrial development.

Even at the level of the nation state, it can be speculated that the differentiation continues. The tropic island nations bent on rapid tourist development must restrict extremely noxious land uses; certain countries such as Sweden and Switzerland reach a high level of economic development (and consequent prosperity) by restricting the sorts of land use permitted and tactics of growth (e.g., belligerent militarism). Switzerland and Sweden are, in some sense, the Los Altos suburbs of the capitalist world.

Such observations wander far afield from Logan's topic of suburb, but in my view scale of locality (e.g., suburb or city) is not ultimately a proper subject for a field of study. Instead we need an integrated analysis of the meaning of territory and locality—at least parallel in breadth to the

scheme that the early human ecologists (e.g., Park and Burgess) set out to develop, but informed by the recognition of location as a means of "trapping" surplus value and speculative profit,¹ and the social organizational consequences of that overwhelming and critical function.

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¹For a particularly promising approach taken through the Marxist paradigm, see Harvey (1973, chaps. 5, 6).

The Anomie of Affluence: A Post-Mertonian Conception¹

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Merton's conceptualization of anomie theory is examined in terms of the influence of the economic and social conditions surrounding its initial formulation: circumstances of chronic depression. The anomie potentially generated by unanticipated affluence, a more central concern for Durkheim, is discussed by way of contrast. A tentative typology of deviant adaptation is constructed utilizing questions of (a) commitment to approved cultural goals and (b) the degree to which achievement of substantial progress toward such goals is realized. It is suggested that this typology might be particularly effective in the understanding of deviance at higher socioeconomic levels. The essay also considers implications of Durkheim's underlying model of the human.

And in too greatly desiring some specific thing, either in prayer or in the strivings of the ambitious, we forget the metaphorical quality of all desires. [KENNETH BURKE, *Towards a Better Life*]

With the possible exception of formulations by Park and Burgess of the dynamic processes and structure of urban places, no sociological conception has been more productive of theoretical discussion and empirical activity than have Merton's writings on anomie and social structure.² However,

¹ Over the several years during which this essay was developed, its content came to reflect a rich research experience. At various times the research was generously supported by grants from the National Institute for Mental Health, National Institute for Child Health and Human Development, and most recently by the Law Enforcement Assistance Agency. We also want to acknowledge gratefully the commentary and editorial suggestions provided by Professor Myron Simon of the University of California at Irvine and by Professor Rose L. Coser of the State University of New York at Stony Brook.

² It is often difficult to keep entirely clear which version of Merton's seminal essay is being cited. The Cole and Zuckerman (1964) citations do not clarify the multiple-edition problem, which is further complicated by Merton's tendency to make revisions and then either not change the title of the article or not note that new materials have been added. The first Merton paper was published in the *American Sociological Review* in 1938 (Merton 1938) and dealt primarily with different types of societies varying in their cultural emphasis on means and goals. The first revision (Merton 1949a, 1949b) was published in two separate places in the same year, but except for

as with the Park and Burgess model of the city, it is necessary to consider the degree to which Merton's paradigm and implicit approach suffer from historically and culturally circumscribed imagery. Since the first publication of these ideas occurred in the late 1930s—years of only partial recovery from a major depression, a recovery completed only by a major world war—it is not at all surprising that Merton assumed a general and differential scarcity of socially structured means for the achievement of widely and diversely accepted culturally defined goals to be a usual, and perhaps permanent, condition in capitalist societies.³

The society of scarcity which the senior generation of contemporary sociologists theorized about and which informed their daily lives was, for all but its most privileged members, constantly problematic. Not only was upward mobility a trial and a risk, but remaining stationary required relatively continuous application as well. This was a world in which economic prosperity could be conceived to be more disordering of and discontinuous with the "normal state of affairs" than was economic depression. Prosperity seemed to engender dangerous illusions, while depression only brought home forcefully the hard lessons of the Protestant Ethic: prosperity encouraged the profligate; depression served to validate the prudent.

Currently, for the United States and much of the Western world, sociology faces the problem of treating transformations of the social landscape that involve simultaneously both the anomie of scarcity and the anomie of affluence. The magnitude of the shift in the character of society be-

some variants in paragraphing and punctuation and plurals the two new versions are essentially the same. However, the subtitle in the volume edited by Ruth Anshen (Merton 1949b) indicates the revised nature of the essay even though the main title remains the same. This revision introduces the notion of adaptations to anomie and reduces the discussion of variant societal types. A second revision, under the original title, without indication that there have been substantial additions, appears in the revised edition of *Social Theory and Social Structure* (Merton 1957, pp. 131–60). In this volume there is a further paper on the topic which extends the arguments made in the second revision (Merton 1957, pp. 161–94). The remainder of Merton's additions to the discussion up to 1964 are adequately treated in Cole and Zuckerman (1964).

³ It is worth noting that the imagery of a societal organization with a permanent condition of scarce rewards and their differential allocation to select roles was central to the Davis-Moore theory of stratification, which is both intellectually and ideologically complementary to Merton's position (Davis and Moore 1945). Written close to the same time by members of the same generation of sociologists, these views share an inheritance from Durkheim, though the parentage is more direct in Davis and Moore as the following quotations from Durkheim's *Suicide* suggest: "At every moment in history there is a dim perception, in the moral consciousness of societies, of the respective values of different social services, the relative reward due to each, and the consequent degree of comfort appropriate on the average to workers in each occupation . . ." (1951, p. 249). "Under this pressure each in his sphere vaguely realizes the extreme limit set to his ambitions and aspires to nothing beyond. At least if he respects regulations and is docile to collective authority, that is, has a wholesome moral constitution, he feels that it is not well to ask more". (p. 250).

tween the period when the Mertonian position was formulated and the present is suggested in an illustration used by Merton in his 1957 addendum to the first revision of "Social Structure and Anomie" (p. 172). There Merton cites a question asked by Roper in 1947 as an indicator of the degree to which the achievement motive permeated, however differentially, the population of the United States. Some 53% of "the prosperous" and 31% of "the poor" then agreed with the item, "Do you think that a young man through ambition, hard work and thrift can rise in life, own his own home, and earn \$5,000 a year?" These rewards exemplified culturally approved goals on which there was powerful emphasis: goals which now appear in the mid-1970s to be too trivial, too commonplace to be linked to a serious definition of a demand for achievement that can organize the scarce resources of time, energy, and perhaps most important, passion. Far more than inflation is involved in the trivialization of these goals, for it is difficult to think of specific transformations of the various elements in the survey question that would carry the same weight they seemed to carry in the mid-1940s. Beyond a shift upward in scope—the simple magnification of desire or achievements—we suspect a shift in the very meaning of achievement for major segments of the society.

One characteristic of Merton's writing on anomie, even in its latest revision, is that without exception all of the existentially relevant language is related to a struggle for access to, utilization of, or commitment to means.⁴ The absence of a need to describe the experience of achievement

⁴It is important to point out that there is, in fact, only a limited intellectual continuity between the majority of the modern users of the concept of anomie and Durkheim. Both Parsons and Merton, who share responsibility for introducing the idea into American sociology, do severe violence to the integrity of the original ideas expressed by Durkheim by selectively abstracting elements from the original text. The original conception involved two separate analytic dimensions in the discussion of the four empirical cases (inflation, deflation, chronic change in the commercial society, marriage and divorce). The table below describes these relations and assigns the empirical cases to them. Parsons selectively interprets the commercial society case by saying that "anomie is precisely this extreme of limit of disorganization where the hold of the norms over individual conduct has broken down" (1937, p. 377). Merton argues that American society meets the conditions of Durkheim's commercial society and then discusses only the problem of goals-means emphasis. The introduction of the notion of accessibility to means and the structural balance between means and goals substitutes for Durkheim's image of the individual's experience of a legitimate relation between effort and achievement.

TWO DIMENSIONS OF ANOMIE: STRUCTURAL AND INDIVIDUAL

CHANGES IN INDIVIDUAL STATUS	IMBALANCES BETWEEN MEANS AND GOALS	
	Yes	No
Yes	Inflation/deflation	Marriage/divorce
No	Commercial society	Eunomia

as more than a fixed goal toward which one struggled is probably the most important continuity between Durkheim and Merton. For both of them, simple invocation of the idea of economic success as a modal goal was apparently self-sustaining. In a society that possessed an abundant experience and imagery of the consequences of the failure to achieve, it was unnecessary to describe the landscape on the far side of achievement in order to motivate behavior. For most people living in such a society it was enough not to have failed; whether one succeeded or merely kept even, the content of the experience of success was constantly protected. However, in contemporary society, what appears to be needed is precisely a specification of success as a state of being or a mode of existence. For at least some sectors of society the goals may no longer be offered in the most abstract of terms, as a blank in the social contract to be filled in at some later date and to the seller's liking. Moreover, the request for specification makes it more frequently possible to evaluate the socially approved goals in terms of the kinds of bargain they represent. Alongside the persons Merton has referred to as lower-class retreatists who abandon striving because the goals are too remote and the means inaccessible, one now finds middle-class retreatists who with access to and knowledge of the means nevertheless withdraw, having decided, at least for the moment, that they have not been made a very attractive offer.

THE STRATIGRAPHY OF ANOMIE

The rapidity of social and cultural changes over the last 25 years requires not only that sociologists attend to the problems of economic and social stratification (by analyzing the original social structures linked to the problems of poverty and affluence), but also that they examine the cross-cutting effects of cohorts who experience scarcity or affluence as a differing societal context for differing parts of the life cycle. Although the deprived poor and the middle classes who grew up and began their work lives in the climate of unpredictable economic conditions prior to World War II differed in their class-linked experiences, they shared a common fate in that scarcity and the fear of scarcity were generally shared by all but a few in the society. Persons of all class layers who grew to young adulthood during this period differed from those who spent their childhoods in the same period but whose work life has been lived in the relatively continuous affluence since 1950. In a sense the latter group was a critical generation in transition, with feet unsteadily in both a scarcity society and an affluent one. This unsteadiness may explain in part the generalized conformity that seemed so apparent to the social commentators of the 1950s (Riesman 1950; Whyte 1956), a conformity of consumption values based on a magical belief that if people just keep doing the same old things

economic prosperity will be insured. For the young who have grown up since World War II—especially those of middle-class and upper-middle-class status regardless of parental origins—the experience of a world that required unrelenting struggle and in which deprivation or potential deprivation was the norm must appear increasingly remote. The cautionary tales and character-building moralities which were associated with that world and which served to dramatize its successes and failures must be as abstract to many of the current generation as the existential experience of achievement was to their grandfathers. Further, there is evidence from an entire sequence of studies of the young—including the working class—that even a verbal adherence to these moralities has largely become a minority phenomenon (Simon, Gagnon, and Buff 1972).

Even within cohorts, however, experience is not uniform, as the values of a parental generation (often still committed to the imagery of a scarcity world) of one or another social class contest with the main drift of a society currently founded on the experience of easy consumption. In these circumstances the goals of the society do not represent some homogeneous set of desires for all sectors (either classes or cohorts), if indeed they ever did. Instead, there is a complex series of adjustments and tensions between commitment to and gratification from a varied set of societal rewards which are themselves under relatively frequent review, in terms of both personal salience and objective valuation.

Clearly, not all members of the society currently have anything resembling equal access to the means to success. Indeed, there may be a widening gap between the worlds of the anomie of affluence and the anomie of scarcity, with diminished capacities for leaps across that gap in either direction: dramatic success may be as rare as dramatic failure, with each requiring equally substantial effort. The very coexistence of affluence and scarcity or deprivation tends to alter both forms as we have historically known them; with the latter being somewhat more profoundly influenced by the former. The experience of affluence on a societal scale has created a novel set of conditions under which differing classes and cohorts can judge both the opportunities to achieve and the experience of achievement itself. The rhetoric that once justified the affluent and instructed the poor ("hard work, thrift, and ambition") may be increasingly inappropriate for both.

The Mertonian reformulation of Durkheim has been accurately defined as a frustration theory of deviant adaptations. In essence, for Merton, anomie is most likely to be engendered when society generally encourages commitment to goals but makes the means of legitimately achieving these goals only differentially available. Commitment to the goals is assumed; it is the means that are problematic. Indeed, Merton's citation of the Roper question and the differential social class responses to it was intended

to demonstrate the general acceptance of the achievement orientation. Similarly, a concern for the anomie of affluence, as for the Mertonian anomie of scarcity, can be said to involve a frustration theory of the generation of deviant adaptations. The difference between the two conceptions and the conditions they are responsive to, however, lies in a reversal of the emphasis between goals and means. Since effective means for social achievement are now generally available to sectors within *certain* classes and cohorts in the population, it is the commitment to and the gratification to be derived from the achievement that become problematic. In contrast to a social order that makes promises and fails to keep them, we must now consider a social order that for many segments of the society makes promises and then proceeds to keep them. But a society that keeps these promises with such ease and abundance can trivialize them to the point where achievement no longer affords what has been called "consummatory gratification." To which we would add that it also no longer affords constraining gratification—the condition in which gratification as a process further ties the individual to the prevailing moral order instead of weakening such ties.

The differences between generations or cohorts are a significant concern in the examination of the differential basis for commitment or noncommitment to the major goals of the society. For a cohort now over 40 (and here it must be noted that working- and middle-class ideologies were perhaps more similar before World War II than they are today) who were socialized in an era when widespread access to societal goals was largely unanticipated and for whom much of the value placed on achievement and the symbols of achievement was predicated on their very costs, or their relative scarcity, absence of rewards was experienced as a compelling factor by both the deviant and the conforming. The sense of Merton's innovative adaptation suggests a willingness to engage in both heightened effort and risk taking in the name of goal attainment. Similarly, for the conforming, a largely unexamined population, the failure to realize dramatic achievement frequently required complicated ideological shifts: the invention of second and third best. The social world in which both conformity and deviance could be described in terms of a possible anomie of scarcity assumed that relative progress toward the realization of conventional goals served to confirm competence, moral worth, and/or good fortune. Attainment of goals ceases to provide such confirmation precisely when the objects or experiences that have symbolized achievement become part of the easily accessible and therefore unspectacular, everyday quality of life that characterizes, as it were, "the anomie of affluence."

Parts of a generation, those raised in periods of relatively easy access to the means of success, with early and often "unearned" access to what were heretofore exclusively adult pleasures, may fall outside the Durkheimian conceptualization of anomie. There is, to be sure, a limited analogy

to the temporary dislocations of prosperity when changes in one's status make rewards available without a proportionate increase in effort. This is not, however, a matter of having goals escalate in the face of achievement (as in the instance of commercial society), but a matter of goals ceasing to be experienced as overdetermined in their meaning, indeed ceasing to be experienced as rewarding.

COLLECTIVE METAPHORS AND PERSONAL METAPHORS

An understanding of the difference suggested above requires that we avoid any excessive simplification of Durkheim that would translate anomie merely into a state of normlessness or deregulation. We come closer to the Durkheimian meaning when we define anomie as that condition in which social metaphors lose their ability to organize personal metaphors: the problem of the link between social facts and personal motivation and judgment.

Durkheim's almost synonymous use of the terms "individualization" and "anomie" suggests that his concern is the ascendancy of the existential over the abstract. Surely for Durkheim, and perhaps for Merton, the stability of social life and the possibility of retaining social integration depended on relatively long-term value internalization and the linkage of elements of the individual life-style to the collective meanings of the society. As we have noted before, in this situation scarcity simply brings home to individuals of the Western world the conventional verities of prudence and self-sacrifice. In contrast, long-term prosperity appears to be a greater threat to social integration since it emphasizes and allows immediate material gratification and fails to punish individuals for acting out their deepest desires. There is in Durkheim, and more faintly in Merton, an implicit psychology that sees the individual's economic and sexual lusts (often indistinguishable, with the latter serving as the metaphor for the former) as barely contained by the attachment to collective needs and purposes. Durkheim expresses this uneasiness in the following terms:

Such is the source of the excitement predominating in this part of society [the commercial sector], and which has thence extended to other parts. There, the state of crisis and anomy is constant and, so to speak, normal. From top to bottom of the ladder, greed is aroused without knowing where to find ultimate foothold. Nothing can calm it, since its goal is far beyond all it can attain. Reality seems valueless by comparison with the dreams of fevered imagination; reality is, therefore, abandoned, but so too is possibility abandoned when it in turn becomes reality. A thirst arises for novelties, unfamiliar pleasures, nameless sensations, all of which lose their savor once known. Henceforth, one has no strength to endure the least reverse. The whole fever subsides and the sterility of all the

tumult is apparent, and it is seen that all these new sensations in their infinite quantity cannot form a solid foundation of happiness to support one during days of trial. The wise man, knowing how to enjoy achieved results without having constantly to replace them with others, finds in them an attachment to life in the hour of difficulty. *But the man who has always pinned all his hopes on the future and lived with his eyes fixed upon it has nothing in the past as a comfort against the present's afflictions, for the past was nothing to him but a series of hastily experienced stages. What blinded him to himself was his expectation always to find further on the happiness he had so far missed. Now he is stopped in his tracks; from now on nothing remains behind or ahead of him to fix his gaze upon. Weariness alone, moreover, is enough to bring disillusionment, for he cannot in the end escape the futility of an endless pursuit.* [Durkheim 1951, p. 256; our emphasis]

For the student of the history of ideas, there is an important connection to be noted in Durkheim's work between erotic luxury and economic excess. This connection was frequently made in a France suffering from a profound reaction to the economic boom and bust of the Second Empire. In his novel *The Kill* (*La Curée*, published first in 1871) Zola provided this account of the inflation in its earlier and then later stages:

Men's enervated minds turned towards pleasure and speculation. Those who had money brought it forth from its hiding-place, and those who had none sought for forgotten treasures in every nook and cranny. And underneath the turmoil there ran a subdued quiver, a nascent sound of five-franc pieces, of women's rippling laughter, and the yet faint clatter of plate and murmur of kisses. In the midst of the great silence, the absolute peace of the new reign of order, arose every kind of attractive rumour, of golden and voluptuous promise. . . . From the very beginning Aristide Saccard felt the advent of this rising tide of speculation, whose spume was in the end to cover the whole of Paris. He watched its progress with profound attention. He found himself in the very midst of the hot rain of crown-pieces that fell thickly on to the city's roofs. [1963, p. 56] Meanwhile the fortune of the Saccards seemed to be at its zenith. It blazed in the midst of Paris like a colossal bonfire. This was the moment when the eager division of the hounds' fee filled a corner of the forest with the yelping of the pack, the cracking of whips, the flaring of torches. The appetites let loose were satisfied at last, in the shamelessness of triumph, amid the sound of crumbling districts and fortunes built up in six months. The town became a sheer orgy of gold and women. Vice, coming from ahigh, flowed through the gutters, spread out over the ornamental waters, shot up in the fountains of the public gardens to fall down again upon the roofs in a fine, penetrating rain. And at nighttime, when one crossed the bridges, it seemed as though the Seine drew along with it, through the sleeping city, the refuse of the town, crumbs fallen from the tables, bows of lace left on couches, false hair forgotten in cabs, banknotes slipped out of bodices, all that the brutality of desire and the immediate satisfaction of an instinct flung into the street bruised and sullied. Then, amid the feverish sleep of Paris, and even better than during its breathless quest in broad daylight, one felt the unsettling of

the brain, the golden and voluptuous nightmare of a city madly enamoured of its gold and its flesh. [1963, p. 120]

Durkheim's debt to this overheated and enriched literary style is evident in this passage dealing with sexual anomie:

The lot of the unmarried man is different. As he has the right to form attachment wherever inclination leads him, he aspires to everything and is satisfied with nothing. This morbid desire for the infinite which everywhere accompanies anomy may as readily assail this as any other part of our consciousness; it very often assumes a sexual form which was described by Musset. When one is no longer checked, one becomes unable to check one's self. Beyond experienced pleasures one senses and desires others; if one happens almost to have exhausted the range of what is possible, one dreams of the impossible; one thirsts for the nonexistent. How can the feelings not be exacerbated by such unending pursuit? For them to reach that state, one need not even have infinitely multiplied the experiences of love and lived the life of a Don Juan. The humdrum existence of the ordinary bachelor suffices. New hopes constantly awake, only to be deceived, leaving a trail of weariness and disillusionment behind them. How can desire, then, become fixed, being uncertain that it can retain what it attracts; for the anomy is twofold. Just as the person makes no definitive gift of himself, he has definitive title to nothing. The uncertainty of the future plus his own indeterminateness, therefore, condemns him to constant change. [1951, p. 271]

The sociologists of the "anomie of scarcity" saw continued collective prosperity as a particularly dangerous turn of fortune; and it is most dangerous for those for whom such prosperity becomes the occasion for substantial upward mobility. It is by these that success was seen only as an abstract goal and rarely as a potentially existential experience. In conditions of eased achievement, conformist success could be viewed as a greater risk than either conformist failure or innovative success or failure. In the case of conformist success (overachievement as a result of structural change) the quality of the rewards could in fact be tested; in the other cases individuals could take refuge in compensatory moral affiliation (the poor but honest) or in rewards that were tainted by the means of achieving them (the rich but undeserving).

It is under the current conditions of affluence that it becomes clear why the far side of achievement was so infrequently described, for to have done so under the conditions of scarcity would have been to demystify whatever concreteness the rhetoric of achievement could communicate. To be sure, success was clearly identified with the ownership of objects; and either through them or in promised interpersonal relationships there was access to a special experience; but these promises of objects and transcendental experiences were commonly representative of something that they themselves could not be.

Involved in this focus upon objects is a social-psychological "fetishism

of commodities": objects and experiences are not only what they are; they are also the justification of, compensation for, and consolation for what in many cases were long years of self-denial, compulsive prudence, and joyless labor. Even if the objects and experiences that were to be the rewards of husbanding and denial failed to serve these purposes intrinsically, the context of scarcity and the attendant continuation of mystery generated a mixture of admiration and envy on the part of non- or lesser achievers that afforded a sense of consummatory gratification. The tension between relative gratification and relative deprivation became the key to the significance of success. However, as affluence has grown more general and the significance of the ownership of objects has declined, the demystification of success has grown more general; and an older admiration and envy of success has given way to a more approximate peership of ownership. Hence, the anger expressed by the deserving poor and the affluent toward the ownership of expensive automobiles by the undeserving poor (who spent their money on luxuries instead of scrimping and saving) was only the first stage of democratization of ownership, giving the first inklings of the current hostility generated by the obscenities of unearned pleasure (Slater 1970).

The revaluation currently under way of the objects and experiences that at one time marked achievement has its origins in more than mere economic prosperity. They are to be found, as John McHale has observed, in the complex technological, scientific, and social transformations that have altered the value of objects and of people's fundamental relationship to them: "Since some time after World War II, a quiet, but profound revolution has occurred. For the first time in the history of man on this planet, we began to make men who lasted longer than things" (McHale and McHale 1975). Prior to this, man-made objects—particularly those that were most directly joined to the maintenance or validation of status, those connected to the collective metaphors of social life—endured longer than their makers or owners. Objects that symbolized a collective experience (e.g., churches, city halls, and hospitals), communities, and children predictably outlived the older generation. Indeed, one of the hallmarks of the affluent society, the generation gap, is perhaps only the experience of transition between the society of scarcity and the society of affluence. The affluent society does not so much create a generation gap as erode generation differences and distinctions in the possession of valued things and pleasurable experiences. Much earlier in the life cycle and for a far longer period, parents now share with children common and often identical lifestyles, activities, and status. If there is a gap, it is historically specific between those for whom affluence was the unexpected outcome (those who most resemble Durkheim's version of anomie) and those for whom affluence is the natural order of things (whose anomie may not be so much an adap-

tation to change as a permanent characterological condition which is a prototypical form of postindustrial normality).

THE ASCENDANCY OF PERSONAL METAPHORS

Advanced industrial societies are increasingly marked by the ascendancy of personal metaphors in contrast to collective ones as the organizing pivots of conduct. This movement is best exemplified in the changing value given to constructed environments. It is clear that in the past buildings expressed collective purposes more significant than the people who used them. The Parthenon has outlasted all the Greeks, and the Gothic church was a more important manifestation of the Middle Ages than those who worshiped in it. No such buildings can be constructed today; and while the sameness of buildings that crowd the main streets of midtown Manhattan may be deplored on a number of levels, they do what they were intended to do. The fact that all of the offices within them look alike is only partially symptomatic of individual conformity and suggests as well that the functions served in the offices of the psychiatrist, the professor, the foundation executive, or the magazine publisher do not depend on place. The persons who use the services of these individuals do not need to know them except as both persons in any pair fall into the reciprocal roles of sufferer-healer, learner-teacher, supplicant-Medici, author-editor. There is no need that other relationships beyond those intrude into the short-term connections suiting the purposes of both.

The problem is one that Kenneth Burke would have described in terms of the scene-act ratio (Burke 1969). In the past, architecture served the purpose of setting the stage of human performances; and in every basic sense the stage was more important than the actors. The ratio was heavily weighted toward the value of the scene rather than the value of the actor; indeed, the scene defined the act. In a sense, architecture in the past was a concrete example of the pathetic fallacy shaping the activities of men. Now the ratio swings the other way; the act becomes salient, and there occurs the final recognition that it is the nature of the human relationship that controls the scene and that the moments of despair and ecstasy are defined, not by the scene, but by the act.

Such environments are dominated by the existential, the transient, and the accidental, and increasingly they constrain people to test concretely their most abstract fantasies; for many, particularly those raised with older expectations about the quality of achievement, this testing dissolves into a kind of bitter cotton candy. Crucial to the values of an achieving society was the mechanism of delayed gratification, which involved not solely the postponement of gratification but the existence of an ordered set of gratifications so that postponement itself became an investment

which was to yield greater, longer-lasting, or higher-quality pleasure. In modern environments where the objects that stood for this pleasure are cheapened by nearly universal consumption and the status manifestations of ownership success have lost much of their public character, the other side of the peak of achievements is often a precipice.

Durkheim's image of persons suffering the necessity—a necessity provoked by opportunity—to test their fantasies (and, in consequence, the fantasies of the prevailing social order) is that of persons consuming themselves in an orgy of escalating appetites. This is an image of the human whose very commitment to gratification is psychologically overdetermined and perhaps even possesses instinctual roots. The wisdom of knowing "how to enjoy achieved results" depends on adequate anticipation of real experiences: experiences that are not inflated beyond the possible. But as Freud—with whom Durkheim shares a version of human nature—observed in a sexual context: waiting, anticipation, denial, anxiety, and guilt often amplify fantasy to levels far beyond those that reality can meet (Freud 1949).

The impulse to achievement and acquisition independent of use clearly describes persons socialized in an era of scarcity, an era when life was lived under conditions of enormous risk, but when such risk (if one includes the dangers of disease and natural disaster) was more nearly shared by all members of the society.⁵ However, this description is far less satisfactory

⁵ In a scarcity society, patterns of individual and social survival depended on the assurances of social stability and the existence of an ordered society. With the emergence of a commercial society with its disordering impact on social life, both the family and ideology became alternative but crucial sources of social integration. In scarcity societies, the family remains the unit in which, through fictionalizing the fact of reproduction, individual immortality can be assured. Participation in family relationships gives the individual a sense of sharing a past, having the potential for a future, and through the drama of reproduction sharing in the collective experience of societal and species survival. In a world of risk and danger, which are steadily increased by the penetration of new economic forms into daily life as well as the impact of new rewards contingent on achievement, the family becomes a central place for mutual help, commiseration, and solidarity. Ideological responses to the dilemmas of scarcity in 19th-century capitalism also abound, since they have the power to order action and to make the world coherent and understandable. Ideological commitments, like commitment to the family, give the individual a way of symbolically participating in the past and the future by possessing an ordered version of the social world around her or him. Ideology makes sense of the world, and political ideologies operate to sustain action and confirm a sense of virtue or a sense of failure, to explain victory and to repair defeat. In an extremely chancy world, ideological responses serve as either an alternative to or a complement of the sources of solidarity found in the family. As we move into a society marked increasingly by affluence, both the family and ideology are reduced in significance as primary ordering experiences for the society. There is a movement from family to peer cultures, as transitory relationships are maximized to serve both increasing role complexity and the fact that persons grow at very different rates. If none of the peer relationships have the permanence, stability, and level of affective involvement that community- and family-based relationships once had, the loss is made up for by the complexity that a world of lower intensity and briefer duration of affect allows. A similar problem exists for the main-

if we begin to examine the behavior of younger cohorts, especially those who have participated most fully through recent socialization and accident of location in the context of affluence. While continued affluence has been troubling for their parents, the young growing up in the middle class and especially the most influential portion of that class, its upper levels, have encouraged an acceptance of affluence as an ordinary state of affairs. This situation of relatively easy access to consumption (and of access to adult pleasures before they have been earned) seems to have produced a relatively cooler attitude toward the possession of objects and the experience of achievement on the part of the affluent young.⁶ For them both objects and experience that were the rewards of a scarcity society may no longer be endowed with a capacity to disappoint, frustrate, or motivate because their promise of gratification and significant reward is not taken as seriously. However, unlike the response of Durkheim's "wise achievers," who may have been those fully socialized to lives of achievement, whose prudence was conditioned by a surrounding landscape of nonachievement, failure, and frustration, and whose middle-range response to success reinforced their attachment to the prevailing social order and its value system, the unexcited response of the children of affluence need not have the same binding effect. On the contrary, this new potential for an unexcited response may disrupt the attachment to the ruling social order as much as the middle-range response strengthened it, but the weakening of attachment may be expressed in markedly unanticipated ways.

This weakening of attachment does not necessarily totally free the individual to invent the values through which experience is evaluated. Still less does it allow for a more genuine expression of some ill-defined original nature or biological mandate, though these are frequently offered as the ideological basis for new values. A basic reliance on institutionalized value systems, though in greater variety and requiring fewer universal assumptions, as well as a reflexive assimilation of the responses of others, though with possibly less permanent significance, is as characteristic of those subject to the anomie of affluence as it is of those describable in terms of the anomie of scarcity. The basic sources of both experience and its meaning

tenance of ideology when commitment to it precludes access to new experience and change by the insistence on doctrinal purity and correct associations. Ideologies erode in the face of economic plenty.

⁶ One might note that it was not the absence of violence at the famed Woodstock festival that was most impressive; after all, the Indianapolis 500 auto race can make the same claim with regard to large numbers of adult working-class participants, ones with a strong commitment to their own forms of drug abuse, mostly alcohol based. Rather, it was the casual abandonment of new cars on the side of the road by the young people who went to this concert (and many others). It is not that they do not want to consume; it is just that all of the articles of consumption can be replaced and are expected to be replaced.

remain the very matrix of social life; the comparative remains intrinsic to the human evaluation of the human. What changes is the content, the generality, and the degree of assumed permanence informing the implicit comparison. A shift from social metaphors, largely involving the confirmation of status, to personal metaphors, largely involving the confirmation of experience, does not lessen social determination so much as it increases complexity and variety. An example of this shift is the change of emphasis in advertisements for a reasonably high-prestige automobile, from the symbolization of formal status ("If you've got it, flaunt it") to the symbolization of access to experience ("For the free spirit in nearly all of us").

ADAPTATIONS TO THE ANOMIE OF AFFLUENCE

Now let us turn to a more specific examination of some of the possible implications and consequences of the anomie of affluence and, in particular, the types of adaptations that might follow. Basic to a notion of the anomie of affluence is easy access—indeed, overly easy access—to institutionalized means of achieving the major goals offered by society. In such a situation, the problematic nature of the means that plays such a crucial part in Merton's formulation tends to lose much of its significance. For a larger segment of the society than ever before, commitment to societal goals implies almost automatic access to legitimate means. Commitment to goals, which is essentially a question of values, then emerges as problematic. Further, insofar as the fact of commitment is associated with a high probability of goal attainment—implied in the notion of easy access to legitimate and efficient means—a second dimension becomes problematic, the ability of goal achievement to generate adequate levels of gratification. Here, then, we have two distinct dimensions: commitment and gratification. Is the individual committed to the legitimate or institutionalized goals of the society? Is the achievement (or assumed achievement) of these goals experienced as gratifying?

Moreover, to the degree that these two dimensions can be conceived as being independent of one another, it becomes possible to create a typology of adaptations along the lines of Merton's (see table 1). It is important to note that almost all the persons falling into our scheme have access to the means of achievement offered by the society. In contrast to the usual tradition of deviance research that focused on the nonconforming outsiders, the proposed typology focuses on those who have secured most of the benefits of the current social order. Under the changed conditions of affluence, those who have automatically been thought to be conforming (in Merton's plus-plus cell) are susceptible to deviance. It is the largely neglected and largely unexplained deviance of those in the higher economic and education strata that is addressed in this typology. At this moment it

TABLE 1
 TYPOLOGY OF ADAPTIVE RESPONSES

Type	Commitment to Goals	Gratification by Goal Achievement
1. Optimal conformist	Plus	Plus
2. Detached conformist	Minus	Plus
3. Compulsive achiever	Plus	Minus
4. Conforming deviant	Plus	Innovative
5. Detached person	Minus	Minus
6. Escapist	Minus	Innovative
7. Conventional reformer	Innovative	Plus
8. Missionary	Innovative	Minus
9. Total rebel	Innovative	Innovative

is not possible to identify the numbers or proportions of persons belonging to the specific types nor is it possible to identify the types that are increasing or decreasing in significance. What is offered is an imagery (again like Merton's) for research.

The first type needs little by way of elaboration. "Optimal conformism" should describe those individuals most thoroughly attached to the society: those for whom the society both appears to be working and is experienced as working. They are committed to its major goals and experience their realization of them as gratifying. For such persons there is little pressure to engage in norm-violating behavior outside the pressure that derives from highly individualized aspects of autobiography. To a considerable degree, the simplest measure of the viability of an affluent society would be the proportion of a population falling into this category. As a commitment to goals and gratification from goals should be the expected outcome of the socialization process, such commitments should be pursued as "the natural way of doing things"; by the same token, moreover, this should produce an absence of self-consciousness with reference to the commitments. Most individuals with deviant commitments, representing unanticipated outcomes of the socialization process, should be more self-conscious about their commitments; and self-consciousness, we can assume, is translated into salience. The degree of salience that commitments hold for conformists should be directly related to their ability to resist the emergence of counterinstitutions. Hence, by way of example, there is the tendency for the upwardly mobile to be more conservative in political ideology than those of continuing high SES positions.

Persons of the second type—defined by us as detached conformists—find the sources of gratifications associated with achievement adequate but are not particularly committed to the goals. They engage in achievement-oriented behavior without passion or enthusiasm but rather with a kind of

bland acceptance. Illustrative of this type are those described by Whyte as accepting a "social ethic" as against the "work ethic" (Whyte 1956). While not constrained to move in deviant directions, this type should be relatively low in resistance to modification of the basic institutions or institutional arrangements of the society as long as such changes do not imply a corresponding change in the quality or meaning of gratification.

Often lost in the presentation of typologies is a sense of the variability potentially contained within each type. Even less frequent is a consideration of the possibilities of what might be called cross-type mobility. For example, there are those who might be included among the optimal conformists, who find the rewards of achievement essentially gratifying but are not strongly committed to the usual goals. Their adaptation may involve a nondeviant commitment to minor goals included in the primary goal structure of the society and valued more highly by such persons than by most others, or it may involve finding a substitute commitment that is compatible with the goal structure of the society. An example of this is to be found among those who apparently reject conventional achievement in favor of careers in the service areas. The cliché about "those who can't teach" is suggestive of just such an alternative adaptation.

The third type represents something of a mirror image of Merton's ritualist. Totally committed to achievement but lacking a capacity for experiencing congruent gratifications, this type must achieve compulsively. Certifications or rewards, it should be observed, serve not only to encourage achieving behavior but also to constrain excessive achievement. Achievement does not occur entirely within existing social structures; it also causes changes in the size, scope, or character of social organization. The rewards of achievement, if experienced as gratifying, become foci of activity and prevent individuals from having constantly to escalate levels of achievement; the life of "making it" is more often than not transformed into the life of "having made it." Some, however, have little capacity for experiencing the gratifications of achievement outside the process of achievement itself. This incapacity should be particularly evident in many of those for whom achievement was particularly "overdetermined" and for whom its correlates were drawn initially in extremely abstract terms. Resembling Durkheim's original description of the pathological anomie of the commercial society, this type is possessed of an unquenchable thirst—the thirst for achievements. It is with this type that we encounter our first instance of a constraint toward marked deviance and/or innovation. Almost logically, if the only appeasement of the thirst created by past achievements is further achievement, this type must be driven either to failure or to straining against the limits set by the shape and character of existing organizations as well as against the transactional norms established by modal

experiences that assume less passionate pursuits of achievement. Both *A Christmas Carol* and *Citizen Kane* represent cautionary tales warning of just such behavior.

Our conforming deviant—the fourth type—comes still closer to Durkheim's original description. Having acquired the means of gratification, such persons must explore the dimensions of pleasure in search of modes of gratification; given the overdetermined character of their pursuit of the unreachable, their quest for new experiences, of which Durkheim spoke, begins to consume them. Reinforcing this quest for the extraordinary, which by definition should bring them quickly to the margins of deviance, is the fact, neglected by Durkheim, that one of the frequent rewards of achievement is an immunity to many of the sanctions that constrain and punish the less successful. Wealth insures a protective primary group as well as differentially protective societal responses. While the search for new gratifications generates individual distress, little need follow that is immediately threatening to the integrity of the surrounding society. A primary focus upon self-gratification is likely to become a more frequent characteristic of a social system marked by easy access to the means of achievement. Possibly this is an aspect of the increasing emphasis placed upon "positive" and "creative" uses of leisure. A powerful imagery of life on this level might also serve to encourage movement into the category of "detached conformists," which in turn may siphon off the pressures on opportunity structures by motivating individuals to abandon their commitments when they reach the middle range of achievement.

A fifth type of adaptation—detachment—involves a low intensity or unpassionate rejection of both the values of achievement and the gratifications attending it. Persons in this category come closest to resembling Merton's "retreatists." However, the very conditions of affluence may make it possible that this quality of detachment need not engender a commitment to either personal or social deviance. Raymond Williams suggests something of this type in his discussion of the "vagrant," "exile," and "stranger" (Williams 1961). Given a relatively affluent society, it is possible to "drift" without being particularly attached to that society. We suspect that for most individuals adopting it this should be an essentially transitional adaptation. Though entry may occur at other points in the life cycle or may last well past youth, this vagrant status should characterize a large number of middle- and upper-middle-class youths who might be mistakenly identified as being in a more markedly deviant category. While the direction of movement out of this type, which cannot be easily predicted, need not automatically be assumed to involve an increase in deviance, such persons may constitute a reservoir of the unattached that is an indicator of the state of the social order at a particular point, as well

as a critical population during moments of particular strain within the society.

The sixth type—the escapist—who rejects the values of achievement and, unlike the detached vagrant, accepts innovative and possibly deviant styles of gratification, also resembles Merton's description of the "retreatist." A crucial difference, however, derives from the context of affluence and the higher social class origins of the populations to which we are addressing ourselves. Merton's retreatist category took on its greatest reality when focused upon the hopelessness of the already deprived. The very condition and prior training of our affluent escapist or, perhaps more accurately, escapee from affluence make it possible for him to seek escape in ways that are less self-destructive or less heavily sanctioned than those we associate with lower-class retreatism. Typical of this adaptation is a considerable part of the continuing drug subcultures of middle- and upper-middle-class youth (Simon and Gagnon 1968). Clearly, here was a case of rejection of achievement norms combined with a commitment to seeking new modes of gratification. The very style of this rejection and the relatively complex intellectual equipment employable in the quest for the innovative in modes of gratification bespeak an affluent background, affluent both economically and culturally. An important contribution made by members of this group is the enrichment of an iconography of pleasure required by a society troubled by larger demands for gratification.

The seventh type—the conventional reformer—combines a desire to replace the major goals of the society with an acceptance of the qualities of life that constitute the content and style of the successful life. This type tends to give rise to the "loyal opposition," whose capacity to pursue radically the changes they desire in societal goals is basically limited by their shared commitment, which tends to have a more immediate existential aspect. In effect, they accept conventional images of the "good life," but would alter the routes to its achievement in order to make it more universally available.

The eighth type—the missionary—resembles the preceding one very closely, particularly in its greater commitment to seeking radical change. However, the absence of a sense of the human experience associated with success within the new order might be expected to produce a wider set of political styles. Moreover, given the abstract quality of this definition of goals, with success the missionary might also run the risk of coming to resemble the compulsive achiever, sharing the inability to translate success into an existential reality. Such an adaptation might commonly involve the not unfamiliar concept of self as an "instrument of history"; a personal, yet depersonalized political response that must seek to transform "the great march" born of strategic necessity into a permanent social and political system.

The ninth and last type is the total rebel. Persons in this category desire to replace both the goals of the social order and the quality of life associated with success within that order. While this double disloyalty to the prevailing order probably encourages a greater radicalism in the pursuit of new goals, the definition of the quality of gratification in the future order will serve to limit the range and quality of political response. Rather than focus on new political goals, such rebels may seek to transform politics into an attractive life-style, developing in turn a commitment to confrontational politics or the politics of immediate and dramatic personal experiences.

MODELS OF THE HUMAN

In a very early section of the essay on "Continuities in the Theory of Social Structure and Anomie," Merton appropriately describes his interpretation of Durkheim's use of the concept of anomie as follows: "... this concept referred to a property of social and cultural structure, not to a property of individuals confronting that structure" (1957, p. 161). However, a sense of the individual organism and its needs remains critical to Durkheim's vision of the origins and meanings of social and cultural structure; indeed, assumptions about the organism's "native drives" as a trans-cultural constant are a necessary prerequisite to understanding of the entire Durkheimian position. From worlds described by the simplest models of "mechanical solidarity" to the most complex forms of "organic solidarity," the human—in terms of needs and capacities—remains a relatively unchanging thread.

The Durkheimian model of the human is essentially consistent with the tradition that runs from Hobbes to Freud: the human, unregulated by the hard "facts" of social life, becomes the instrument (or victim) of overwhelming passions that in the absence of imposed constraints will exhaust the host society-organism before individuals themselves are exhausted. As a result, high anomie—as a characterization of some forms of collective life and its effects upon given members of such collectivities—becomes almost by definition a temporary condition which requires *either* some subsequent reordering and revitalizing constraints *or* societal and individual decay. An analogous position may be seen in the frequent slogan of the left: Socialism or Barbarism. Given the alienation generated by capitalism, the two alternatives are seen as the only options.

The basis for this type of position is elaborated rather extensively by Freud. It finds its sources in the experiences assumed to occur in the pre-social phases of infant experience. From this perspective, infancy ontologically prepares or patterns us for the anomie of deprivation as the unmediated expression of infant appetites decrees their ultimate frustra-

tion. The subsequent experiences of the character-forming family drama are seen as the development of affect-laden commitments that make possible a renunciation of the direct expression of instinctive life in favor of the controlled, self-disciplined styles required by collective life. The initial acts of renunciation, however, are anything but final; barely satisfied by disguised or channelized expression, the instinctual appetites exert a constant pressure upon the psychosocial contract. From this perspective, historical moments of high anomie represent lapses in the psychosocial contract.

The appropriateness of fit between Durkheim and Freud per se is not so much the major question as is the validity of the underlying model. This model of the human, it should be noted, tends to inform the critical assumptions of much social science theorizing; it accounts for a kind of conservatism that transcends the conventional distinctions between right and left. It is possible that, given the perpetually truncated nature of the historical record, social scientists have confused the outcome of a general sociocultural commonality—deprivation as a constant or near constant threat—with some intrinsic and fixed character of the human organism. In effect, they engage in what might be termed “sociomorphic” thinking: *imputing to the nature of the organism something that is actually a function of social life* (Simon 1974). As we have suggested above, the essential problem of the relationship between reward as a social event and gratification as a psychological response centers on the performance expectations associated with legitimate access to rewards. It is the latter component, performance expectations, that inflates the value of rewards and that, under different conditions of attainment, either enriches or threatens gratification. In social worlds that were predicated upon “pay now and go later” schemes, changing circumstances alter, not the rewards, but their capacity to gratify by affecting their overall price. The demoralization resulting from changed conditions of access structures the individual pathological moment.

The larger part of the human record is a chronicle of capricious natural forces and relatively chaotic societies. The ascendancy of social or collective metaphors (i.e., the legitimation of performance expectations) over personal metaphors (i.e., gratification as a personal experience) depended and depends upon the need of unpredictable collectivities for predictable performances. This view is condensed in the parable of the seven fat years followed by an inevitable reversal and is currently echoed by the new puritanism of the ecology movement.

It is possible, however, that the continuing transformation of technological processes, giving rise to the current images of a “postindustrial society,” will begin to create more stable and predictable collective processes—processes that require far less by way of self-disciplined individual actors or actors for whom the demands of self-discipline are narrowly

segmented, describing only a fractional part of social time, role commitments, or locations. It is also possible that economies of abundance, reflecting in part the greater rationalities and capacities of emergent technological systems, will lead to fewer overvalued rewards. In such economies, rewards might be evaluated in terms of their pragmatic value instead of standing also for social and moral worth.

The typical response to conditions of anomie has been to think in terms of restorative measures that would once again restructure social constraints and/or refurbish conventional rewards. Such a response is understandable when unanticipated affluence is predictably a temporary interruption of the "normal" order of social life. However, the capacity of advanced industrial societies to sustain long periods of affluence, albeit unevenly allocated, suggests a problem not of interruption but of transition. For example, the responses of members of one cohort, those who might almost classically fit the Durkheimian expectation by having been committed to performance before experiencing rewards, could be vastly different from those of a subsequent cohort that has experienced, if only experimentally, many of the available rewards prior to being expected to demonstrate performance abilities. Though there may be a sharing of value systems, the former cohort often being major socialization agents for the latter, the translation of values into commitments and behavior styles may well produce substantial differences. Thus, responses to expressions of anomie might well be examined more closely. We must learn to distinguish not only between the anomie of deprivation and that of affluence, but also, perhaps, between different kinds of the latter. Indeed, much of what appears to be anomie, for example a rejection or moderation of the achievement motive, might well be reinterpreted as being expressive not of decay but of transition. It is a more flexible approach to human motivation that seems very much in order. This note was struck by Merton in his description of a functionalist approach (with applicability well beyond functionalism narrowly defined) when he suggested the need to conceive "... of social structure as active, as producing fresh motivations which cannot be predicated on the basis of knowledge about man's native drives" (1957, p. 121).

A POSTSCRIPT ON RECESSION

Theories are in a considerable measure the products of specific historical moments and the experience of theorists. Too often sociologists unself-consciously substitute intellectual history (the changing present of the past) for the sociology of knowledge when discussing their own theoretical stock in trade. The content of the present paper was undeniably generated by the quarter-century of affluence that followed World War II

and the concurrent, largely unanticipated careers of the authors. The present economic recession (1975) raises for the essay essentially the same question that the essay itself raises with reference to Merton's approach to anomie theory: to what degree is its applicability (if any) limited to a fixed period of sociocultural time?

Though conservatives of both the Right and the Left offer different theories describing the sources of the recession, they tend to agree that the fact of recession demonstrates the impossibility of sustained affluence and, on the individual level, the need to relearn the lessons of hard work, productivity, and prudence. Despite the visible distress inevitably accompanying economic difficulties, there is an implicit celebration of a return to "normality." For many sociologists, particularly those working within academic institutions, the renewal of student concern for grades and serious careerism is viewed with an ambivalence frequently tipping toward the positive; for many it is a return to power as well as relief from their own versions of the anomie of affluence.

The authors, on the other hand, have viewed the experience of affluence not as the expression of some complex historical accident but as the expression of a cumulative and continuing technological revolution. A technological revolution that has pressed upon conventional economic, political, and other social institutions in the recent past and will continue to do so in the foreseeable future. Indeed, the recession, particularly when viewed in terms of its global character, may be seen as an expression of this continuing tension. In considerable measure, conditions highly generative of the anomie of affluence continue to exist for large, if for the moment diminished, segments of the population. It is possible that the very mechanisms that ease the consequences of economic downturns for the already affluent presage a return to the trajectory characterizing most industrial societies during the past quarter-century.

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Race, Achievement, and Delinquency: A Further Look at *Delinquency in a Birth Cohort*¹

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This paper outlines the consequences of an inadequate and cursory use of tabular analysis in Wolfgang, Figlio, and Sellin's *Delinquency in a Birth Cohort*. Through a reconstruction and reanalysis of some of the data presented I found that the verbal model proposed in that work concerning the interrelationships and relative importance of several variables was in error. An alternative model fits the data much better and supports conclusions in marked contrast to those stated. Contrary to the model suggested in *Delinquency in a Birth Cohort*, the relation between achievement level and delinquency status is not a spurious one due to a common association with race. Moreover, achievement appears to be at least as strongly related to delinquency status as are race and social class.

During the last decade delinquency research has grown increasingly sophisticated in matters of data collection and analysis. In fact, included among the 1973 nominees for the American Sociological Association's award for contributions to the quantitative analysis of sociological data was Wolfgang, Figlio, and Sellin's *Delinquency in a Birth Cohort* (1972), an analysis of the recorded delinquent careers of a cohort of nearly 10,000 boys in Philadelphia. However, in moving toward sophisticated modes of data analysis we continue to encounter very simple methodological errors which can have significant consequences for the adequacy of research conclusions. Hirschi and Selvin (1967, chap. 14) have convincingly demonstrated the dangers stemming from one such problem, percentaging in the wrong direction. This paper attempts to outline the consequences of a similar error for some of the conclusions presented in *Delinquency in a Birth Cohort*. This reanalysis should not detract from the overall value of that work as a study of delinquent careers but must be reported since it calls for major revisions in several of the authors' summary statements and interpretations.

RACE, SES, AND OTHER VARIABLES

The concluding chapters in detailed monographs are very important not only because they should "bring everything together" but because they

¹ The author first raised the issues dealt with in this paper as part of a book review in this *Journal* (Jensen 1974).

are likely to be a source of generalization and quotation in subsequent works as well. In the concluding chapter of *Delinquency in a Birth Cohort* we are presented with the following observations supposedly summarizing the relative importance of interrelationships among a variety of variables:

After examining the relationship between the various background variables of race, SES, types of schools attended, residential and school moves, highest grade completed, I.Q., achievement level, and the state of being delinquent or not, we concluded that the variables of race and SES (of somewhat lesser importance) were most strongly related to the offender-nonoffender classification. The remaining variables in the school records had little or no relationship to delinquency status. For example, the variable of achievement level is inversely related to delinquency, that is, high achievers are much less likely to be classified as offenders than are low achievers. However, the relationship between race and achievement is such that most of the variation between achievement and delinquency status is explained by race, for being a poor achiever is highly related to being nonwhite. [Pp. 245-46]

This argument is reiterated when the authors conclude that "these factors are indirectly related to delinquency because they are strongly correlated with race" (p. 253). In short, we are told that their analysis shows (a) racial and socioeconomic status to be more strongly related to delinquency than the other variables examined and (b) the relationships between variables such as academic achievement and delinquency to be "spurious" due to their common association with racial or socioeconomic status.

These are not minor or trivial conclusions. They have already been cited as matters of fact in a recent text on juvenile delinquency (Kassebaum 1974, p. 26). Moreover, they run counter to a variety of delinquency theories which posit nonspurious connections between achievement and delinquency. For example, "social control" theories imply a negative correlation between "stakes in" or "commitments to conformity" and involvement in delinquency (e.g., Hirschi 1969; Briar and Piliavin 1965, pp. 35-45; Toby 1957, pp. 12-17). From such perspectives variable stakes in conformity intervene to explain any association between sociodemographic variables, such as race, and delinquency. Thus, in marked contrast to Wolfgang, Figlio, and Sellin's stated findings, these theorists argue that the connections between such variables are indirect through their association with variable stakes in conformity. In sum, social control theorists imply that variables such as achievement level make a contribution to delinquency which *cannot* be attributed to their association with sociodemographic variables.

The plausibility of claims concerning these variables is important for quite pragmatic reasons as well. If, in fact, the relationship between achievement and delinquency is spurious, then efforts to improve achieve-

ment as a means of delinquency prevention will be ineffective. If, on the other hand, there is a nonspurious relationship between achievement and delinquency, policies oriented at enhancing achievement might have an impact on delinquency. This issue of spuriousness, then, is important both theoretically and pragmatically.

My concern over the adequacy of these statements stems from the mode of analysis used to reach such conclusions. Wolfgang, Figlio, and Sellin begin their analysis quite appropriately by examining the distribution of officially recorded delinquent acts among adolescents of various racial and socioeconomic statuses. These variables are shown, in fact, to be associated with juvenile delinquency. However, part way through their initial data chapter the authors switch the direction of the analysis from percentaging in the causal direction to either percentaging or presenting means in a descriptive direction (Zeisel 1957; Hirschi and Selvin 1967, pp. 235-54). For example, instead of presenting the distribution of delinquency among adolescents at different levels of achievement, they present us with the distribution of achievement level among categories of delinquent and nondelinquent youth. Yet, their conclusions concerning the relative impact of variables and their interrelationships require, *at the very minimum*, consistent presentation and analysis.

A REANALYSIS

In their appraisal of analytic methods in delinquency research, Hirschi and Selvin (1967, pp. 237-42) reanalyzed some well-known delinquency research and concluded that poorly constructed tables can mislead both reader and analyst and that even simple errors in percentaging procedures are potential sources of erroneous conclusions. They were also able to show that a reanalysis can often rectify some of these problems. Where possible such a reanalysis is attempted here using tables presented in *Delinquency in a Birth Cohort*. While the manner in which the data are presented for residential and school moves, highest grade completed, and IQ prevents a reassessment of their relative impact on delinquency, it was possible to derive the data necessary to assess the zero-order relationships between delinquency and three background variables: achievement level, race, and socioeconomic status. I was also able to reassess the interrelationships among race, achievement level, and delinquency.

Table 1 is based on table 4.1 in *Delinquency in a Birth Cohort* and summarizes the delinquency by SES by race relationship. Table 2 was created by repercentaging table 4.6 to make the direction of analysis consistent with the authors' earlier analysis. By merely inspecting the tables we can begin to question the adequacy of their claims. First, there is no evidence that race and SES are more strongly related to delinquency than

TABLE 1
DELINQUENCY BY SES BY RACE
(%)

	NONWHITE		WHITE	
	Lower SES	Higher SES	Lower SES	Higher SES
Nondelinquent	47 (1,151)	64 (293)	64 (1,377)	74 (3,649)
Delinquent	53 (1,293)	36 (165)	36 (763)	26 (1,254)

SOURCE.—Wolfgang, Figlio, and Sellin 1972, table 4.1.

NOTE.—N's shown in parentheses.

achievement level, and, second, the relationship between achievement level and delinquency cannot be attributed to its association with race. In fact, if we compare zero-order gamma coefficients relating each of the three variables to delinquency, achievement level (dichotomized) proves *more* strongly related ($-.62$) than race ($-.43$) or SES ($-.39$). With race constant, the net partial gamma coefficient relating achievement to delinquency is $-.43$.

While the percentage tables themselves led me to question their conclusions, I also examined the interrelationships among race, achievement, and delinquency utilizing techniques recently advanced by Goodman (1972, pp. 1035–86; 1973, pp. 1135–91) for the multivariate analysis of qualitative data. Goodman's procedures allowed a test of the "fit" between the expected multivariate contingency table based on the model claimed by Wolfgang, Figlio, and Sellin and the multivariate contingency table actually observed. I considered several other models involving race, achievement, and delinquency as well.

The first model noted in figure 1 represents that proposed in *Delinquency in a Birth Cohort* and posits a spurious relationship between achievement and delinquency due to a common association with race. The second model (H_2) is closer to the view advanced by some social control theorists, achievement level "intervening" between race and delinquency. The third model (H_3) assumes an association between race and achievement but allows both to contribute to delinquency.

Goodman's "likelihood ratio X^2 " statistics and their " P -values" for the three models are summarized in table 3. Model H_3 is the only one of these three to provide an acceptable fit to the data. Since the difference between H_1 and H_3 is the independent association between achievement level and delinquency, we can create a "partial correlation" indicating the relative decrease in "unexplained variation" when such a connection is introduced (Goodman 1972, pp. 1056–58). Similarly, since the difference between H_2

TABLE 2
DELINQUENCY BY ACHIEVEMENT BY RACE
(%)

	NONWHITE LEVEL					WHITE LEVEL				
	Very Low	Low	Average	High	Very High	Very Low	Low	Average	High	Very High
Nondelinquent	44 (217)	42 (151)	52 (137)	64 (47)	86 (12)	54 (105)	58 (215)	68 (529)	84 (625)	87 (478)
Delinquent	56 (277)	58 (209)	48 (128)	36 (27)	14 (2)	46 (89)	42 (153)	32 (253)	16 (122)	13 (73)

SOURCE.—Wolfgang et al. 1972, table 4.6.
NOTE.—N's shown in parentheses.

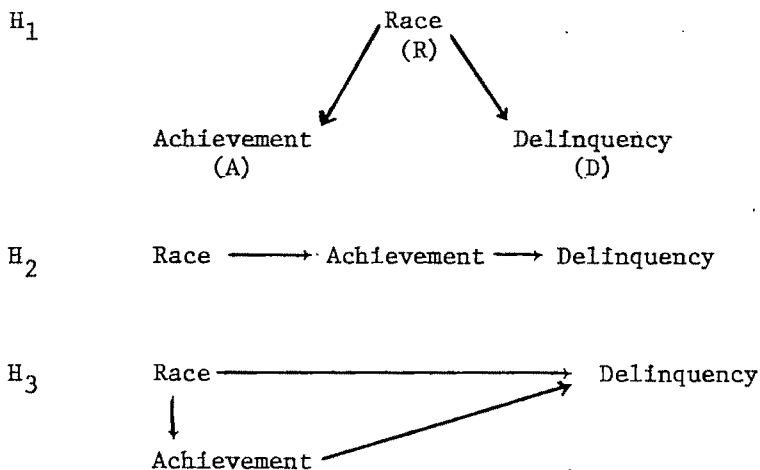


FIG. 1.—Three hypothesized models

and H_3 is the independent association of race with delinquency, we can create a partial correlation for that association. Following such a procedure, the partial correlation between achievement and delinquency is .97, and the partial correlation between race and delinquency is .92. Consistent with our inspection of the percentage tables, *both* race and achievement are related to delinquency:

The relationship between race, achievement, and delinquent status is depicted graphically in figure 2, where figures for "log-odds" delinquent are plotted for whites and nonwhites. We can observe that there is a persistent negative relationship between achievement level and delinquency for whites and nonwhites and that there is a rather persistent difference between whites and nonwhites as well. Only at the very highest achievement level are the two racial categories nearly similar for log-odds delinquent. However, only 1% of the nonwhites fall in this category. We can also observe that the relationship is nonlinear. There appears to be greater variation in log-odds delinquent between the middle and high achievement categories than between the low and middle categories.

TABLE 3
LIKELIHOOD RATIO X^2 AND SIGNIFICANCE FOR HYPOTHEZED MODELS

Model	Marginals Fitted	X^2	df	P
H_1	{RD} {RA}	211.85	8	<.001
H_2	{RA} {AD}	62.54	5	<.001
H_3	{RD} {AD} {RA}	5.22	4	.265

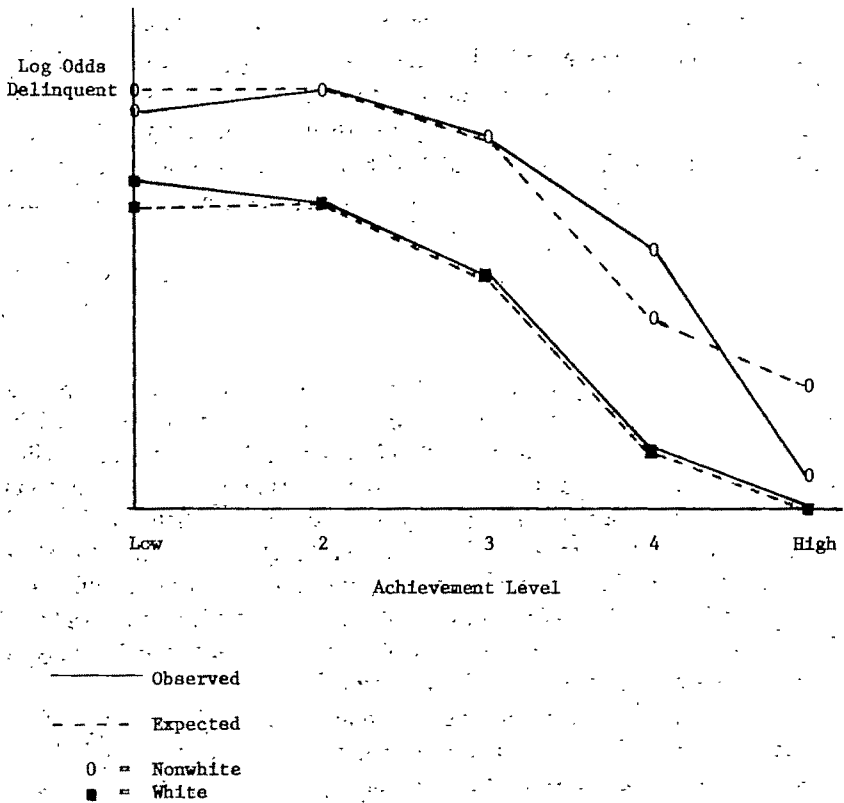


FIG. 2.—Log odds on delinquency by race and achievement: observed and expected values.

The shape of the relationship might be meaningful for programs concerned with reducing delinquency by increasing achievement level. For example, figure 2 suggests that movement from the lowest to the next lowest achievement category is not likely to reduce the probability of delinquency for nonwhites and might even increase it. Since the lowest category encompasses 41% of the nonwhites, achievement-oriented delinquency-prevention programs may appear unsuccessful if their accomplishments are limited to change at the lowest achievement levels. Improvements beyond the lowest levels might ultimately make a difference with respect to conflict with the law.

Of course, we must remember that we are dealing with cross-sectional associations and have been merely *assuming* that delinquency status is the dependent variable. Particularly since we are dealing with official designations of "delinquent," it is quite conceivable that labeling and any associated stigmatization may affect achievement. Moreover, the relationship between achievement and delinquency status may be spurious

as a result of common associations with variables other than race. Thus, we do not know if changes in achievement level will reduce the probability of trouble with the law. All we know is that variations in achievement as reported are associated with officially recorded delinquencies. There may be a wide range of factors associated with achievement which account for that relationship. The white-nonwhite racial distinction, however, is not one of them.

CONCLUSIONS

I hope that this reanalysis has served several purposes: first, to support a number of substantive conclusions in marked contrast to those stated in *Delinquency in a Birth Cohort* concerning race, achievement, and delinquency; second, to raise the possibility that a reanalysis of other aspects of that body of data might salvage yet other variables which are depicted as less important than race and SES or as only spuriously related to delinquency; third, to illustrate the dangers of reaching comparative multivariate conclusions on the basis of inconsistently constructed tables. Finally, I hope it can further illustrate the utility of recent techniques for the analysis or reanalysis of multivariate qualitative data.

Substantively, it appears that both race and achievement are related to delinquency and that the relationship between achievement and delinquency is nonlinear. Just as the relationship between achievement and delinquency may ultimately be shown to be spurious, so the relationship between race and delinquency may yet be shown to be indirect. Other sociocultural or social psychological variables may intervene between race and delinquency which "explain" the association of the former with conflict with the law. There are a variety of possible explanations for the relationships observed, and the findings might have been dealt with in the context of a variety of theoretical perspectives. *Delinquency in a Birth Cohort*, however, is virtually devoid of any consideration of theoretical perspectives concerning these interrelationships. In fact, it may be that the lack of concern with theoretical relevance contributed to inconsistent table construction. One of the virtues of theory is that it forces decisions concerning independent and dependent variables. Such an apparently simple decision puts restrictions on the form of analysis used. In addition, the analysis of findings in relation to theory encourages careful attention to proper modes of analysis, since one or another theoretical position may be negated. In this case erroneous findings were stated in such a way that they could be interpreted as inconsistent with one or more major brands of criminological theory. Thus, findings which appear superfluous to the

overall impact of a study may have great theoretical and pragmatic relevance whether the researchers recognize that relevance or not.²

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² NOTE ADDED IN PROOF.—Wolfgang, Figlio, and Sellin have been invited to prepare a comment on this research note.—Ed.

Commentary and Debate

AN ASSESSMENT OF GARNIER AND HAZELRIGG'S PAPER ON INTERGENERATIONAL MOBILITY IN FRANCE

In the history of sociology, rare have been the cases in which various research teams were able to work on the same data. The secondary analysis of the original data on social mobility in France by Maurice Garnier and Lawrence Hazelrigg, of the University of Indiana, is such a case (*AJS* 80 [September 1974]: 478–502).

What is interesting is that Garnier and Hazelrigg reach a different set of conclusions from those of the French demographers and sociologists who had analyzed the same data earlier (Praderie, Salais, and Passagez 1967; Bertaux 1969, 1971, 1974). The discrepancy is all the more interesting since these data are as objective as data will ever be; they do not deal with attitudes or opinions, but with “hard facts”: occupations and levels of education. The rates of social mobility calculated by the American authors are, of course, correct. The discrepancy is not located at this level, but at the level of the *sociological meaning* of these different rates—and the level of the hidden assumptions which have led to the very construction of these rates. Apparently, even hard facts do not speak for themselves—or at least they tell different things to different persons.

Too often the debate on social mobility has been restricted to technical matters. The opportunity to raise it to the substantive level should therefore be highly welcome. It is possible to do so here, not by opposing one theory to another without reference to data, but by discussing substantively the process of data analysis.

3 × 3 tables.—The first point of criticism concerns the system of categories employed by the authors. In their tables they use the conventional trichotomy: farm-manual-nonmanual. This has become a habit since Lipset and Zetterberg used it for international comparison purposes; it is, however, a bad habit. Is it not paradoxical to claim to observe and analyze *social* mobility, as opposed to occupational mobility, by using a classification which is based not on a system of categories expressing social stratification but on an *economic* view of occupations (Colin Clark's “three sectors” conception)? The error has been repeated many times since the *première* of Lipset and Zetterberg. As a result, sociologists in general, and Garnier and Hazelrigg in particular, keep talking about upward mobility, downward mobility, stratification, all concepts pertaining to social mobility, while at the same time using economic instead of social categories. This results in a chain of blunders.

To begin with, Garnier and Hazelrigg interpret the passage of sons of

farmers to the position of workers as "upward mobility": "Seven-tenths of the mobile sons were ascendant in comparison to their fathers, and of these nearly half represented a flow of manpower from the farm to the urban blue-collar sector" (p. 480). Notice that the authors do not even have to justify qualifying this flow as "ascendant": it goes without saying, it is a well-established idea of the literature of social mobility. Still, —.

In this flow there are five sons of farmers for each son of a farm laborer. It may be argued that the move from farm labor to factory is an upward move; but what about the move from the family's farm to factory work? First of all, it is undoubtedly a proletarianization in terms of relations of production. Now, in some cases (e.g., when going from a small farm to a skilled factory job) this proletarianization might acquire the social meaning of an upward move, in terms of life chances. In most cases, however, sons of farmers have acquired none of the skills which industry needs, and they find themselves projected into unskilled (or so-called semiskilled) jobs. This is verified by the data themselves: 316,000 such cases against 133,000 in skilled jobs (Garnier and Hazelrigg, table 4). When they do have a skill, it is very often in construction, transportation, or nutrition (bakeries, butcher's shops): the artisanal structure of these sectors results in comparatively low salaries and job insecurity. Thus, it is certainly grossly false to speak of the overall flow of farmers' sons becoming workers as representing upward mobility; in what is probably the majority of cases, it has most of the characteristics of a *downward* movement.

The very difficulty of qualifying this overall flow has, actually, a deep sociological significance. Farm workers, small farmers, and workers make up, together, the bulk of the productive—and thus, of the exploitable—part of "society." In French sociology they are often grouped together under the heading "*classes populaires*." In a classification of occupations by means of relative life chances (what some sociologists still mistakenly call "prestige scales"; on this, see Goldthorpe and Hope [1973]), peasants and workers find themselves at the same end, the lower one. Besides, one worker out of four comes from a farming family (two out of four come from an industrial working-class family). Thus, despite their extremely different characteristics, it makes sense to consider them together, for they stand in a similar relation to the rest of society: a relation of subordination and of exploitation (direct exploitation for the workers and farm workers, indirect exploitation for the small peasants). We shall see below how a sociological picture of social mobility can be drawn by grouping together peasants and workers.

Having missed the meaning of the "farm to manual" flow, the authors hit a second tree when dealing with the "nonmanual to manual" flow: they interpret it as a flow from white-collar origin to blue-collar destination. Most other authors, of course, have also seen it this way. And when inter-

national comparisons are made, France regularly appears as the Champion of the Northern Hemisphere for Downward Mobility with a huge 33% (only Australia has more: 36%). The 33% for France compares with 28% for the United States, 26% for Sweden, 24% for Italy, and 24% for Japan.

The picture changes, however, when one takes a closer look at the content of the nonmanual category. It appears that artisans and shopkeepers have been put into this category, of which they are a sizable proportion (two-fifths of the social origins, one-fifth of the destinations). It appears also that almost half of the cases of "nonmanual to manual descent" involve sons of artisans or shopkeepers who have become workers (244,000 cases out of 510,000). These are not exactly of white-collar origin.

This high figure should be related to the evolution of the socioprofessional composition of the French population in the last hundred years, as known through the Censuses. The Censuses confirm that the *petite bourgeoisie* has been very numerous in France, as compared with other countries (more than 10%); but also that it has been shrinking quite rapidly, especially since the Second World War. It is this shrinking that is now producing the flow of sons of this category into workers' jobs (Bertaux 1971, 1974). It is a flow of proletarianization. However, not all of it deserves the label of "downward mobility." Many sons of artisans who become *skilled* workers have rural-artisanal backgrounds, which provide a low initial level of life chances. Thus the sociological meaning of such a flow is not simple; but it is in any case very different from the meaning that the authors confer on it of a "white-collar to manual" flow. If an international comparison of this latter kind of flow were to be made, I would expect France to lose its championship by a huge margin.

8 × 8 tables.—In analyzing the 3 × 3 tables, Garnier and Hazelrigg followed the well-established trends, and these led them into traps. When moving from the 3 × 3 to the 8 × 8 tables, the authors could have refined their analysis and perceived some of their earlier mistakes. But such is the strength of the "empiricist consensus" that it prevented them from seeing the relevant features and turned their attention toward irrelevant questions.

To begin with, the authors did not take advantage of the refining of categories to separate artisans and shopkeepers from white-collar personnel, as all French authors have done. They preferred to combine small owners with middle management and other salaried personnel in a "middle-nonmanual" category. Having done so, they were surprised by the density of the flow of sons of middle-nonmanual origin who became workers. Such a phenomenon needed an explanation, and the authors found one:

Though we lack the data necessary to underline the point, it is conceivable that the influx of farm-born sons into the urban labor market stimulated recruitment and selection processes in such a manner that, through a series

of replacement chain effects, certain sons of middle- and low-level non-manual origin who otherwise would have been similarly occupied themselves were constrained to take lower-status positions. An expanding bureaucratic sector, with its characteristically attendant ideology of white-collar opportunity and desirability, could have resulted indirectly in an upgrading of the average intrinsic-skill qualifications of the pool of applicants for particular occupations such as technicians and middle-level managerial positions—at least enough of an upgrading to disqualify by comparison some sons who originated from middle-level nonmanual homes and who would have otherwise moved horizontally into those positions as adults. [P. 492]

Unfortunately, this brilliant exercise in sociological imagination does not survive a breakdown into finer categories. First of all, of 100 sons of the middle-nonmanual category, 38 are sons of artisans, 26 sons of shopkeepers, 16 sons of foremen, and only 20 are what I would consider genuine middle nonmanual! The proportions for the downward flow into workers' jobs are even worse (for our authors' imaginative hypothesis); the sons of genuine middle-nonmanual fathers make up only 8.5% of this flow, while a solid 50% of it is composed of sons of artisans; 20% of the mobile persons are sons of shopkeepers, 21% are sons of foremen. As for the sons of genuine middle-nonmanual fathers, they have remained nonmanual in the comfortable proportion of 80% (Praderie et al. 1967, table 3, or Bertaux 1969, fig. 19).

It is interesting to note that the authors have given rein to their sociological imagination on a point on which they "lack[ed] the data" (or at least, they thought they lacked them), as if it were somehow unsafe to theorize when you do have the data: one will find in their paper very few theoretical passages besides the one quoted above. This is again a typical feature of what I shall reluctantly qualify as "empiricism": the urge to theorize upon one empirical item (a proportion or a relation), instead of trying to see and think about the pattern formed by a whole set of relations. In fact the authors *had* the data necessary to make sense of the flow they observed, but they did not use them.

In another part of their paper, the authors again fall in the traps inherent in ad hoc theorization. Having observed that two categories, namely unskilled workers and unskilled nonmanual, are less self-recruiting than the others, they are led to theorize that those categories fulfill some "bridging functions" (p. 486). The hypothesis might be correct; certainly the "fact" on which it is based is fragile: the only reason why the above-mentioned categories are less self-recruiting is that they are smaller than the other categories (as constructed by the authors); relative to their size, these categories retain their sons exactly as much as the others, as can be seen in table 6 of Garnier and Hazelrigg's paper itself.

Hazelrigg's Model

The second part of the paper consists of an application to the French data of Hazelrigg's technique for "partitioning structural effects and endogenous mobility processes" (Hazelrigg 1974, p. 115). The result is a model of intergenerational mobility in which the flows are "estimations of the conditional probabilities of status change that would have obtained had the occupational structure of France remained constant" (p. 490). The disturbing point is the fact that the model generates a number of flows which are, very obviously, sociological absurdities—and that the authors take them seriously, going so far as to theorize upon them. For instance, the model yields a flow of 13.6% of farm workers' sons who become farmers, as against the low 3% actually observed; and they conclude that it is structural change (the diminution of the number of farms) which has prevented the other 10.6% of farm workers' sons from becoming farmers.

But anyone who knows anything about social relations in the French countryside knows that what prevents the sons of farm workers from becoming farmers is the institution of inheritance. The relation to the land, be it one of ownership or of *fermage* (renting land), is transmitted from fathers to sons. One has to be the son of a farmer to become a farmer. The data show it: 92% of farmers are farmers' sons. The sons of farm workers actually become anything but farmers, as shown by the observed 3%, despite all the "orientation towards farming" that armchair sociologists might like to imagine.

To be fair to the authors, one should add once more that they have only reproduced in their paper errors that are quite common in the literature. For instance, they rely on the well-known work of Blau and Duncan (1967); but the idea of inheritance (capital inheritance, i.e., inheritance of an institutional relation to some means of production or distribution) is almost completely ignored in this work. The entry "inheritance" in the index of the book refers only to the "job inheritance"—a quite different concept (and a misleading one, as I shall show). Besides, in one of the very few passages where Blau and Duncan mention the fundamental process of capital inheritance (the passage actually referred to by Garnier and Hazelrigg), they manage to treat it from a psychological point of view! They write: "It seems that proprietorship . . . *discourages* sons from leaving the occupation of their fathers" (my emphasis); and four lines farther on: "[proprietorship may produce a stronger occupational] investment and commitment than mere employment, and these are transmitted to sons" (p. 41). This is not necessarily incorrect, but it misses the point entirely. Proprietorship is a socially structuring feature, not because of the feelings it induces in the hearts of proprietors' sons, ("investment," "commitment," "discouragement" from quitting) but because of the fact that it divides

the population into proprietors and nonproprietors. The nonproprietors are obliged to hire themselves out as employees of the proprietors and thus to reproduce the capital/labor relation. This situation of *prolétaire* (one who is forced to sell his labor) is what the *nonproprietors* transmit to *their* sons. This is why the notion of "job inheritance" is so misleading: it covers two completely different processes, or rather one process (capital inheritance) and its inverse: the "inheritance" of a proletarian situation. Besides, job inheritance orients one's thought toward the level of individuals, or rather, families; whereas capital inheritance allows one to perceive the *whole* process, and the relation between one part of it and the other.

Notions are not innocent. I have met supporters of Blau and Duncan's book who argue that capital inheritance is irrelevant because "it concerns only a very small part of the population." For these so-called sociologists, "persons who do not own capital cannot be concerned by capital inheritance." The statement sounds pretty straightforward. It is to be expected that, when one is immersed in the empiricist, atomistic, psychosociological view of society, he will arrive at this idea and will be totally unable to see the truly sociological aspect of the process. (I believe it can be shown that all the quantitative methods tend implicitly to reinforce the atomistic view of society—and thus lead some modern sociologists to miss precisely those aspects of social questions which are sociological; but it would take too much space to develop this point.)

To come back to Garnier and Hazelrigg's paper and to the question of inheritance: like many others, these authors refer to the supposed "boundary" between nonfarm and farm which would prevent nonfarm people from falling into farm jobs. "As in other industrial societies, the farm-nonfarm boundary was highly permeable to intergenerational flows of manpower away from the farm, but much less so to flows into the farm category" (p. 480). (The vocabulary evokes a physical phenomenon, and more precisely the phenomenon of "semipermeable membranes.") Why not explore the hypothesis that it is ownership of the land which prevents sons of "non-farmers" from moving into farm occupations? This hypothesis is consistent not only with the absence of movement from nonfarm to farm but also with another observed process that the boundary hypothesis does not explain: the absence of a flow of farm workers' sons into positions of farmers. Besides, it is a sociological hypothesis, not a physical analogy.

Hazelrigg's model also generates certain currents from the category of sons of upper *cadres* (*cadres supérieurs*, executives) and professional men into the category of workers. I think it may be said that such flows have never been witnessed in France. For instance, of the 97 sons of engineers actually questioned in the survey, only one was a worker; that is to say, 4% with correction for representativeness (sampling rates vary according to the categories). Of the 65 sons of schoolteachers, only one was a worker.

Of the 121 sons of professional men, three were workers—themselves in the professions (with the corrections for representation the proportions become respectively 8% and 20%). The genuine downward mobility (class change) among the scions of the upper classes and of their allies is, in my opinion, the most significant feature of the survey. The blank squares of the mobility table are more interesting to me than the others: they lead to the formulation of a hypothesis for the theory of “mobility” or, more correctly, of anthropodistribution (the distribution of human beings in social structure): it is the hypothesis of “not down,” of the existential struggle on the part of the families in the upper and middle classes of society to keep their children from “sinking,” in terms of status (Bertaux 1971). This simple hypothesis opens the door to the understanding of many phenomena related to the distribution of social status, including social mobility. The paradox of this hypothesis is that it postulates the existence of a process which can be verified only in the absence of flows—a feature which may confuse the empiricist and give no mental image of the whole process; when there is “no flow,” they see nothing.

There is therefore something wrong in the Hazelrigg method to “forget” that the mobility tables represent a social process rather than a statistical distribution. Social determinations (such as capital accumulation) which the logic of the model cannot take into consideration nevertheless exert a very heavy influence on the actual social process. Sociologists have to be treated sociologically; no methodological tricks can be applied to them. If one wants to know what the circulation would have been in the absence of structural effects (provided this question is not asked by one who is not a mathematician (Deming) that one should ask. Or, at least, it is the right moment to remember that one is a sociologist and that there is such a thing as social history.

About “Education”

The last part of the paper compares the relative influence of the father's occupation and son's education upon the son's eventual occupation. Though only preliminary results are given, the direction in which the authors are heading appears to be that of increased methodological sophistication, to the detriment of sociological thought. Let me mention one inadequacy. Quantitative techniques like path analysis consider “education” (the passage through “educational” institutions) to have the same “objective” meaning, or rather the same status for all persons being “educated.” Path analysis must make a distinction or be dropped. But does education have the same meaning

meaning, not subjective meaning) for the children who are going to inherit some means of production (such as the sons of farmers, of shopkeepers, of owners of businesses, provided a farm or business is large enough to allow a decent living) and for the children who will inherit nothing? The latter will be "distributed" into the occupational system by, precisely, the institution called "the educational system" (mostly through elimination camouflaged as "school failure"), while the former will be distributed through inheritance (Bertaux 1974). It seems therefore that the two groups do not stand in the same relation to the educational system. If they do not, is it legitimate to lump them together in a causal analysis based on statistical associations between father's occupation, son's education, and son's occupation? The statistical associations, which look formally alike, may hide social-structural relations which are wholly different.

Alternatives

Of course I do not want to say that quantitative techniques should be rejected, only that they should be applied on a sound sociological basis. Let me try to exemplify this statement. If one regroupes the INSEE categories into three sociological categories, "upper," "middle," and "lower" (meaning of course that the "lower" categories have the worst life chances, not that they are lower in any other sense), he gets a wholly new picture of the process of social distribution: see below, the second half of table 1,

TABLE 1
INTERGENERATIONAL DISTRIBUTION ACCORDING TO DIFFERENT CLASSIFICATIONS

FATHER'S OCCUPATION	SON'S OCCUPATION, 1964			
	Nonmanual	Manual	Farm	Total
Nonmanual	20.8	10.5	0.7	32.0
Manual	9.4	23.6	0.6	33.6
Farm	5.9	12.6	15.9	34.4
Total	36.1	46.7	17.2	100.0
Total <i>N</i>	4,880,400
	Upper	Middle	Lower	Total
Upper	2.6	2.5	0.8	5.9
Middle	3.1	12.6	10.4	26.1
Lower	1.4	13.9	52.7	68.0
Total	7.1	29.0	63.9	100.0
Total <i>N</i>	4,880,400

NOTE.—Nonmanual, manual, and farm categories defined as in Garnier and Hazelrigg (1974); upper, middle, and lower categories defined as in Bertaux (1969, p. 454).

to be compared with the usual classification given in the first half of table 1. Both tables are of course derived from exactly the same data.

Three features which remain hidden in the first half appear immediately in the second half of the table. First, there is practically *no structural mobility*: the relative proportions of the three large strata remain constant from one generation to the next. This is confirmed if one takes into account the differential fertility rates (Bertaux 1969, p. 471). This result has considerable theoretical significance. Specialists in social mobility tend to accept a view of the social structure as constricting at the bottom and expanding at the summit (fewer farmers and more *cadres*). This representation has practically acquired the status of a demonstrated fact. But if one compares the two parts of table 1, he reaches a very different conclusion: French class structure appears to be reproduced from one generation to the next without change in its structural proportions, in spite of the transformation of its elements. The productive-exploited classes have evolved from a predominantly agricultural and rural character to a predominantly industrial and urban one, but they still made up two-thirds of the whole. The middle classes, once represented by small property owners (many of them rural), now tend to be made up increasingly of salaried urban *cadres* and lower civil servants. The capital-owning bourgeoisie may be contracting, but the stratum of managers of corporations and state institutions is expanding. Thus if one speaks strictly in terms of *social* mobility, there is very little structural mobility.

The second conclusion derives from the first: since there is almost no structural mobility in terms of social status, the bulk of social mobility consists of exchange mobility (also called "circulation"): *some will rise only if others descend*. This is a tough conclusion, in clear opposition to the often implicit assumption that industrial societies create large currents of upward mobility which are not counterbalanced by equally important downward currents. (What is probably happening in the 19th and 20th centuries is an upward move *en bloc* of the so-called industrial societies, to the detriment of Third World countries; this fundamental historical process is a process of group mobility, but it remains hidden by the talk about the supposedly high rate of *individual* social mobility in industrial societies.)

A third conclusion is obtained when the observed flows are compared relative to the sizes of the social categories between which they take place (the calculations are identical to those generated by the so-called random-assortment or perfect mobility model; the latter models, however, include also value connotations that are superfluous). The results are given in table 2. Exchanges appear to be relatively more frequent between "middle" and "upper" strata than between "middle" and "lower" strata. Although this first result should be studied in more detail, it throws addi-

TABLE 2
RELATIVE STRENGTHS OF TABLE 1 (BOTTOM HALF) FLOWS

FATHER'S OCCUPATION	SON'S OCCUPATION		
	Upper	Middle	Lower
Upper	6.2	1.5	0.2
Middle	1.7	1.7	0.6
Lower	0.3	0.7	1.2

tional light on what is already known of the dynamics of 20th-century France. Social historians tend to see at the core of these dynamics, a class alliance of the ruling *grande bourgeoisie* with the *petite bourgeoisie* to counter the rise of working class socialism; the alliance is said to have been made after the Paris Commune of 1871. According to this view, the *petit bourgeois* character of France from the 1880s to the 1960s stems from this class alliance, which manifested itself in protective tariffs, a new, "democratic" (i.e., open to the *petit bourgeois*) school system, and many other legislative and political characteristics dating from the 1880s. The social mobility data add one element: the class alliance might have bettered the social prospects of the children of *petit bourgeois* families (table 2 is not sufficient to prove the hypothesis; proof would require a comparison with similar tables on other countries).

Conclusion

Garnier and Hazelrigg's paper may thus be read as an accumulation of misinterpretations of the data; but this reading is possible only because a lot of work has already been done elsewhere on the same data. I tend to think that many well-known papers and books written from the empiricist point of view continue to stand only because no other reading of the data than the one they propose is available.

One fundamental assumption of empiricists is that it is possible to discover the sociological significance of the data of one survey by limiting oneself to the analysis of that survey. Thus one tries to replace historical knowledge and sociological thought by methodological sophistication and a commonsense discourse spiced from time to time by some psychosociological remarks. From Stouffer to Lazarsfeld and now Duncan, three generations have been educated in this style of research and are convinced that empirical sociology is identical to the analysis of single surveys.

My own work with the INSEE survey has led me to different conclusions. Having lived with its data for seven years now, I have learned that in order to understand its sociological meaning I had to pursue historical knowledge and theoretical questions. It might seem to be a series of long

detours in order to arrive at an understanding of one survey; however, I believe it is the only sound way. Methodological shortcuts invariably lead into dead-ends.

Thus, if Garnier and Hazelrigg have accumulated errors, it is not through some lack of methodological accuracy. Their calculations are correct. It is their overall point of view which is not. But "their" point of view is not theirs, it is that of empiricism, which so strongly dominates empirical sociology nowadays. One cannot make them responsible for it. I have tried to level my critique at the point of view itself, not at the particular application they made of it. On the other hand, both authors should be thanked for having taken the risk of a confrontation between their own analysis and that of the French sociologists. Sociology as such can only benefit from such encounters.

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REPLY TO DANIEL BERTAUX'S ASSESSMENT

There are two levels of critique in Daniel Bertaux's commentary on our paper: one consists of a critique of theoretical perspective; the other of a series of claims regarding technique, within the given perspective. We will address his criticism of technique first, in order to clear out some of the underbrush, and then turn to the issues of theoretical perspective.

I. On Technique

Although the questions about technique in our paper clearly are not separable from issues of theoretical perspective, we will focus on the former in this section, for the purpose of clarifying our procedures and answering Bertaux's more particular charges. In general, our format will be to discuss each of Bertaux's claims in the order in which he presents them.

About the trichotomy.—Unquestionably a trichotomy presents less variation than a more detailed classification, and in a given situation one trichotomy may be preferable to another, depending on the purpose at hand. Our use of the nonmanual-manual-farm trichotomy was governed mainly by consideration of the accumulated body of transition matrices from various countries, most of which have been reported either in the form of that trichotomy or in a form that is most amenable to reduction to that classification from the standpoint of comparability. Certainly the trichotomy is crude, certainly it is not devoid of difficulties of comparability across populations; but certainly also the distinctions among nonmanual, manual, and farm occupations are not without relevance in describing populations that were economically active 30, 20, or 10 years ago—indeed, the distinctions are not irrelevant today.

However, Bertaux's concern about our use of the trichotomy runs deeper, extending to our alleged "claim to observe and analyze *social* mobility, as opposed to occupational mobility, by using a classification which is based not on a system of categories expressing social stratification but on an *economic* view of occupations" (Bertaux's emphases). We have two objections to this allegation. First, we did not claim to observe and analyze "social mobility" and "occupational mobility" as interchangeable, isosemic terms. Second, we cannot understand the view, expressed by Bertaux, that economic categories are inappropriate to the study of social stratification. Inasmuch as the latter refers to matters of the distribution, exchange, and consumption of resources, "the economic" is very germane (although we are perfectly willing to admit that "the economic" is also "the social").

As for the question of what constitutes upward and downward movements in the occupational hierarchy, it is of course correct to say, as Bertaux does, that in relation to the "*classes supérieures*" both urban manual workers and peasants are "at the same end, the lower one." Nonetheless, in view of what Bertaux acknowledges as "their extremely different characteristics," it is reasonable to treat urban workers and peasants (or farmers and farm laborers) separately as *occupational* categories. Admittedly the question of their relative rank is difficult to answer clearly for all observations, insofar as the nominally defined categories (i.e., gross occupational titles) will sometimes overlap at a level of finer description. For instance, some "farm families" are owners and/or managers of large-

scale commercial enterprises, and, for sons of these families, a movement into the urban manual sector at any skill level could well be described as downward rather than upward. But at the time of the survey in question most farm families in France either were unpropertied farm laborers or renters, or were owners of very small holdings. Excepting the wine-growing farms, the minimum viable acreage in France is, on the average, about 50 acres, yet about one-third of the farms are half that size or smaller. Average annual base wages have been typically higher among even unskilled manual workers than among farm laborers (e.g., Fr 9,600 for unskilled urban workers versus Fr 8,700 for farm laborers; see Pohl, Thélot, and Jousset [1974], table 16.1), and in view of the circumstance that offspring of the owners of farms (most of which are very small) must share inheritance of farm property, average income of the farm-born sons would have been quite low had they all remained on the farm. Moreover, urban workers have had relative advantages such as more aggressive unions, subsidized housing, and entitlement to various kinds of transfer payments (e.g., family allowances) that were not available, prior to 1970, to persons in the farm sector.¹ For reasons such as these, we believe our description of the "overall flow of farmers' sons" to manual jobs "as representing upward mobility" was justified in the context of occupational stratification. Contrary to Bertaux's implication, we did not claim that by moving into the category of urban manual workers the farm-born sons gained greater control over their conditions of production, that is, were "ascendant" in terms of the relations of production. (Nor can we accept the romantic view that farm owners in industrial society, excepting perhaps the few very large proprietors, have more control over their conditions of production than do urban workers.) In fact, we made no claims of isomorphism between occupational hierarchy and class structure; there is no simple correspondence between the two.

According to Bertaux, we managed to "hit a second tree" in interpreting the nonmanual to manual flow as downward mobility from white-collar origin to blue-collar destination because we included shopkeepers and artisans in the nonmanual category. It seemed to us then, as it does now, that the shopkeepers (*petits commerçants*) fit better on the nonmanual than on the manual side, inasmuch as they are proprietors and sometimes employers (and were grouped in the INSEE report, along with *gros commerçants*, *industriels*, and artisans, as *patrons de l'industrie et du commerce*; see Praderie, Salais, and Passagez [1967], tables 3 and 4). The case of the artisans was troublesome: while some artisans may be described as proprietors and employers of dependent workers, the bulk of them would be better placed in the manual category; but the basic

¹ During the past several years, the French government has "liberalized" its welfare policy toward the agricultural sector.

table of the INSEE report (Praderie et al. [1967], annexe 2A, reproduced below as Appendix A) did not segregate artisans from shopkeepers. Bertaux is concerned that the flow from the "*petite bourgeoisie*" to the category of urban manual workers, while in his words a "proletarianization," does not entirely deserve the label of downward mobility. We certainly acknowledge that the gross descriptions afforded by a trichotomous classification will include a proportion of errors; our construction of the more detailed 8×8 transition matrix was in part an attempt to reduce such misdescription. But we do not agree that our use of the 3×3 matrix—presented as a basis for generating a "distribution of grossly defined status change" that could be juxtaposed to similar distributions from other industrial societies (1974, p. 481)—was inappropriate.

About the 8×8 matrix.—Bertaux argues that our eightfold classification of occupations was not sufficiently detailed, especially with respect to the "middle-nonmanual" construction. Indeed, we could have added more detail, insofar as the basic INSEE table that we utilized (Appendix A below) reported data separately for the category of middle *cadres* (*cadres moyens*, administrative personnel) and foremen.² But the consequences of the absence of that refinement were not so severe as Bertaux suggests. This can be seen from an inspection of data previously presented by Bertaux (1971, table 3, reproduced below as Appendix B). For example, Bertaux says that we were "surprised by the density of the flow of sons of 'middle nonmanual' origin who became workers," the implication being apparently that the sons of "genuine middle nonmanual" origin (i.e., the *cadres moyens*) moved infrequently into the category of urban workers. Yet, as juxtapositions of outflow rates from Bertaux's table and from our 8×8 matrix (1974, table 4) demonstrate, the chances of movement into the urban manual category by sons of middle-*cadre* origin, while not as great as those of sons of shopkeepers or artisans, were nevertheless quite substantial (see table 1). Approximately 21% of the sons from middle-*cadre* homes occupied urban manual positions in 1964; the comparable flow for our middle-nonmanual category was 36%.

Similarly, there is the suggestion in Bertaux's remarks that sons of middle-*cadre* fathers remained in the nonmanual category in "comfortable proportion," but that sons of shopkeepers or artisans did not. Again we may juxtapose the two sets of results (see table 2). While undoubtedly the former had the advantage, the likelihood of the latter being in nonmanual occupations in 1964 was quite large. Whereas the rate of nonmanual persistence among sons of middle-*cadre* origin was 75%, it was 62% for sons with origins in our middle-nonmanual category.

² As already noted, given the construction of the INSEE table, we could not separate the artisans and foremen as urban manual workers from the shopkeepers and middle *cadres*.



TABLE 1

RATES OF OUTFLOW FROM VARIOUS CATEGORIES OF NONMANUAL OCCUPATIONS
TO THE URBAN MANUAL CATEGORY (%)

Bertaux's Results		Our Results	
<i>Industriels, gros commerçants</i>	15 }	12	High nonmanual
<i>Cadres supérieurs, professions libérales</i>	9 }		
<i>Cadres moyens</i>	21 }	36	Middle nonmanual
<i>Petits commerçants, artisans</i>	38 }		
<i>Employés, services (autres)</i>	44	43	Low nonmanual

With regard to the "percentage of sons of 'middle-nonmanual' origin who became workers," Bertaux takes us to task for proposing an "explanation" (1974, p. 492) that is not supported by examination of a more detailed outflow matrix. To some extent he is probably correct. The cited "explanation" focused on the circumstance that the upward redistribution of occupational positions, according to our estimations, generally had the effect of inhibiting downward movements that otherwise would have resulted from circulation—except in the cases of flows from middle- and lower-nonmanual categories to the category of skilled urban workers, which may have been *increased*. Bertaux's contention is that in the case of our middle-nonmanual category that effect was located in the shop-keeper-artisan component and was explicable in terms of the relative constriction of that component in the occupational hierarchy (e.g., compare the appropriate marginals in Appendix B below). As the viability of particularly the artisanal activities diminished, large proportions of sons of artisans who otherwise would have themselves been artisans moved into the category of skilled workers (and, as we acknowledged earlier, were not thereby downwardly mobile). In this, Bertaux is probably correct. However, our discussion pertained also to sons of low-nonmanual origin (i.e., clerical and sales employees), and Bertaux's counterargument does not seem to apply to that flow. We would add that our "explanation" was

TABLE 2

RATES OF NONMANUAL PERSISTENCE BY VARIOUS CATEGORIES OF
NONMANUAL OCCUPATIONS (%)

Bertaux's Results		Our Results	
<i>Industriels, gros commerçants</i>	85 }	87	High nonmanual
<i>Cadres supérieurs, professions libérales</i>	88 }		
<i>Cadres moyens</i>	75 }	62	Middle nonmanual
<i>Petits commerçants, artisans</i>	60 }		
<i>Employés, services (autres)</i>	53	56	Low nonmanual

presented explicitly as only a possibility, a hypothetical description of flows through the hierarchy of occupational status. Certainly we did not intend it as a construction of theory.

On another matter, Bertaux claims that "the only reason [the lower-nonmanual and skilled-manual] categories are less self-recruiting is that they are smaller than the other categories" and therefore that our description of the categories as "bridging" or "distributive" occupations was based on "fragile" evidence. In fact, the measure of self-recruitment was simply the inflow rate—a column-based percentage—which means that comparisons across categories (columns) were not hindered by differences in sizes of the categories (column totals).³ Only 12% of the lower-nonmanual sample members were sons of lower-nonmanual fathers; the remainder came from other origin categories. Similarly, only 12% of the unskilled blue-collar sample members were sons of unskilled blue-collar fathers; the remainder came from other origin categories. By contrast, the next smallest rate of self-recruitment in the 8×8 matrix was 27%, for the category of semiskilled workers (see also Appendix A).

Partitioning observed status change.—Bertaux argues that our application of the Deming-Stephan technique in estimating rates of circulation is "bizarre," that it "generates a number of flows which are, very obviously, sociological absurdities" or which "have never been witnessed in France." Inasmuch as our application of the technique was an attempt to gauge the rates and patterns of circulation that would have occurred *had there been no change* in the distribution of occupational positions,⁴ it is not surprising that the estimated circulatory flows have never been witnessed. We did not declare that they had. With regard to the example of "sons of upper *cadres* . . . and professional men," for instance, it is quite true that only 4% actually moved into the category of urban workers and that such observations may be taken as evidence supportive of what Bertaux calls the "not down" hypothesis. But the relevant point of our partitioning procedure is not contradictory: in a situation of "pure circulation" (or our rough approximation of such a situation) *many more* than 4% of those sons would have been downwardly mobile to blue-collar occupations; structural factors represented in the changing distribution of occupational positions had the effect of retarding that otherwise implied downward movement.

³ The mobility ratios in our table 6 to which Bertaux refers were computed from *outflow*, not inflow, rates.

⁴ The estimates resulting from such application are admittedly approximate, inasmuch as the distribution of "fathers' occupations" derives from the retrospective and variable accounts given by the sample members (the "sons," some of whom could have been fathers of other members of the sample) and does not necessarily provide an accurate description of the occupational hierarchy as it was at any particular time. But that is not the point of Bertaux's objection.

Bertaux's other example of the "absurdity" of results given by the partitioning technique concerns the flow of farm workers' sons into the category of farm owners. Again, the point of the analysis was that under conditions of "pure circulation" nearly 14% of such sons, instead of the actually observed 3%, would have become farm owners, the disparity between the two outflow rates being attributed to shifts in the distribution of occupational positions. This hardly denies the operation of "capital inheritance," the transmission of farm ownership from father to son. Inspection of our observed and adjusted matrices (1974, tables 4 and 5) shows that whereas 80% of the sons of farm owners would have received proprietorship of farms in the constructed situation of circulation, the proportion was actually 43% because of the diminution of the number of farm proprietorships available. No doubt the institution of capital inheritance would have been operating in the situation of "pure circulation," as it has in fact been operating in the observed situation of actual status changes. If anything, its effect on rates and patterns of occupational status change may have been *more* restrictive because of the diminution of farm proprietorships; as shown in our matrix of "mobility ratios" (1974, table 6), the observed and adjusted rates of self-supply for the category of farm owners (43% and 80%, respectively) were larger than the random-expectation values by factors of 3.2 and 2.8.

About "education."—Bertaux is quite right in insisting that the social meaning of a given quantity of formal schooling or level of certification need not be the same for all groups of persons. But we made no claim to accomplish more in our analysis of educational- and occupational-status attainment than "a preliminary examination" based on admittedly gross measures and a very simple analysis design. As such, the analysis provided a brief description of the surface of some empirical relationships among father's occupational status, son's level of educational certification, and son's occupational status for the population as a whole, which description we could then compare roughly with similar descriptions for other societies. Our examination does not preclude a more detailed investigation of those empirical relationships or of the structure beneath them.

Bertaux's alternatives.—As a preferable classification of occupational categories, Bertaux regrouped the INSEE categories into a trichotomy of "upper," "middle," and "lower" and retabulated the data on status change accordingly. Relative to our 8×8 matrix (1974, table 4), his top stratum is the same as our "high nonmanual" and his middle stratum consists of what we termed "middle" and "low nonmanual," leaving the remainder—skilled, semiskilled, service, and unskilled workers, farm owners, and farm laborers—to form his bottom stratum. Not surprisingly, since his classification assigns two-thirds of the population to the lowest category, the overall amount of status change recorded by his 3×3 is somewhat less

than was shown by ours (32% versus 40%; for upward mobility alone, 18% versus 28%). Certainly the outflows from the bottom category are substantially smaller, and the outflows from the top category are substantially larger, in Bertaux's than in our 3×3 matrix.

We do not dispute the claim that Bertaux's alternative classification of occupational categories has utility. Nor do we dispute the argument that the topmost sector of the occupational hierarchy has not expanded very much, proportionately, and has not been very open to flows from the "bottom" two-thirds of the labor force; our own 8×8 matrix (1974, table 4) showed as much.⁵ However, a point that we tried to make in the earlier analysis—that the characterization of French society as having "a relatively low rate of intergenerational *occupational* mobility and a relatively 'closed' or 'rigid' system of *occupational* stratification" (1974, p. 498; emphases added) is exaggerated—is not contradicted by Bertaux's alternative construction of data. In table 3 we have retabulated data from the Australian and Italian studies (presented as comparison cases in our paper), according to the definition of Bertaux's trichotomy.⁶ These and the French data are shown as outflow rates and corresponding mobility ratios (based on the random-expectation model). Together, they underline our earlier conclusion: relative to other industrial societies, the likelihood of occupational status change in France was generally neither especially high nor especially low.

II. On Theoretical Perspective

Facts, "hard" or otherwise, are social products. They *do* speak, in an elliptical sort of way, but what they say is affected by the manner in which they are produced. Some theoretical perspective, or conglomerate of perspectives, is always involved in the construction of the product. One does not discover facts, one *makes* them—just as one makes bread.

Bertaux has objections to a theoretical perspective that he discerned in our paper, and to this level of his commentary we are more sympathetic. Our paper was intended not as a contribution to theory but as a primarily descriptively analytic account of features of occupational stratification and status change. But of course it inescapably contains theoretical perspective.

⁵ We doubt that anyone who has referred to trends in the shape of the occupational hierarchy ("constricting at the bottom and expanding at the summit") was unaware that the expansions have been concentrated in the managerial and bureaucratic sections of the nonmanual category and not in the top-most strata.

⁶ The data for the United States have not been represented here, because the occupational classification given in the Blau and Duncan study (1967, table J2.1) did not provide a detailed enough classification of the nonmanual category. Sources for the Australian and Italian data are Broom and Jones (1969, table 3) and Lopreato and Hazelrigg (1972, table 13.4).

TABLE 3

COMPARISONS OF OUTFLOW RATES USING BERTAUX'S TRICHOTOMOUS CLASSIFICATION:
AUSTRALIA, FRANCE, AND ITALY (%)

FATHER AND COUNTRY	SON'S OCCUPATIONAL CATEGORY			Total
	Upper	Middle	Lower	
Outflow Rates				
Upper:				
Australia	42	27	31	100
France	44	42	14	100
Italy	70	14	17	101
Middle:				
Australia	13	46	40	99
France	12	48	40	100
Italy	24	40	36	100
Lower:				
Australia	4	24	72	100
France	2	20	78	100
Italy	8	9	83	100
Mobility Ratios				
Upper:				
Australia	4.7	0.9	0.5	...
France	6.3	1.4	0.2	...
Italy	4.1	1.0	0.2	...
Middle:				
Australia	1.4	1.6	0.6	...
France	1.7	1.7	0.6	...
Italy	1.4	2.9	0.5	...
Lower:				
Australia	0.4	0.8	1.2	...
France	0.3	0.7	1.2	...
Italy	0.5	0.6	1.2	...
Aggregate rates:				
Upward	22	18	16	
Downward	13	14	8	
Total	35	32	24	
Dissimilarity index	8.3	4.1	6.8	
Somers' dyx	0.37	0.45	0.57	

Bertaux objects to a perspective that sees the descriptive analysis of rates, avenues, determinants, and consequences of the movement of individuals through an occupational-status hierarchy, wherein the notion of occupational status is conceived as an atomistic property of atomistic individuals, with little or no attention to the social meaning of historical-structural relations, as constituting the theoretically most important issue in the study of social stratification. He is hardly alone in this objection.

The study of social stratification (including but not restricted to occupational stratification) most directly concerns matters of the distribution and then the exchange and consumption of resources. It presupposes some perspective on and understanding of the historical structure of production of resources—that is, of class structure in the mid-19th-century sense. The stratification of a population in terms of resource distribution does not occur independently of the conditions and composition of resource production; rather, it is informed and limited by them. Given this set of relationships, one might imagine that the study of class structure would be a major activity in the sociological workshop, that it would be a crucible of much innovation in research techniques, that it would be the topic of a very large corpus of incisive, powerful literature. But it is currently none of these. Both quantitatively and qualitatively the study of class structure must be counted among the minor activities performed in the sociological workshop; the study of social stratification, not of class structure, has been the locus of most of the recent innovations in (or borrowing of) research technique; with very rare exceptions the only incisive, powerful literature on class structure dates from the previous century. (Much of what currently passes as the study of class structure is based on the untenable assumption of a simple correspondence between occupational stratification and class structure.)

Thus, investigations of stratification (usually occupational) are generally conducted without the benefit of a proper, larger theoretical framework. This is not to say, of course, that such investigations are entirely useless. They do describe the movements of individuals or groups of individuals through a status hierarchy, the many gradations of which have great salience for most members of industrial society (i.e., jobs and the resources associated with them, chiefly wage/salary income for most people), and they do describe complex patterns of correlation among the occupational and other status hierarchies in the stratification of a population. Descriptions of these sorts have considerable relevance to questions of the internal homogeneity of consciousness and behavior of particular classes. But about questions of why the movements of individuals are as they are, why the patterns of correlation in the stratification of a population are as they are, how those movements and patterns have changed or remained stable, and how they are conditioned by existing class structure—about such questions the investigations of social stratification have had little to say. The kinds of issues that inform these studies—for instance, the relative determinants of access to the differential resources attached to various occupational categories and the patterns of exchange and consumption that follow—fail to penetrate the structural relations of domination-subordination, of appropriation and exploitation, in the control of resource production.

Bertaux's alternative organization of the data on occupational status was undertaken in an attempt to gauge certain features of French class structure. Although he did not measure class structure per se (instead he relied on a trichotomous categorization of gross occupational titles), his conclusion that "French class structure appears to be reproduced from one generation to the next without change in its structural proportions . . ." may well be correct—indeed, may apply equally well to industrial societies other than France. Certainly a high rate of individual movements through the hierarchy of occupational statuses is not by itself necessarily contradictory of such a conclusion, nor is the observation of proportionate expansions and contractions of various regions of the hierarchy. But the study of individual movements through an occupational status hierarchy, no matter how one divides the hierarchy, is not equivalent to the study of class structure. According to our understanding, Bertaux's complaint is that the latter receives so little attention. Assuming he recognizes that his alternative construction of the data on *occupational stratification* is actually no closer to the study of class structure than was our trichotomy (or our eightfold classification), we are quite sympathetic with this aspect of his critique.

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APPENDIX A

FATHER-TO-SON OUTFLOWS AMONG MALES BORN 1918 AND AFTER AND ECONOMICALLY ACTIVE IN 1959 AND 1964 (%)

FATHER'S OCCUPATION	SON'S OCCUPATIONAL POSITION IN 1964											N	%
	1	2	3	4	5	6	7	8	9	10	11		
1. Industriels, gros commerçants	22	18	1	23	9	11	8	8	121,200	2
2. Cadres supérieurs	2	38	5	27	5	11	7	2	...	4	1	134,000	3
3. Professions libérales	2	23	23	27	9	7	3	6	31,600	1
4. Cadres moyens, contremaîtres	1	13	1	35	5	11	17	12	3	1	1	342,000	7
5. Petits commerçants, artisans	3	7	1	13	26	9	23	12	4	1	1	620,000	13
6. Employés	1	7	1	24	7	16	29	11	3	1	...	315,400	6
7. Ouvriers qualifiés	1	2	0	15	5	9	41	18	6	1	1	788,600	16
8. Ouvriers spécialisés, mineurs, pêcheurs	1	1	0	10	4	8	28	39	7	1	1	608,400	12
9. Manœuvres	1	1	...	10	2	10	39	21	16	...	1	243,000	5
10. Exploitants agricoles	0	1	0	5	5	6	9	15	8	43	8	1,400,600	29
11. Salariés agricoles	0	1	...	5	4	5	16	26	17	3	22	275,600	6
Total	2	5	1	13	8	9	22	18	7	13	4	4,880,400	...

SOURCE.—Praderie et al. 1967, annexe 2A. We have rearranged some of the rows and columns of the original table. Category 4 consists of the middle *cadres* and foremen; 7, of skilled workers; 8, of semiskilled workers, miners, and fishermen; 9, of unskilled urban workers; 10, of farmers; 11, of farm laborers. For this 11 X 11 matrix, the aggregate rate of upward status change is 41%, the downward rate is 24%, and the total rate is 65% (comparable rates from our original 8 X 8 matrix are: up, 40%; down, 23%; total, 63%); the index of dissimilarity is 23%.

APPENDIX B

FATHER-TO-SON OUTFLOWS AMONG MALES BORN 1918 AND AFTER AND ECONOMICALLY ACTIVE IN 1959 AND 1964 (%)

SON'S OCCUPATIONAL POSITION IN 1964

	FATHER'S OCCUPATION										N	%
	1	2	3	4	5	6	7	8	9	10		
1. <i>Industriels, gros commerçants</i>	23	21	20	9	12	12	3	0	0	0	210	2
2. <i>Cadres supérieurs, professions libérales</i>	2	41	23	5	17	7	2	0	3	1	330	3
3. <i>Cadres moyens</i>	1	17	36	3	18	11	9	1	2	2	390	4
4. <i>Petits commerçants, artisans</i>	3	8	10	25	14	24	11	3	1	1	1,235	12
5. <i>Employés, services (autres)</i>	1	7	16	5	24	29	13	2	1	0	1,015	10
6. <i>Ouvriers qualifiés, contremaîtres</i>	1	3	10	5	12	44	17	6	1	0	1,830	18
7. <i>Ouvriers spécialisés, mineurs</i>	0	1	7	4	12	30	38	7	1	1	1,195	12
8. <i>Manœuvres</i>	1	1	5	2	13	42	20	16	0	1	475	5
9. <i>Agriculteurs</i>	0	1	3	5	10	11	15	8	41	7	2,780	28
10. <i>Salariés agricoles</i>	0	1	2	4	8	18	25	17	3	21	540	5
Total	1	5	9	7	13	24	17	6	12	4	10,000	...

SOURCE.—Bertaux 1971, table 3; also 1969, p. 480. We have rearranged some of the rows and columns of the original table. Category 6 consists of skilled workers and foremen; 7, of semiskilled workers and miners; 8, of unskilled workers. For this 10 × 10 matrix, the aggregate rate of upward status change is 41%, the downward rate is 24%, and the total rate is 66%; the index of dissimilarity is 23%. Collapsed to an 8 × 8 matrix closely comparable to our original 8 × 8, the aggregate rates of status change (up, 40%; down, 24%) are virtually identical to those produced from our matrix (up, 40%; down, 23%).

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SOME COMMENTS ON RITTER AND HARGENS'S
"TEST OF THE ASYMMETRY HYPOTHESIS"

Ritter and Hargens's discussion of Parsons's asymmetry hypothesis in terms of married working women's occupations and class identifications (*AJS* 85 [January 1975]: 934-48) draws needed attention to some important gaps in much contemporary stratification theory and research. Found widely in the stratification literature are the popular Parsonian assumptions that the nuclear family is the proper unit of stratification analysis, that the family's social status must be unequivocal in order to avoid "psychological insecurity" among its members, and that this unitary status is determined primarily by the occupational status of the male head. These assumptions have recently been subjected to more careful scrutiny, as the authors note, and their own research challenges them as they are found in Parsons's notion of "asymmetry" (1942, 1943, 1953, 1955).

Parsons argues that in order to maintain the stability and solidarity of the isolated nuclear family unit, particularly for urban, middle-class families, the link of the husband and wife to the occupational structure must be asymmetrical. Family solidarity is maintained in that husbands and wives don't face each other as jealous rivals for occupational prestige, and threatening problems of ambiguous family status deriving from more than one occupation in the family are precluded. Further, this differentiation by sex facilitates "specialization" of the wife/mother in the crucially important nurturing, child-rearing, and domestic functions vital to the maintenance of family stability. Ritter and Hargens claim that

their research provides an adequate test of this asymmetry hypothesis. I think that a much more modest claim is in order, owing to a lack of fit between the "theory" in question and their analysis and data.

This problem of fit has two parts. First, the authors' test actually applies directly only to the narrower question of the determinants of individual family members' social status, specifically to the Parsonian proposition that a married woman's social status is determined primarily by her husband's occupation rather than by any ascribed or achieved statuses of her own (this is a consequence of the asymmetry hypothesis, as they themselves note). Second, the authors' choice of the variable "class identification" as an indicator of social status seems clearly inconsistent with what Parsons had in mind as the dependent variable.

The asymmetry hypothesis that Ritter and Hargens identify is not formally stated in Parsons's essays, a situation characteristic of his theoretical writing and one that increases the variety of interpretations as to just what the hypothesis is. A careful reading of the sources suggests that Parsons uses the notion of asymmetry in two contexts, one representing the major thrust of his theory of kinship and stratification in the United States, the other, a narrower version, dealing with what is actually an "exception" to his general argument. The meaning of asymmetry more fundamental to Parsons's discussions is the one given above and cited by Ritter and Hargens: husbands and wives in the urban middle class tend to be quite differently, "asymmetrically," related to the occupational structure in the United States. Specifically, the overwhelming majority of such husbands have full-time occupations, whereas most of the wives are not part of the occupational structure.¹ As a consequence of this asymmetry, married wives of the middle class (who are, by this definition, *not* in the occupational structure) depend primarily on their husbands' occupational prestige and income as the sources of their own and their families' social status. Simple labor-force participation rates for the spouses of these middle-class families, as Parsons himself notes (1955, pp. 14-15), would seem to bear directly on this argument. An empirical test of the hypothesized determinants of a wife's social status would examine the contributions of her husband's occupation relative to any relevant achieved or ascribed statuses of the wife (e.g., education and social status background). Ritter and Hargens do not address these questions inasmuch as their sample includes only wives who work full time.²

¹ See, e.g., the reference to "one" occupational role in the lengthy Parsons quote cited by Ritter and Hargens (1943, pp. 34-35). This emphasis on a single occupational status in the middle-class nuclear family characterizes Parsons's discussion of asymmetry, including that found in the 1955 essay.

² In one sense, middle-class married women who are not part of the occupational structure are perhaps the most taken for granted in stratification analysis; the pre-

The second meaning of "asymmetry" in Parsons's essays apparently refers to occupational prestige differences between urban middle-class husbands and wives in those families where both spouses are in the labor force (this is most unequivocally stated in the 1955 reference, pp. 13-15). Parsons holds that even though these wives work, their jobs will not be "competitive" in prestige with their husbands' jobs. A direct empirical test of this would involve careful examination of the covariation in such husbands' and wives' occupational prestige scores, which Ritter and Hargens do not supply.³

The authors' analysis of the determinants of the status of wives who are employed full time does address an important and much neglected question, but Parsons's theoretical argument in the four essays in question devotes scant attention to this category. The dual occupation pattern in the nuclear family is a "deviant case" of Parsons's general theoretical proposition of asymmetry, and while such patterns must not be ignored, empirical evaluation of them alone cannot be considered a "test" of the more general proposition.

Given Ritter and Hargens's conclusions and Parsons's analysis of the U.S. stratification system (1953), it is even possible to suggest a certain consistency rather than contradiction between them. The primary determinant of social status in the United States is occupation, according to Parsons. Since nonworking married women of the middle class have no achieved status of such primary importance, they must rely on that held by their husbands. However, in the case where both have occupations, Parsons admits the likelihood of a more ambiguous family status. Presumably this is due to there not being one major social status determinant. Consequently, the social status of such a family can be seen as the product of a combination of the prestige of both jobs or, barring that, certainly of the collective income that they yield.⁴ In this context, Ritter and Hargens's conclusion that wives' occupations are almost equally as important as their

vailing ideology is illustrated in the self-characterization, "I don't work, I'm just a housewife."

³ As Ritter and Hargens note, the question of why women are where they are occupationally in the United States is more difficult to unravel, and the issue of institutional discrimination must be part of such an analysis. Nevertheless, the question of where married women are in fact located in the economy is distinct from the issue of why they are so found. Some insight into the covariation of occupational prestige can be gained from Ritter and Hargens's table 1 (p. 939). It suggests slightly different conclusions depending on which of the two white-collar occupational groups is studied, but in neither case are the working wives' jobs overwhelmingly non-"competitive" with those of their husbands. These are the data that would seem to suggest Parsons's second version of "asymmetry" is empirically questionable.

⁴ Parsons unequivocally identifies income as a necessary input for the "life-style" dimension of family status. There is no reason to believe that a family income significantly increased by a working wife yields less potential in this regard.

husbands' occupations in determining the former's status seems, if not directly consistent with Parsons, at least not in conflict with his description of the stratification process in the United States.

The second part of the poor fit plaguing this research centers on the authors' measurement of social status. The variable "class identification" is measured by asking respondents to identify themselves as part of the "middle class" or the "working class," with the justification that "most people" choose one of these two classes when asked that question. The difficulty with this variable in American sociological research is that it has virtually no theoretical identity; that is, no one seems quite sure just what theoretical concept it measures (Schneider 1970). Gross (1953) and Kahl and Davis (1955) have discussed the sizable disparities within a single sample between the responses to such a forced-choice item and those to its open-ended counterpart. Gordon (1958, pp. 197-202) and Rosenberg (1953) have noted the probably contaminating effects of egalitarian cultural values and mass cultural patterns on identifications with "class" in the United States. These are problems of validity.

Even if the measure were completely valid, it would remain inconsistent with Parsons's concept of social status. Although the authors are probably correct in suggesting that the measure taps some dimension of social status, its use seems clearly inappropriate in a theory which defines status as system determined, via the "dominant value system" and people's "reputations." Prestige or status in the Parsonian model is not an individually determined quality and cannot be measured by a self-report item alone; it is not simply "whatever you say it is" (Parsons 1953). The authors attempt to justify the class identification measure on the grounds that the dysfunctions of multiple occupational roles in the family derive from "perceived disparities in social status." But if Ritter and Hargens set out, as they certainly seem to, to test that part of the Parsonian model that deals with the determinants of married women's social status, it is difficult to see how a gross measure of perceived personal prestige (identified to the respondents as "class") could be defined as consistent with that model. As before, the problem is one of poor integration of Parsons's theory and the authors' analysis and data. A measure that would more closely conform to Parsons's notion of "status" would require placement according to consensual evaluations based on others' judgments of the individuals in question.

Neither of these problems is noted here with the intention of suggesting that Ritter and Hargens's research is trivial for stratification theory and research; women have been too often treated as "givens" (especially married women who are not in the occupational structure), and the Parsonian assumptions stated at the outset are much too uncritically used. The argu-

ment I make is that their claim to have provided an adequate test of the asymmetry argument in Parsons seems gratuitous.

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REPLY TO SCHNEIDER

We agree with Schneider that our work does not constitute a definitive test of Parsons's asymmetry hypothesis and certainly did not wish to give the opposite impression in our paper. Contrary to Schneider's evaluation, however, we believe that our results do impugn central elements of the hypothesis.

It is clear from Parsons's discussions that the asymmetry he hypothesizes in men's and women's occupational orientations may take on a variety of manifestations: first, women may have a lower probability of being in the labor force; second, they may be reticent about holding positions which are of higher status than their husbands' positions; and third, they may simply not perceive their own jobs as status resources. It is not clear to us that any one of these manifestations is any more fundamental than the others since all are consequences of the hypothesized asymmetry. While it is true that Parsons places greater emphasis on the first two manifestations in his earlier papers, reference to the third is also present

in assertions to the effect that employed married women have "jobs" rather than "careers" (Parsons 1943, p. 35). The greater emphasis on the third manifestation in the later papers is probably a response to the post-World War II increase in married women's labor-force participation rates—especially given the fact that the greatest expansion took place in white-collar occupations—rather than an attempt by Parsons to flesh out his argument with "deviant cases" and "exceptions" (these are Schneider's terms, not Parsons's).

We pointed out in our paper that tests of the second manifestation of the alleged asymmetry in men's and women's occupational orientations cannot be made in the absence of a measure of occupational discrimination against women, and the same difficulty confronts the use of labor-force participation rates² to test the first manifestation. Thus, it seemed to us that the most feasible way to test the asymmetry hypothesis was to examine the third manifestation, and this is what we attempted in our paper. Obviously, tests of the first two have yet to be carried out, but we fail to see how this renders our work less than adequate.

According to Schneider, Parsons's analysis of the relation of the nuclear family to the U.S. stratification system is consistent with the idea that family status is determined jointly by the husband's and wife's occupations when both are in the labor force. Given Parsons's frequent statements that a family's status in the stratification system is based on only one occupational status, that of the husband-father,¹ we believe that Schneider is in error on this point. As a result, we believe that Schneider's attempt to show a consistency between our findings and Parsons's theoretical statements is unsuccessful because it ignores the fundamental importance in Parsons's work of the third manifestation of the asymmetry hypothesis. It is only by leaving this element out of his account of Parsons's theory that Schneider is able to claim that the theory is consistent with our results.

In our paper we argued that, if, as Parsons claims, it is only the husband's occupational position that determines the common status of the members of a nuclear family, there should be no residual association between the occupational statuses of working wives and their perceptions of their own social statuses, net of their husbands' occupational statuses. Schneider argues that our use of a measure of class identifications constitutes a major flaw in our work—first, because measures of status self-perceptions are incommensurate with Parsons's concept of social status and, second, because the class identification measure is of questionable validity. On the first point it should be noted that Schneider is mistaken

¹ Recent empirical work (Rossi et al. 1974; Sampson and Rossi 1975) suggests that this element of Parson's theory of the relationship of the nuclear family to the U.S. stratification system is incorrect.

in his belief that we were attempting "to test that part of the Parsonian model that deals with the determinants of married women's social status." We were instead examining the hypothesis that married women do not see their occupations as status resources. According to Parsons, this prevents the emergence of status rivalry between husband and wife. It seems to us that a test of this hypothesis requires the examination of working wives' self-perceptions of status and not others' attributions of status to them.

As far as the second point is concerned, although we disagree with Schneider's claim that the usual measures of class identification are without theoretical identity, the real issue here concerns the reliability and validity of the measure we used. The problem of variation in the marginal distributions of responses to questions about class identification is less important for this issue than the strength of associations between our measure of identifications and other measures of social status. Kahl and Davis (1955) show that the forced-choice class identification measure has substantial associations with a number of such measures, and they present a factor analysis which shows this class identification measure has a substantial loading on a factor which includes, among other variables, the Warner and North-Hatt occupational ratings and interviewers' ratings of respondents' social statuses. These results seem inconsistent with Schneider's argument that the class identification measure is of low reliability and validity, and we believe that he underestimates the usefulness of the measure.

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ECONOMY, POLITY, AND MONOTHEISM:
REPLY TO SWANSON

Swanson (*AJS* 80 [January 1975]: 862-69) has criticized my paper, "Economic and Political Antecedents of Monotheism" (*AJS* 80 [January 1975]: 841-61), on both conceptual and empirical grounds. His major conceptual criticism is based on a misreading of my central argument. I did not propose that economic complexity can account for the relations of political complexity to monotheism. Nor did I assume that "religious beliefs directly reflect the 'material' aspects of a society's economy" (p. 862). Rather, I followed Marx and Engels's view that religious beliefs reflect social relationships and relationships of people to the economic process. I said, ". . . monotheism appears as the reflection of a stage in social evolution. The ultimate basis for such evolutionary change is the nature of the production process. As the economic system becomes more complex, monotheism appears. However, . . . the relationship is not necessarily direct. While production is the *ultimate* determining force in history, it is not . . . the *only* force. . . . Other aspects of social organization including political organization are important as well" (p. 846).

Durkheim's theory has important similarities to that of Marx and Engels—a fact Durkheim himself recognized—but differs in that Durkheim did not emphasize the ultimate importance of material production with respect to religion.

My argument is not that economics and technology tell the whole story, but that they tell a part of the story which should not be neglected. In empirical terms, the point is not that economic complexity explains the correlation of political complexity with monotheism but that economic complexity is at least as important as political complexity in producing monotheism. The results of my study contradicted the theoretical perspective of Swanson and Durkheim in one important respect. "The effects of economic complexity on monotheism are not explained through the intervening mechanism of political organization" (p. 859). Political complexity is, nonetheless, an important factor. In this respect my results supported Durkheim and Swanson.

Apart from this central misunderstanding, Swanson's main criticisms seem to be: (1) I am wrong to think that his concept of sovereign organizations concerns the complexity of a society's political organization; (2) the measure of political complexity used in my study is less valid for predicting monotheism than is his original measure of sovereign organizations. Swanson claims to show this by evidence that the relationships of political complexity and economic complexity to monotheism are explained by sovereign organizations in two samples.

Regarding sovereign organizations and political complexity, it is true that I view Swanson's measure as an indicator of political complexity. The number of hierarchically arranged sovereign groups in a society seems to me to be similar though not identical to the number of levels of administration extending beyond the local community. Sovereign organizations are ongoing groups with autonomous decision-making power in some area of social affairs. Swanson counts the number of levels in a hierarchical structure of decision-making groups. His coding instructions specify that the family and local community are always counted as sovereign organizations. Because the family and local community are virtually universal, almost all societies have two or more sovereign organizations, and the family and local community contribute no variation to the measure. Thus, most of the variation in sovereign organizations comes from levels of administration extending hierarchically beyond local communities. Both Swanson's variable and mine reflect the political structure of societies. Empirically, Swanson's measure of sovereign organizations is correlated at .49 with political complexity in his original sample and at .72 in the second sample reported in his criticism of my study. These figures compare favorably with the intercoder reliability of .66 for sovereign organizations reported by Swanson (1960, p. 224) in his original study.

It seems to me that Durkheim also explains the origin of high gods in political terms. Durkheim argued that high gods grew out of clan totems and united the various clans into a tribe. Thus the high god represents the social integration of the tribe in its political aspects.

Regarding the comparative validity of my measure of political complexity and Swanson's measure of sovereign organizations for predicting monotheism, Swanson seriously misinterprets the results obtained in his two samples. He argues that differences in coding between the sovereign organizations and political complexity measures lead to differences in results using them. This is not true. As I will show, differences in results in Swanson's samples are due, not to coding differences, but to the manner in which the samples are selected.

The important issue here is whether sovereign organizations (or political complexity) can explain the correlation between economic complexity and monotheism. In the first of Swanson's two small samples the correlation between economic complexity and monotheism goes down from .33 to .24 with sovereign organizations held constant. In his second sample this relationship is reduced from .12 to .09 with sovereign organizations held constant. In my data, using the *Ethnographic Atlas* (Murdock 1967), the correlation between economic complexity and monotheism is reduced from .43 to .32 when political complexity is controlled. These differences in partial correlations among the three studies are not due to the use of sovereign organizations as a measure instead of political complexity. The

partials differ principally because the zero-order correlations between economic complexity and monotheism differ. One can substitute the correlation of .43 between economic complexity and monotheism from the *Ethnographic Atlas* into the formula for the partial correlation in Swanson's two samples. This gives a partial of .36 instead of the .24 obtained in his sample 1 and a partial of .41 instead of the .09 obtained in his sample 2. These hypothetical calculations give results which compare fairly well with the partial of .32 obtained in the *Ethnographic Atlas* when political complexity is controlled.

The problem, then, is why economic complexity is less strongly related to monotheism in Swanson's samples than in the *Ethnographic Atlas*. Reassurance that the *Atlas* result is not wide of the mark can be had from Murdock's World Ethnographic Sample (WES [Murdock 1957]), which gives a result ($r = .40$) very close to that in the *Atlas*. Swanson's two small samples give results which are discrepant not only from the *Atlas* and the WES but also from one another.

It should also be noted that Swanson's samples are not independent of the *Atlas* but are subsamples from it. These subsamples are not, however, random but are instead seriously biased. In his first sample "anthropological experts" eliminated from the universe societies for which information on "the problem of these studies" was unlikely to be available and societies converted to one of the great world religions. In this case instances of monotheism (and associated economic complexity) were systematically excluded. Swanson's second sample is "a world-wide sample of pastoral and horticultural peoples" (p. 868). In this case instances of economic complexity (and associated monotheism) were systematically excluded.

Table 1 documents these sample biases. The first row shows a lower incidence of economic complexity in Swanson's samples than in the *Atlas* or WES. The second row shows a lower incidence of monotheism in Swanson's samples. The third row shows a lower joint occurrence of the two

TABLE 1
ECONOMIC COMPLEXITY AND MONOTHEISM IN FOUR SAMPLES

	SAMPLE			
	<i>Ethnographic Atlas</i>	WES	Swanson 1	Swanson 2
Percentage high in economic complexity*	57	56	45	46
Percentage with a high god*	64	65	45	53
Percentage high in economic complexity and with a high god (joint occurrence)*	47	47	29	27
$r(\phi)$, economic complexity by high god43	.40	.33	.12

* Base $N = 100\%$ is, in each case, the table total.

variables in Swanson's samples, and the final row shows the lowered correlations which result.

In conclusion, Swanson's criticism of my study contains a misunderstanding of my central argument and a serious misinterpretation of statistical data. His contention that my "predictions fail" when "put to a direct comparison in two samples" (p. 869) is incorrect. Swanson's samples show lower correlations between economic complexity and monotheism than those in the *Ethnographic Atlas*. This is due to systematic biases in the manner in which he selected his samples.

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COMMENT ON UNDERHILL'S REPLY

I think that I understood Underhill's argument and reported it with reasonable accuracy, but interested readers will judge that for themselves. Of greater importance are the empirical questions at issue. I found in two samples of primitive and ancient societies that there was no significant correlation between a belief in monotheism and Underhill's indexes of economic and political complexity if one held constant the number of types of sovereign groups in those societies. I also found that the number of sovereign groups was significantly correlated with monotheism even when his indexes were held constant.

Underhill now asks whether his indexes might have a significant and independent relationship to monotheism were one to include in one's samples societies of greater complexity than those I selected. Specifically, he suggests that a sample having the range of complexity found in the *Ethnographic Atlas* (Murdock 1967) is desirable and says that my samples have a lesser range of variation. I have checked the distribution in my samples of scores on his two indexes as against their distributions for societies in the *Ethnographic Atlas* (employing a sample, sample 3, consisting of every fourth society listed in the *Atlas*). A goodness-of-fit test (Hays 1963, pp. 580-84) shows that the second of my samples, sample 2

(Swanson 1969), does differ from societies in the *Atlas* as Underhill proposes ($\chi^2 = 19.52$, $df = 4$, $P < .001$) but that my first sample, sample 1 (Swanson 1960), does not ($\chi^2 = 6.63$, $df = 4$, $P < .20$). There are no differences between my first two samples and sample 3 with respect to his index of political complexity: sample 1 versus 3: $\chi^2 = 1.11$, $df = 4$, $P < .80$; sample 2 versus 3: $\chi^2 = 0.82$, $df = 4$, $P < .98$.

I have also drawn a new sample (sample 4) from samples 1 and 2, designed both to maximize the number of societies having scores at the highest levels on Underhill's indexes and to correspond to the distribution of scores on economic complexity that is found in sample 3. Within these constraints, I employed random means to obtain sample 4. (The goodness-of-fit χ^2 between the distributions of scores on samples 3 and 4 is 0.41, $df = 4$, $P < .99$.) The number of cases in sample 4 is small ($N = 31$), and the results are therefore only suggestive, but they do alert us to considerations which may be important in the design of further research. The partial correlation of monotheism with number of sovereign groups in sample 4, with economic complexity controlled, is .49 ($t = 2.96$, $df = 28$, $P < .01$); with political complexity controlled it is .55 ($t = 3.49$, $P < .01$). By contrast, the partial correlation between economic complexity and monotheism with number of sovereign groups controlled is a nonsignificant .31 ($t = 1.73$, $P < .10$) as is that between political complexity and monotheism when such a control is applied ($r_{14.2} = -.31$).

We find, then, that in two samples (sample 1 and sample 4) which do not differ significantly in their distribution of Underhill's indexes from that obtained from societies in the *Ethnographic Atlas*, his indexes have no significant correlation with monotheism when number of sovereign groups is controlled. On the other hand, the number of such groups is significantly related to monotheism when Underhill's indexes are held constant. These findings run counter to his expectations.

Suppose, however, that he is correct in thinking that his indexes have a significant and independent relationship to monotheism for at least the most complex societies in the *Ethnographic Atlas*. This remains to be shown, but it would still leave us with the result that those indexes do not have such a relationship to variations in monotheism among the bulk of primitive societies, whereas number of sovereign groups does. And number of sovereign groups may also have such a relationship among societies that are still more complex.

When further research is done, I would urge not only that it employ Underhill's and my operational definitions exactly as we describe them but also that it not overlook the possible usefulness of the number of jurisdictional levels within the local community as a further measure of political complexity. Contrary to Underhill's statements, there is considerable variation among societies in the number of such levels, the standard devia-

tions in samples 1, 2, and 3 all being significantly different from zero at a point well beyond the .0001 level of probability.

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ETHNICITY AND PARTICIPATION: A COMMENTARY

Growing attention is being devoted to the theoretically interesting phenomenon of categorical differences in social and political participation, much of the attention centering on the differential involvement of blacks and whites and how such differences can be best explained (Orum 1966; Olsen 1970; Williams, Babchuk, and Johnson 1973). A welcome addition to this accumulating body of research is the study by George Antunes and Charles Gaitz ("Ethnicity and Participation: A Study of Mexican-Americans, Blacks, and Whites," *AJS* 80 [March 1975]: 1192-1211). Focusing in particular on the participation patterns of Mexican-Americans and the applicability of the compensation and ethnic community (identification) hypotheses in explaining these patterns vis-à-vis those of other groups, Antunes and Gaitz find, contrary to their expectations, that unlike blacks Mexican-Americans participate less than the white majority. This finding leads them to the conclusion that the compensation and ethnic community hypotheses "as . . . currently constituted must be rejected" (p. 1202).

We contend that rejection of the explanations on the basis of the evidence presented is premature, for as commendable as is their attempt to apply these hypotheses, Antunes and Gaitz fail to do so.¹ They do not seem to realize that the explanations as they are "currently constituted"

¹ Our concern is mainly with the authors' interpretation of the hypotheses and what they consider to be an adequate test of the explanations. Methodologically inclined readers should consider also their operational measures, especially the one having to do with ethnic identification (see n. 10, pp. 1203-4, and the discussion that follows on p. 1204).

are *ex post facto* interpretations of sample-specific findings. The explanations have never been fully explicated or directly tested, regardless of what Antunes and Gaitz think they may have done. In our judgment, they could have made a greater contribution had they not failed to recognize this and had they more fully appreciated the complexity of the explanations, the intricacies of social participation, and the fact that not all minority groups are necessarily alike. For example, their suggestion concerning the introduction of the terms "public" and "private" participation indicates a lack of acquaintance with the substantial literature in this area and with the generally accepted and widely used distinction between "formal" and "informal" involvement (see Edwards and Booth 1973). Furthermore, had Antunes and Gaitz been aware of the already documented differences in participation among Jews, Mexican-Americans, blacks, and other select ethnic groups which, on the face of it, all have high levels of ethnic identification, they would not have naively predicted that the patterns displayed by Mexican-Americans would be similar to those of blacks.

Essentially, their analysis, with the exception of the interesting findings on perceived social distance, simply confirms the patterns of participation previously noted by Williams et al. (1973) and some aspects of the studies by Orum (1966) and Olsen (1970). While such confirmation has definite value, what we desperately need at this point are not more empirical findings but better theory and less cavalier rejection of existing interpretation. If Antunes and Gaitz had directed their considerable efforts to the actual testing of theory and not to another demonstration of what has been largely established, dressed up with a simplistic interpretation of the hypotheses, they might have more fully comprehended that a social psychology of affiliation underlies the process of participating and becoming politically involved. To their credit, a social psychological process accounting for differential involvement is implied in the alternative interpretations they consider, but Antunes and Gaitz do not pursue or develop the possibility. In our opinion, explication of the process is necessary if we are ever to test directly the various explanations of participation.

As we have argued elsewhere (Klobus and Edwards 1974), the compensation hypothesis posits, at the level of the individual, a need on the part of self-defined minorities for ego reinforcement, reinforcement which must be derived from personal achievement in that it may not be otherwise obtainable in an ethnically segmented society; in short, it suggests that participation is a manifestation of "egoistic" concerns. The ethnic community hypothesis, on the other hand, postulates an "altruistic" orientation. High social involvement is viewed as the result of strong identification among individuals who perceive that, through active participation, common collective interests may be attained.

Both explanations, as Antunes and Gaitz correctly suggest, predict high

levels of participation. But the orientations toward affiliation are quite different. One who manifests compensatory behavior emphasizes personal goals necessary to counteract a perceived stress on the ego. Participation serves to anchor the self within the environment, as opposed to placing on society the responsibility for any perceived social inconsistencies. The compensation orientation thus entails a positive evaluation of personal competence, while maintaining accountability for one's conditions on a personal rather than a societal level. In contrast, the awareness of defective or unjust relationships between collectivities can produce a predisposition toward fulfilling subsocietal needs instead of intrapersonal ones. Collective action, via social participation, constitutes one means to fulfill these needs. Such an orientation, implied in the ethnic community hypothesis, presupposes both a high level of personal competence and a tendency to blame the system for any perceived systematic incongruencies. An altruistic person believes that he or she has something worthwhile to offer and that the root of the condition which he or she seeks to redress, regardless of its nature, lies within the social system. When these orientations are operationalized in terms of their social psychological dimensions—self-efficacy and system blaming—and directly tested, as in Edwards, Klobus, and Klemmack (1975), both of the explanations considered by Antunes and Gaitz prove viable.

It should be noted that Antunes and Gaitz fail altogether to consider the applicability of the isolation hypothesis to their findings (Wright and Hyman 1958; Hyman and Wright 1971); this widely current view could account for the patterns they observe among Mexican-Americans. Explication of the isolation hypothesis suggests two analytically distinct orientations, anomie and self-estrangement. Anomie not only involves a perceived incongruity between what is expected and actually received from society and the attribution of blame for such an incongruity to the system itself but also incorporates the belief that any such circumstances cannot be altered through personal or collective efforts. Anomic isolation thus implies a low sense of self-efficacy coupled with a high level of system blaming. Self-estrangement, a second form of isolation, involves a similar incongruity between personal capabilities and societal conditions, but the resulting alienation is directed toward the self, not society. Taken together, the orientations we term egoistic, altruistic, anomic, and self-estranged allow a four-fold typology which can be operationalized in terms of self-efficacy and system blaming and directly tested (Edwards et al. 1975).

An inspection of the accompanying table shows that blacks who are characterized as "compensators" (high self-efficacy/low system blaming) and "ethnic identifiers" (high self-efficacy/high system blaming) consistently have higher mean rates of participation than those characterized by isolation (low self-efficacy/low system blaming, low self-efficacy/high

TABLE 1

MEAN RATES OF BLACK PARTICIPATION IN FIVE MODES OF SOCIAL INVOLVEMENT,
ADJUSTED FOR OCCUPATIONAL PRESTIGE AND AGE

	Ethnic Community (HSE/HSB)	Compensation (HSE/LSB)	Anomic Isolation (LSE/HSB)	Self- Estranged Isolation (LSE/LSB)
Informal involvement	27.92	27.81	25.30	23.89
Organizational membership	2.33	1.28	0.96	0.99
Organizational involvement	6.99	3.55	2.66	2.81
Religious involvement	5.79	5.36	4.45	5.28
Political involvement	6.27	6.62	5.30	4.40

NOTE.—*N* = 97, from a multistage random sample of blacks living in Roanoke, Virginia. Age and sex quotas were incorporated into the sample design, and ethnic homogeneity between interviewer and respondent was maintained.

system blaming). Except in one instance, that pertaining to political involvement, "ethnic identifiers" participate to a greater extent than blacks characterized by compensation. The differences in rates displayed by the two, though generally small, show emphatically that it is misleading to lump together the compensation and ethnic community hypotheses as Antunes and Gaitz have done. Moreover, in a more extensive analysis (not shown here), where blacks and whites are compared, we have found that ethnicity by itself has a significant influence on only two types of involvement, informal participation and religious activity. Self-efficacy, as the single most crucial variable, best accounts for the differentials in participation.

With the explanations of participation reconceptualized in the manner indicated, our findings suggest that comparisons of patterns can be made not only across ethnic groups but within such groups as well. In the typical intergroup comparison, if 60% of the blacks of a given community are altruistic (ethnic community) while 60% of the Mexican-Americans or whites are anomic or self-estranged (isolation), it follows that rates of participation among blacks will exceed those of Mexican-Americans and whites. This is not to say there are no altruistic Mexican-Americans or whites; it only suggests the possibility that there are proportionally fewer of these individuals than exist within the black population. Assuming a differential distribution of these social psychological orientations toward participation, we conclude that, had the sample used by Antunes and Gaitz been representative of the community, their findings merely would have indicated that a larger proportion of Mexican-Americans are anomic or self-estranged and, as a result, participate to a lesser extent than either blacks or whites. We submit that, had Antunes and Gaitz actually focused on the theories themselves, explicating the explanations, and then attempted to test them directly, the compensation and ethnic identification

explanations of ethnic participation patterns could not have been so easily rejected.

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Individual Rights and the State: A Review Essay

Anarchy, State, and Utopia. By Robert Nozick. New York: Basic Books, 1974. Pp. xvi+367. \$12.95.

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In an earlier issue of this *Journal*, I examined a recent work in moral philosophy, *A Theory of Justice* by John Rawls (Coleman 1974a). My aim in doing so was not merely to bring into the sociological discourse on equality and inequality a perspective from moral philosophy. The goal was greater than that. I hoped by so doing to help in the reorientation of sociology to certain important macrosociological problems, those having to do with the processes through which a society governs itself. Central to these problems is that of maintaining the delicate balance between the individual and society, between rights individually held and rights held collectively at the level of the state. Closely related are problems of balancing liberty and equality, or justice and individual autonomy.

The sociological issues involved in maintaining the balance between these partly incompatible ideals (a balance which must be capable of changing temporarily when the balance between individual problems and collective problems changes, without losing the capability of returning to its earlier state) are often neglected within the discipline, in favor of questions more easily addressed by research. The neglect is understandable because of the difficulty of the problems; but it is no less unfortunate for that reason. The passing of the tradition of the grand theorists has left a vacuum in certain central regions of sociology, and it is important to find ways in which that vacuum can be filled.

Recently there has been another event of similar proportions to the publication of Rawls's book.¹ This is the publication of *Anarchy, State, and Utopia* by Robert Nozick. The publication of this book provides an opportunity to examine these same issues from another perspective. In

¹ The reawakening in moral philosophy was proceeding somewhat before the appearance of Rawls's book. For example, in 1956, Laslett published his *Philosophy, Politics, and Society: First Series*, opening with an introduction commenting upon the moribund state of political philosophy. By the *Second Series* (1962) he was able to remark upon surprising signs of life, and by the *Fourth Series* (1972) the revitalization was so secure that the introduction reflected upon how it could have been so recently thought dead. It is interesting to note that Rawls's "Justice as Fairness," which spelled out an early formulation of the theory published in book form in 1971, was one of the papers in the *Second Series*.

this essay, I will continue the attempt I began in the earlier paper, using a detailed examination of Nozick's book to refocus attention on these problems which, though central to the discipline's mission, have languished with minimal attention.

Nozick is a natural-rights theorist, beginning his book with these sentences: "Individuals have rights, and there are things no person or group may do to them (without violating their rights). So strong and far-reaching are these rights that they raise the question of what, if anything, the state and its officials may do." The book, then, is an attempt to answer this question, taking as given a set of rational, self-interested persons who possess individual rights in the "state of nature" and are potentially engaged in a Hobbesian war of all against all.

Nozick's solution is in three parts, to which correspond the three parts of his book. In part 1, he asks how the voluntary assent of rational individuals can lead to something resembling a state instead of continued anarchy. Thus the first part is an argument against the anarchists, and Nozick shows the logical steps which lead in a given territory to a "dominant protective association" with the essential character of the state—a monopoly over the use of coercion or violence. This is his "minimal state"; part 1 shows how it must arise through rational voluntary choice. In part 2, Nozick shows how nothing beyond such a minimal state can arise, without violating individual rights. In particular, he shows how any form of redistribution in which the state engages violates the rights of those from whom anything is taken. And in part 3, he shows how the combination of his "minimal state" with communities, possibly coercive and nonminimal ones, can constitute a utopian ideal.

In these three parts, Nozick's strongest aim is clearly not to defeat the anarchists by showing that a state will arise without violating individual rights, nor to convince the reader that his utopia is the proper ideal, but to show that a nonminimal state does violate individual rights and therefore is unjust. Nozick is principally concerned to address the issue set forth by Rawls, that is, what is a just society (or state, or set of institutions)? And the result he arrives at is very different from that of Rawls.

Before turning to specific comments on part 2 of the book (and I will restrict my comments to this part, where Nozick's own greatest interest lies), it is useful to ask what it is that gives Nozick's book such an immediate and large reputation. It is not a work like that of Rawls, containing a well-developed theory. It is most successful in its counterarguments and counterexamples, least so in its own theoretical proposals. It is rough at the edges, unfinished.

I believe the instant attention this book has received is stimulated by Nozick's direct confrontation with recent tradition in the social sciences. With the single exception of the intellectual thrust of classical economics,

which is libertarian, recent social science has been (and current social scientists have been) largely statist, interventionist on behalf of the social underdog, concerned with redistribution, and more interested in equality than in liberty. Rawls reflects this in his *Theory of Justice*. But Nozick challenges all this—not quite single-handedly, of course—but, for the first time in recent years, from first principles and at the most fundamental level. I believe it is this challenge to received tradition which, more than anything else, generates the attention Nozick has received. He, of course, recognizes this. He says, in his preface, “I do not welcome the fact that most people I know and respect disagree with me” (p. x).

Now let us turn to part 2 of his book, where Nozick presents the position and its supporting arguments that lead most people he knows and respects to disagree with him. Here he presents arguments opposing any state beyond the “minimal state.” But the central and longest portion consists of a direct confrontation with the ideas of his colleague John Rawls. Rawls’s theory of justice is a theory of distributive justice, and Nozick begins the confrontation with Rawls by a discussion of theories of distributive justice in general, and Rawls’s own theory.

As he points out immediately, the very presentation of the problem of justice in the distribution of individuals’ “holdings” (Nozick’s frequently used term) as one of “distributive justice” biases the issue. The terminology suggests a central agency distributing collectively held resources to individuals. Rawls partakes of this conception, as Nozick points out (p. 228), viewing even individuals’ natural abilities as a collective asset, one to be used for the good of the least advantaged. Nozick argues that a proper theory of justice in holdings, however, is a theory of entitlements, consisting of three parts (p. 151): (1) Justice in acquisition of holdings: “A person who acquires a holding in accordance with the principle of justice in acquisition is entitled to that holding.” (2) Justice in transfer of holdings: “A person who acquires a holding in accordance with the principle of justice in transfer, from someone else entitled to the holding, is entitled to the holding.” (3) Rectification of injustice: “No one is entitled to a holding except by (repeated) applications of 1 and 2.” If someone has a holding obtained directly or at some earlier point in a way other than 1 or 2, then a rectification of the injustice is necessary, to attempt to recreate the situation which would have occurred under just acquisition and transfer.

Nozick leaves unstated precisely what are just acquisition and just transfer, or even what are good candidates for justice in acquisition and transfer. I will return to this later. But even without specific content, the principles sharply contrast to most theories of distributive justice. For according to Nozick’s principles, justice inheres in the *process by which holdings are obtained* and is content with whatever distribution results from that pro-

cess. In contrast, most theories of distributive justice, including Rawls's as well as the utilitarians', ignore the process by which holdings are obtained, and attend only to the end distribution. Nozick calls these "end-result" theories and contrasts them to "historical" theories (what I have termed *process* above), of which his entitlement principles constitute an example, as does Locke's theory of property.

Nozick points out that any end-result conception of distributive justice, if embodied in law, gives each citizen an enforceable claim to some portion of the total social product, regardless of who currently holds that product or how they came to hold it. This right, as he argues, is equivalent to appropriating the actions of other persons, and therefore, equivalent to acquiring "part-ownership" of other citizens. Thus Nozick presents a dismal picture of the position of a citizen in the modern redistributive state, that of partial slave to a many-headed master, the state.

It is in his refutation of end-result theories of distributive justice that Nozick is at his best. He shows that any particular system of distribution of holdings (such as distribution of income) based on end-result theory can be immediately upset by the exercise of choice, not only by those who are well-off in the distribution but also by those who are worse off. An illustration is his Wilt Chamberlain example: If Chamberlain contracts with the owner of his team to get 25 cents for each home-game ticket sold, and there are a million customers, he ends up with \$250,000 for the year; this will then destroy any pattern of holdings that limits inequality, unless a large portion of the \$250,000 is expropriated from Chamberlain and redistributed to persons like those who freely chose to give him their quarters. But is not Chamberlain entitled to this income? Each of a million persons chose to spend his money on Chamberlain. Is this unjust? Nozick argues that it is not unjust, because the income was acquired through a just process. Most end-result theories of distributive justice would argue that it is unjust, because this income is so much greater than the average when, in fact, some have not enough to eat. The example, of course, is only a special case of the very simple point that any operation of a market process, with exercise of free choice, is quite capable of producing, from methods of distribution that appear "just," distributions of holdings that will appear "unjust," if justice is measured by the inequality of holdings. Thus there are only two avenues open: to restrict the free choices of persons in their transactions, either by direct constraints or by partial expropriation of the result, or to refuse to measure the "justice" of a distribution of holdings in this way. Nozick chooses this second path, arguing that the first constitutes a violation of individual rights of the most fundamental sort: expropriation by others of a portion of oneself.

Although Rawls's theory of distributive justice does not specify a particular distribution of holdings, it does, as Nozick shows, entail expropria-

tion by the state of the product of the best off for the benefit of the least well-off. There is no incentive, according to Nozick, for the well-off to participate in a "Rawls-just" society.

Rawls would argue that this is of no import, because rational persons, from the "original position" in which they are ignorant of whether they are to be well- or badly off in the future, would choose a set of institutions that attended only to their welfare when they were worst off. They would, in game-theoretic terms, choose according to a minimax principle. Rawls would argue that there is no violation of rights at all in his just society, since in the original position, each person freely chose to be governed by that set of institutions—that is, chose to have a portion of his self expropriated if he becomes well-off in return for a similar claim on others if he becomes one of the worst off.

To counter this argument, Nozick must attack directly Rawls's notion of the form the social contract takes from the original position of ignorance, and he does so (pp. 198 ff.). But here he is less successful, and this lack is the key to the major deficiency in Nozick—a deficiency which, as we will see, is great indeed.

First, Nozick argues that "a procedure that founds principles of distributive justice on what rational persons who know nothing about themselves or their histories would agree to *guarantees that end-state principles of justice will be taken as fundamental*" (p. 198, emphasis in original). He further tries to imagine the probability calculation that could lead rational persons to arrive at some historical-entitlement principle, and concludes that "their calculations will not lead them to select the entitlement principle" (p. 201).

But these conclusions stem from a too-narrow view of what kinds of choices rational persons can make in the original position, that is, behind a veil of ignorance. Nozick's view might have been broadened by some attention to the way actual persons have acted in creating constitutions (which is the closest real-world approximation to a social contract from an original position). Rawls made the same error (see Coleman 1974a, 1976) when he concluded that rational persons would choose his two principles by which to be governed.

Nozick's narrow view of the choices available to persons in the original position is evidenced by an example he introduces to buttress his argument that the entitlement principles would not be used by persons in the original position. The example is this: There is "a group of students who have studied during the year, taken examinations, and received grades between 0 and 100 which they have not yet learned of. They are now gathered together, having no idea of the grade any one of them has received, and *they are asked to allocate grades among themselves* so that the grades

total to a given sum (which is determined by the sum of the grades they actually have received from the teacher)" (p. 199, emphasis added).

Now we will disregard the fact that to conform to the "original position" model, the choice should be made at the outset of the course, rather than after examination. The crucial point is that Nozick has a narrow choice forced upon the students. They have no opportunity to choose not to have collective authority over the grades of each. They are not given the chance to decide how to allocate the rights which, taken together, determine grades. If they were, it is quite likely that they would agree to something like this: that each should have individually the right to show what he had learned, through some performance at the end of the course, and that the right to attach scores to these performances should be held by an outsider, such as the teacher or another observer.²

Nozick has forced the conclusion which he wants: in his example he has given the students collective authority without their individual assent. Thus he would, just like the distributive justice theorists whose work he correctly dismantles, take away individual rights rather than allow a social contract to determine what rights will be individually retained and how the other rights will be allocated.

Both Rawls and Nozick assume that rational persons behind a veil of ignorance will choose how the social product (or individual products, socially expropriated) is to be divided up (for Nozick, see pp. 198 ff., especially the footnote on p. 199; for Rawls, see [1971], p. 12). But why should that be? Is there not a prior question which, when decided, will dictate for these rational persons what distributions of holdings will result? That prior question, of course, is the fundamental question of the social contract: what rights will people agree to give up to the collectivity (and what mechanisms of representation, etc., will they use to help insure that those rights are used in some correspondence to the aggregation of their individual wills), and what rights will they reserve to themselves as individuals? Now some contract theorists, like Rousseau, have specified that all rights are to be held collectively, none individually, and it is this that makes Rousseau in effect the father of the modern totalitarian state.³ But others, like Locke, are very careful about specifying the rights to be held collectively.

If rational persons in the original position behind a veil of ignorance

² I will not here go into the question of just why this would be rational except to observe that if a student has an interest not only in the grade but in learning something, he will attempt to choose an allocation of rights that will increase his learning. Allocation of rights of performance to himself individually, on the basis of which performance an outside observer will judge, would constitute an incentive to learn.

³ See Talmon (1952) for a development of this point.

act as I have suggested, deciding which rights to hold collectively and which to retain for individual use (and this is close to what we find actual persons do in the construction of constitutions: they do *not* decide how the social product is to be divided up), then it is quite likely that something with a structure close to that of Nozick's entitlement theory will result. Why do I say that rational persons would act in this way? Because their very decision to come together to form a compact is a decision by each that he will be better off by pooling certain of his rights and resources with those of others, to be used collectively (e.g., for common defense) than by reserving them all to himself for individual use. Thus he must decide which of his rights it is in his interest to give over to collective use (and with what institutional safeguards), and which of his rights it is in his interest to reserve for individual use.⁴ This decision depends on the expected distribution of events of two kinds. First are events in which the added benefits to him of joint use of resources outweigh the potential cost of the resources being used in a direction that is against his will; second are events for which this is not so. Events of the first type are better faced with pooled rights and resources, those of the second, with individually held ones (see Coleman 1971, 1970a, 1973). Clearly, such a decision can be made rationally, if one has an expected distribution of these two kinds of events and the expected consequences of each event for him. Roughly, the more events of the first kind one expects, and the more consequential each is expected to be, the more rights and resources he will give up to collective use.

Now once a set of rational persons behind a veil of ignorance has made such a social contract, such a constitutional choice, they are precluded by that decision itself from making further choices of the sort that both Rawls and Nozick assume, that is, choices about how the "social product" is to be divided up. If, in the set of rights reserved for individual use, there is full entitlement to the product of one's labor (Nozick's justice in acquisition) and full entitlement to the holdings one receives from the voluntary actions of others (as in a gift or exchange), then Nozick's entitlement theory follows directly. Only if full rights to all resources are given up in the original position (as Rousseau assumes explicitly, and Rawls, implicitly) is it possible to proceed as Rawls does, or as does any theorist of distribution justice who specifies a particular end-result distribution. So Nozick is right that Rawls's difference principle would not result from

⁴ A somewhat more sophisticated social contract will also specify a process by which, as events confronting fellow-contractors change, they can change their allocation of rights between individual and collective use (not merely to increase the latter, but to decrease it as well, when desirable to do so). Most political systems and the constitutions that underlie them are very poor in their capacity for such change. See Coleman (1966, 1970b, 1974a, 1976), for suggestions of more flexible systems, including the "multiple-contract theory" (discussed in the last two citations).

rational persons behind a veil of ignorance—but he is quite wrong that no principle of distributive justice that specified the process, or how those holdings came to be (such as his entitlement theory), could be chosen. Indeed, it is *only* such principles that could be chosen, because rational persons in the original position would specify an allocation of their rights between individual and collective use—and this allocation would determine what individuals were entitled to do qua individuals, and what the central collective was entitled to do to modify the results of those individual actions. We may in fact press Nozick a little more directly, and ask just how he would expect his entitlement principles to arise in a society. Even though we begin with individual rights, the entitlement principles must be jointly enacted, or socially agreed to be *legitimate*; and how does that legitimacy come about? Through something like a contract among rational persons behind a veil of ignorance. That is, because the entitlement principles *in use* in a society involve not only certain individual rights, but also the social legitimation or acknowledgment of those rights, they can arise only from something like a social contract in which all persons decide which of their individual rights are to be given up to the collective and which are to be held individually.

But if that is so, then why cannot those rational persons make a somewhat different allocation than that of full entitlement to the product of one's labor? Why should I not, for example, agree to provide, via a small tax when I am not destitute, some insurance in case I become through some misfortune destitute?⁵ Many ancient societies had some such arrangement.

⁵ Nozick has, I believe, a fundamental disagreement with the idea that constitutional choice occurs behind a veil of ignorance about one's future position. Nozick would argue that everyone has some knowledge of his future position. He argues that if persons are to be regarded as beginning with a set of natural rights, then theoretical consistency does not permit arbitrarily depriving them of those rights by assuming there is a veil of ignorance when they make a constitutional choice. Thus, according to Nozick, if a social contract were to be made, it could only be made by a set of persons (differentially) naturally endowed, and each could anticipate, because of his endowments, just what position in society he would come to occupy. But the veil of ignorance is, as I see it, a fiction which, though never wholly realizable, is nevertheless theoretically useful. It is an ingenious device for translating *interpersonal* comparisons of utility, and attendant interpersonal conflicts of interest, into *intrapersonal* comparisons of utility, with the attendant intrapersonal balancing of interests among different situations in which one may find oneself. If persons were not behind a veil of ignorance as to their future positions in the society, then those who knew they would have control of the state apparatus would favor more power for the state, while the disenfranchised would favor less; the rich would favor full entitlement to the fruits of one's labor, while the poor would favor a tax upon the rich. Thus without the veil of ignorance, rational self-interested persons would be embroiled in an unresolvable conflict with one another. But with the veil of ignorance, ideally, each person sees himself as potentially in all these possible positions, and makes the decision taking into account each of the different standpoints. Thus although we begin with selfish persons, we arrive at a contract by unanimous consent which is equitable for all, whatever position they might find themselves in. For the theorists of distrib-

In Judaic law, for example, the widows and strangers were entitled to the grain left at the edge of the field during the harvest. Would it not be rational for a person in the original position to agree that a certain small portion of the individual products to which he is entitled be taken away from him, be subject to collective rights, and be reserved for use of the destitute (i.e., for himself when he might be destitute, since by hypothesis, he is acting not out of concern for others but only his own self-interest)?

The amount to be thus set aside, that is, the amount to be collectively held, or the amount of incursion that such a person would allow into his individual entitlement, would depend on how much he felt his entitlement might vary with good times and bad. But the amount is not of concern in our assessment of Nozick, for once we have established the principle, once we have let the camel's nose under the tent, then Nozick's full entitlement theory no longer holds a special place. It is merely the (unlikely) case in which rational persons behind a veil of ignorance agree to hold all their rights individually and not to relinquish any to the collectivity for insurance against time of need.

This does not necessarily mean that rational persons would so empower the state (defined as the monopolist over means of coercion). They might well prefer to so empower a different agency (e.g., an insurance agency), either one whose rates are progressive (Social Security or unemployment insurance) or nonprogressive (private insurance or Blue Cross). But that is another question, which we can leave aside. What is worth pursuing, however, is some understanding of just what would lead rational persons to allocate greater portions of their rights to the state or another agency for collective use.

First and most obvious, of course, is the existence of collective threats, whether from the natural environment or external enemies. Eskimos treat many of their resources collectively during winter months; similarly the Essenes who lived in the desert by the Dead Sea 2,000 years ago maintained a largely communal existence. Modern Zionists, resettling the agricultural lands of Palestine, created collective kibbutzim and cooperative moshavim to confront both the natural and human environment. Insofar as there can be a reallocation of rights at different times within a state, we repeatedly see the same thing: in time of war, citizens voluntarily submit themselves to greater central direction and greater taxation than at time of peace.

utive justice like Rawls, the veil of ignorance is a device for weighing the utility of goods to a person when he is well off against their utility when he is badly off. For a correct form of social contract, such as I have outlined above, in which rights are allocated to the collectivity or reserved to oneself, it is a device for weighing the utility to be expected from having a combined and powerful set of rights used in one's behalf (or possibly against one) versus the utility to be expected from having only individual rights, less powerful but certainly directed as one wills.

Somewhat more subtle is the difference between a largely agrarian economy and an industrial one. In an agrarian subsistence economy, most of a person's essential needs are directly satisfied by his own efforts: he grows most of his own foodstuffs and builds his own shelter. There are good times and bad, but they vary in continuous gradation, for there is not the concept of a "full-time job." In an industrial economy, most of a person's essentials are provided by exchange using money obtained from his labor (in a "full-time job"). In good times, he has a job, in bad times he does not. He cannot subsist by the direct fruits of his own labor in the absence of the job, but must receive aid from some source, or migrate to a place where he can subsist. In addition, the interdependence of the economy suggests that bad times would be likely to hit many persons at once, and one person would be less likely to be able to fall back on resources held by others especially close to him.

Thus it is reasonable to expect that rational persons uncertain of their own position, but knowing the character of the economy, would agree to reserve a larger portion of their product for insurance if their economy were an industrial and highly interdependent one than if it were one of agricultural subsistence; they could agree to a larger tax (and perhaps even a more progressive one). Furthermore, this rational allocation of resources for collective use would carry with it the right to use the resources not merely for common defense but for redistribution to the needy or destitute—just as, in a smaller way, the grain at the edge of the field in ancient Israel was left for redistribution to the needy, not for common use.

Thus it is quite possible to go beyond Nozick's minimal state if we begin with individuals each holding individual rights, simply because it comes to be in the interest of those persons not only to socially cooperate, but to engage in a social contract in which they allocate certain of their individual rights to a central agency for collective use. This could then, of course, lead to a sense of full complacency, as with Dr. Pangloss: it is so because it must be so, and we live in the best of all possible worlds. That is, the rights held collectively by the state were voluntarily vested in the state by its citizens, and thus there is no excess of power held by the state—at least, if the state came into being through a constitutional agreement or other kind of general assent among a wide fraction of its members. Whatever rights (i.e., power) it holds, it holds justly, because it acquired these rights through a just process. This is the line of argument one could follow in justifying whatever powers the state might have, following Nozick's principle of justice in acquisition, and recognizing the state as an actor in the same way that any person is.

Such complacency about the rights held by the state is, however, misplaced for another reason. To explain it, I must digress for a moment to discuss in somewhat more detail Nozick's entitlement theory. Nozick does

not specify what constitutes justice in acquisition or justice in transfer, except for a short discussion of a proviso by Locke that acquisition by one person leave "enough and as good in common for others." Yet what seems like a simple principle (for both acquisition and transfer can be reasonably subsumed under a single heading) may not be so simple under close inspection. For what enables a person to acquire resources or goods or assets? It is, of course, the assets he already has. In the first instance, these are natural ("inalienable") assets such as abilities, skills, and personality. But an asset gained can, if not consumed, be employed to gain further assets. This is most evident with money which, through the interest it earns, substitutes for labor. Is there a difference between the justice of the \$250,000 a Wilt Chamberlain earns from fans' voluntary payments to watch him perform, and of the \$25,000 that *that* \$250,000 earns each year? The subsequent \$25,000 per year also comes from voluntary transfers from others for the right to use the \$250,000, so it would appear to be, on the surface, an equally just acquisition. But if so, is the justice independent of the *rate* of interest?⁶ What about loans in the ghetto, with rates of interest as high as 100% per week or month? These are market-established rates, for the borrowers cannot get loans at *any* interest rate from other sources.⁷ And what about the fact that large holdings of money are able to earn higher rates of return than small holdings?

Nozick would argue, I believe, that any market process, since it is based on voluntary actions by both parties to an exchange, is just, independent of the market prices. But there are problems with this because of imperfections in the market, and it appears likely that some proviso such as Locke's would be necessary (and with this I believe Nozick would agree) to exclude markets in which one party to an exchange is able to restrict the opportunities or choices of the other.

But now we are led into a whole thicket of problems of specifying the conditions under which market processes are just. These problems are compounded, from the point of view of justice, by the additional fact that, as I have stated above, assets beget new assets. This is true of money and it is true of other types of resources as well. For example, the holding of control of communications, whether by blanket advertising or by actual control of a communication medium, can shape others' choices and garner new rights or resources for the holder. Are such new rights or resources justly obtained? In the era of the massive expansion of American capitalism,

⁶ The existence of usury laws in most societies, modern and ancient, shows the ambivalence about the justice of interest, and of allowing the market to establish rates of interest.

⁷ An even sharper question is that of justice when various extreme methods, short of violence, are used to enforce payment and are known to the borrower before borrowing.

when corporations held great power vis-à-vis their workers (and vis-à-vis their owners as well), were the processes of acquisition of new holdings just (excluding those cases in which illegal coercion was used)?

With all this in mind, we must recognize also that individuals are not the only actors in modern society, but that society is populated by intangible corporate actors as well.⁸ Some are business corporations, some are trade unions, some are other types of corporate actors; an especially prominent one is the state. Just as individuals have rights and acquire holdings, corporate actors do so as well, except that corporate actors initially come into being only through individuals' vesting of certain rights and resources in them. And the state has certain special rights vested in it, including the monopoly of force within its boundaries.

Now we may return to the question of collective rights held by the state. Did the state as a corporate actor acquire all of its rights by a just process? We can ask this question without having fully answered the question of what is a just process of acquisition; all that is needed is the recognition that the existence of great and concentrated resources on the part of one actor can distort the choices made by other actors with which it has transactions. If there are any processes of acquisition of holdings (rights and resources) short of coercion that are not just, either according to the definition of justice in acquisition in Nozick's entitlement theory or to a similar principle arrived at among individuals in the social contract through which they formed the state, then it appears likely that those used by the state to augment its powers would be among such unjust processes.

Thus the state, as a corporate actor, is in a very special and advantaged position in its relation to individuals who have, through something like a social contract, vested certain of their rights in it. It is in a position to use those rights as resources in acquiring more rights. Robert Michels (1949), examining this process in a much weaker corporate actor (weaker than Nozick's minimal state), the German Social Democratic Party, labeled it the "iron law of oligarchy." A. A. Berle and Gardiner Means (1940) examined the same process for corporations and termed it the "separation of ownership and control."⁹ The means by which these corporate actors, and even more, the state, use to augment and consolidate their power (i.e., their rights) are several, and they are enhanced by two conditions: (1) the fragmentation and disorganization of the ordinary members or citizens in

⁸ See Coleman (1974*b*) for an elaboration of this notion and a discussion of the emergence of the modern corporate actor in society.

⁹ This has been a more important instrument of the state on the Continent than in English law, where the device of trusts gave a shield of protection from the state, first to the Nonconformist and Catholic churches, and subsequently to business ventures. See Maitland (1904).

contrast to the high degree of organization and mobilization of those who control the central state apparatus, and (2) suboptimal levels of activity in opposition to those in control due to a "free-rider" problem (i.e., we will win or lose with or without my efforts, which are a drop in the bucket, so I'll sit on the sidelines). The means at the disposal of the state include (1) control of, or at least greater access to, means of communication; (2) special legal rights associated with its position as the corporate actor controlling the charters of other corporate actors and the association of persons who might collectively oppose the state's acquisition of further rights; and (3) the capacity, when acting in the name of certain actors, to abrogate the rights of others.¹⁰

As is clear from this catalog of favorable conditions and special means at the disposal of the state, it can acquire and accumulate centrally held rights in ways that any reasonable principle of justice in acquisition would classify as unjust if used by an actor within the state. Few would question such a conclusion when applied to certain states, particularly those practicing state socialism (the Communist states) or national socialism (Nazi Germany), for in those states, the initial holding of rights by the central state apparatus is especially great and remaining individual rights are easily abrogated. But the conclusion holds for *all* states. Unless there are some means for opposing or neutralizing the special state powers, the state will come over time to have an unjust accumulation of rights and resources, to the detriment of its citizens.

The program for political philosophy, then, is not merely to determine what is a just allocation of rights and resources (among persons, or between persons and the state), as Nozick and Rawls have attempted to do, but, recognizing that a state can always acquire additional rights unjustly, to find ways to limit the state's capability for doing so. It is not enough to argue for a minimal state, as Nozick has done; it is necessary to show how ordinary persons can prevent a minimal state from becoming a maximal one. One way of doing this is to develop in detail a theory of justice in acquisition and justice in transfer. Such a theory would provide the basis for constraining the processes by which an individual or a corporate actor (in particular, the state) can justly acquire holdings, including rights themselves.

But this is not merely a program for political philosophy: to see it in

¹⁰ Examples of this are acting in the name of the majority to abrogate the rights of Jehovah's Witnesses or other minorities; in the name of the disadvantaged, to command the central resources necessary for a Rawls-just redistribution; in the name of disadvantaged minorities, to reduce the rights of employers to hire and fire on the basis of merit; in the name of those who drive tractors on hills, to require those who drive tractors on only flat ground to acquire roll bars; and in the name of those who live in large cities, to require those who drive only in small towns and the country to pay for emission-control devices on their cars.

that way is to cast off from the shoulders of sociology a task which is central to its mission—the task of discovering how a society can govern itself while maintaining the delicate balance between the individual and the society, between rights individually held and rights held collectively.

Suppose for a moment that this balance were static, independent of social conditions (though it is not): Then to maintain it would require special structures which prevent a movement either toward anarchy (i.e., a withdrawal of all collectively held rights), or toward an omnipotent state (i.e., a forfeiture of all individual rights). If my conjectures above are correct, concerning the special powers of corporate actors, and particularly states, to accumulate rights that formerly were held by individuals, then the major danger to the delicate balance in an established society lies in the accumulation of powers in the hands of central authorities, primarily the state but also lesser corporate actors. Thus a major task for sociology is precisely the discovery of the processes by which corporate actors are able to amass rights and destroy the balance between individually held and collectively held rights. The above description of processes, such as “the fragmentation among individuals compared to the organization of the state” and “the control of means of communication,” are merely surface descriptions of phenomena that require extensive sociological investigation. What are the means by which the state (or another corporate body) can use its greater organization to acquire further rights from individuals? What structure of control of communication in a society best protects both rights individually held and those collectively held? What social or political structure best insures the continued possibility of bringing together individually held rights and resources to oppose those who hold positions of central authority?

And in addition to the processes by which corporate actors (and especially the state) acquire power, there are the processes by which persons withdraw legitimacy from such a body, destroying its power to act. These also require investigation. For example, just as control of the means of communication by the state gives it the capability of amassing greater rights and powers for itself, that control of communication by another corporate actor or actors can aid in destroying the state's legitimacy. It may also be that the two sets of processes are partly bound up together: for instance, that one cause of the withdrawal of legitimacy from the state is the exercise of power by the state beyond the point that had been its traditional limit.

These are not problems commonly addressed in sociological research, for they are macrosociological problems which are difficult to investigate. They are not wholly resistant to examination at levels below that of society (for example, many of them are exhibited in large organizations which are nominally subject to self-government, such as trade unions), but they can-

not be studied at the interpersonal level. Also, they are not easily studied as macrosociological resultants of microsociological processes, such as demographic movement, occupational mobility, and educational attainment.

The political and moral philosophers, from Rousseau and Locke to Rawls and Nozick, address the normative questions of the appropriate balance between liberty and equality in society, the balance between individually and collectively held rights; it is the task of sociology to discover the processes that affect this balance; this knowledge can allow a society to arrive at a normative consensus and maintain the normatively specified balance.

In the meantime, what can we say for Nozick? I think we can say that his book is an enormously important contribution to the rich and rapidly changing field of moral and political philosophy. I don't believe Nozick is correct, but he's less incorrect, I believe, than those who have preceded him.

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Review Essay

Becoming Modern: Individual Changes in Six Developing Countries. By Alex Inkeles and David H. Smith. Cambridge, Mass.: Harvard University Press, 1974. Pp. xi+436. \$15.00.

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The evolutionary perspective toward social change is a major part of the "great tradition" which 20th-century sociologists have inherited from seminal theorists of the 19th. The twin but polar concepts of "community" and "society" have their descendants in the ideal types of "tradition" and "modernity." That Manichean duet has been the conceptual foundation of modernization theory as it emerged in studying developing countries and new nations after World War II. Although they make some important revisions in that theory, Inkeles and Smith accept membership in its theoretical family and, in *Becoming Modern*, extend and elaborate the traditional.

Their study is an attempt to answer two major questions: (1) Is there a coherent syndrome of "modern man" or only a set of unrelated traits? (2) With respondents classified as closer to the traditional or to the modern pole of a questionnaire scale, why do individuals fall "at one or the other end of the continuum" (p. 6)?

The design of the research reflected the interests of the investigators in the effects of early versus later socialization and the specific effects of factory, education, and city on the development of modernity. In the early 1960s, the authors and a staff of American and local social scientists conducted interviews with samples of approximately 1,000 persons in each of six countries and areas—Argentina, Chile, East Pakistan, India, Israel, and Nigeria. Purposive quota samples were selected so as to provide a comparison of agricultural cultivators, factory workers of rural and urban origins and of new and old experience, nonindustrial urban workers, and experienced factory workers with high levels of education. The interviews were lengthy, approximately four hours, and utilized a survey-style pre-coded questionnaire, as nearly identical as possible for each of the six samples.

In an early part of the book the authors tell the reader that one part of their "hidden agenda" is "to bring people back in" (p. 9). It is not modernization at the institutional or societal level that occupies their attention. Their concern is with attitudes, values, and behavior as these make up the personality and predisposing elements of traditional and modern men. (The

exclusion of women is explained by reference to practical circumstances and the dominantly male character of industrial workers.)

The study makes an important contribution to research on social change in its conclusions about *why* some men are modern and some are not. Their modernity scale items did correlate well with each other, and they could talk about "overall modernity" and "modern man." Their analysis of the effect of early socialization on later attitudes and values led them to conclusions sharply negating the view that early experiences cannot be overcome through later ones. Men brought up in rural, agricultural life and currently working as agriculturalists were at the traditional end of the scalar pole, but men with the same background, currently working in factories, were to be found at the opposite end of the pole. Education was a crucial variable in promoting modern attitudes and values. Mass media performed a lesser but still important function. However, for men whose early life lacked these modernizing influences, the factory itself was a "school for modernity." Using partial correlation and regression analysis, Inkeles and Smith report that the effects of such variables as urban residence, ethnicity, and religion are largely indirect, affecting chiefly access to education and/or exposure to mass media.

What do Inkeles and Smith mean by "modern"? In preparing their questionnaire they used a sociopsychological approach to modernity, "a way of perceiving, expressing and valuing . . . a set of predispositions to act in certain ways" (p. 16). In developing a list of modern characteristics they placed great emphasis on "the presumed *requirements* of daily living in a complex society" (p. 18) and especially the "particular *requirements* of running a factory" (p. 16; *italics mine*). Such qualities as openness to new experience, readiness for social change, orientation to the present, dispositions to form and hold opinions, and belief in the efficacy of one's actions are among the traits of the modern. So too are attitudes toward kinship and family, women's rights, aging and the aged, birth control, and other institutions in which the modern is seen as moving away from hierarchical control and the opinions and attitudes of the past. In their concluding section, the authors sum up their view of the modern man: "He is an informed participant citizen; he has a marked sense of personal efficacy; he is highly independent and autonomous in his relations to traditional sources of influence . . . and he is ready for new experiences and ideas" (p. 290). In keeping with the traditional and evolutionary conception of change, they see the man of tradition as the polar opposite of the modern in all details.

The echoes of Weber's *Protestant Ethic and the Spirit of Capitalism* are heard counseling that change depends on shucking off one self-conception for another. The authors are optimistic about the possibilities of such change, but they are emphatic in seeing the past and the future as irre-

concilable antagonists. Their description of the traditional man is continuous with the sociologist's traditional view of peasants: passive acceptance of fate; fear of innovation and distrust of the new; dependence on traditional authority and the wisdom of the elders and religious and customary leaders; fear of larger regional and national entities; exclusive identification with purely local and parochial primary groups. Their conception of the modern man is constructed, as they themselves assert, from their view of the functional requirements for the operation of a factory and a complex society. The two types are discontinuous and opposed. New industrial institutions need, and fortunately create, new men. Traditional men and modern institutions are implacably in conflict.

Courage in the defense of a scholarly tradition is admirable, on moral if not on intellectual grounds. In stubborn allegiance to a shaky methodology and an outworn theory, the authors ignore almost all of the research and the study of underdeveloped societies published during the past 20 years. They never mention, let alone confront, the criticisms of modernization theory which have appeared during the past few years. While they are sensitive to some of the critical thrusts at attitude survey methodology, their defense is not pertinent to the task they have outlined for the study. It is almost as if the book were being published circa 1965 instead of circa 1975.

In the past eight years the evolutionary theory of modernization and the utility of the dual, opposing types of "tradition" and "modernity" have been subjected to a sharp critical attack from historically oriented sociologists, both Marxist and non-Marxist. Since 1967 modernization theory and the "tradition-modernity" typological confrontation have had an intellectually problematic status. The authors of *Becoming Modern*, in ignoring the corpus of specific studies of local situations and events, repeat the errors that have limited the value of the evolutionary perspective in earlier modernization theory, and they are open to the same criticisms.

Consider two areas of concern to the authors: the role of factories in developing "modern men" and the possibilities of economic innovation. When we examine the studies in which specific factories have been studied to gauge their impact on a population, the conclusions which Inkeles and Smith derive from questionnaire analysis are contradicted. Joseph Elder's study of "Brahmins in an Industrial Setting" (northern India) and Manning Nash's *Machine-Age Maya* (Guatemala) both fail to find the conflict between traditional and modern ways which implies the necessity or the occurrence of changes in other values and customs. While Inkeles and Smith certainly do emphasize the ability of traditional man to change, they both find and imply that the change is an overall one; the factory creates a man who is modern in most segments of his life. Contra, Richard Lambert showed the significant way in which traditional values of security

and primary group loyalties were a strong element in the operation of Indian factories in Poona.

Tangentially, but importantly, Inkeles and Smith also ignore the mixture of traditional and modern elements which has been the mainstay of many studies of comparative political and educational research. (I am most familiar with the Indian ones, such as Weiner's *Politics of Scarcity* and Lloyd and Susanne Rudolph's *The Modernity of Tradition*.) What is the reader to make of these studies, which emphasize that modern institutions are by no means devoid or independent of tradition, that the mixture of the two types is too situational and historical to be consistent with the evolutionary theory which posits them as opposites? In ignoring the corpus of past research and its possible implications for theory, the authors of *Becoming Modern* fail to come to grips with evidence.

Consider the view of the peasant as fatalistic, passive, custom-bound, and lacking "a thrust toward more instrumental kinds of attitudes and behavior" (p. 291). Again the studies of village and agricultural settlements have revealed a much more complex picture. Thus Geertz, in his study of economic development in Indonesia, found the role of the traditional networks of influence and sponsorship vital to one of the major sources of economic innovation generated by the traditional nobility. The importance of close extended family ties in India has often been seen in the economic activities of investment and expansion. To speak of the political passivity of peasants is to ignore the complex politics of bribery and corruption (an often effective form of political participation), the large vote turnouts in Indian villages, and the immense significance of peasant revolts and revolutions in the 20th century. This is not to construct an opposite stereotype of peasant activism but to point to the inadequacy of such general types as "tradition" and "modernity" to capture the historical and situational features of behavior. Such studies cast doubt on the underlying thesis that to become modern is necessarily to become untraditional.

Inkeles and Smith might well object to this mode of analysis and reply legitimately that their research has dealt with the differences within the samples studied and not the analysis of institutions. Their objective is to explain why some men are *more* modern or *less* traditional than others. It is process and not essence which they confront. This leads us to the methodological difficulties and their relation to the theoretical.

The operational definition of tradition and modernity is given by their principle of "selecting that point in the distribution of answers to any question which came closest to putting half of the country sample on the modern side and half on the traditional side" (p. 87). One point was given for the traditional side of the median and two points for the modern, and then the points assigned were converted into an overall

score of zero to 100. (The assumption of equidistance is never explained.) The concepts of the traditional and the modern are, in this fashion, always relative to the sample. It is possible for persons defined as traditional in one country to have higher scores than those defined as modern in another, and such a difference occurred in the study (especially in the East Pakistan sample, most frequently in education). How high a score constitutes modernity? Are we looking at one end of some possible universal continuum in which the "distance" from one type to another is small compared with the total range possible? Are the "traditional" men *really* only slightly less modern than the "modern" men and/or vice versa? These are not idle questions, especially in the light of the past studies mentioned above. Inkeles and Smith, in their system of purposive sampling, are less interested in empirical description than in "laws" which tell us what *will* happen *if* a situation exists. For example, they state that rural men are overwhelmingly less modern than urban men, but their analysis finds the causes of these differences in such variables as education, media exposure, and nature of work rather than rural origin per se. Thus they derive policy implications to show what would happen *if* rural men were brought into urban milieus.

At the heart of the methodology is the central question of the relation of attitudes and values to behavior. The authors do not dodge the issue, but at the same time they do not grasp it by any central part of its anatomy. What does it signify for a man to be deemed "modern" or "traditional" in his responses to questions, most of which are precoded? Does this segment of communications behavior (which is itself used, in "the holding of opinions," as one of Inkeles and Smith's traits of the modern man) tell anything beyond how respondents react to being asked certain kinds of questions? Does what is said in an interview situation bear any relation to what we do or are likely to do?

In their defense the authors cite the relation between the modernity scales and the behavior reported and validated in the interviews. Men high on the attitudinal scales of modernity were also high on such behavioral items as knowledge of news, ability to recognize consumer items they wished to have, ability to read, and correct answers to information items. They were also more likely to report political participation than were those low on the attitudinal scale. The problem with this defense is that it begs the question at stake. Most of the modernity scale and the theoretical reasoning consists of generalization about the importance of values and attitudes to modernity. It is the change in attitudes and values that the authors conclude to be "one of the most essential preconditions for substantial and effective functioning of . . . modern institutions . . ." (p. 313). Whether the attitudes expressed in such questionnaire responses as quickness to adopt innovation, readiness to implement birth-control measures,

urging of a son to go as far as he can in school, and rejection of passivity, resignation, and fatalism differentiate a man's behavior from that of his opposite, the more traditional man, in work behavior, family behavior, political behavior, and other areas of life cannot be demonstrated by his greater degree of information. It is not a question of lying by respondents but one of the status and influence of such situationally induced attitudes with regard to other situations. Doubtfulness is increased by the conclusions of the case-study literature.

As so often in reading studies based on survey research, I am impressed by the ingenuity and imagination of the researchers. There is an aesthetic appreciation of the artfulness with which problems were met and resolved. But there is also a feeling of emptiness. The modern man of Inkeles and Smith is a strangely desiccated and ethereal human being. Either he has little relation to the flesh-and-blood world or he must be accepted as the embodiment of attitudes and values that somehow exist but are only tangentially in the world. Like a Hindu or Buddhist ideal, they reflect an inner being divorced and distant from an acting society. The effect is one of abstract laws and abstract sociology. I have no difficulty with abstract art as long as it hangs on the walls. When it comes to a communicable map of territory, I prefer Wyeth to De Kooning.

Book Reviews

The Rebellious Century, 1830-1930. By Charles Tilly, Louise Tilly, and Richard Tilly. Cambridge, Mass.: Harvard University Press, 1975. Pp. xiv+354. \$15.00.

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This is an important contribution to the growing volume of literature on collective behavior and protest. It has many virtues, not the least of which is that it is a work of genuine collective endeavor by three authors from different disciplines who have pooled their resources in a study of comparative history.

The authors begin by questioning the methods and assumptions of others who have worked in the field. These persons, they contend, fall into two main schools of thought: those who explain collective violence or protest in terms of "breakdown" and those who explain it in terms of "solidarity." To the first school belong those who see protest, as they see crime or mental illness, as the direct outcome of the breakdown of old social norms and the dissolution of old social ties. Among proponents of this view the authors cite Émile Durkheim, Neil Smelser, and the authors of the McCone report on the Watts riots of 1965. (They might have added Louis Chevalier with his "uprooting" thesis on crime and protest in 19th-century Paris.) The "solidarity" school comprises those who relate protest to the struggle for power of antagonistic social classes and political groups. They include Marx and, in our own day, the socialist E. P. Thompson and the liberal Clark Kerr. The authors see weaknesses in both theories, though patently more in the first than in the second; but before putting forward a counter-theory of their own, they propose to present the reader with the results of their inquiries into a century of protest in Germany, Italy, and France.

So their method is essentially an inductive one, and, unlike Gustave Le Bon and others of his kind, they have thought it important to look at the facts. In the first place, they have asked a number of questions about each country in turn, such as the following: what were the incidence, the dimensions, and the distribution of collective violence? what forms did it take? when did it occur? who were the protesters and what were their targets? did crime and collective violence go together? how did those in authority react, and how was violence affected by their reactions? what part did industrialization, urbanization, and the evolution of the nation-state play in the stimulation and shaping of violence? For their answers they have relied largely on newspaper accounts and less on contemporary observers and the appropriate secondary literature that has mushroomed in the past 50 years—earliest in France, later in Italy, and later still in Germany (rather more in the east than the west). And, like good social scientists, they have made effective, though unobtrusive, use of the computer to enumerate and collate the results.

The results, as one would expect, show considerable variations among the three countries concerned. Basically, the nature and pattern of collective violence have, at each major stage, reflected the point reached in the development of the resources at the command of the state. In 1830, France alone of the three was a developed nation-state. After 1850, Italy continued to lag behind France, and it lagged behind Germany as well: although both Italy and Germany were nominally united by the events of 1870, in reality Italy remained divided between north and south. Industrialization followed a broadly similar course, occurring first in France, next in Germany, and last in Italy, where the south limped far behind the north. All of this strongly influenced the forms that collective behavior and violence took and the times when they erupted. Thus, France, the earliest "developed," was the first to pass from the "reactive" type of protest (concerned with the retention of established rights) to the "modern" or "proactive" type (involving claims to new rights), with the Italian south lagging far behind the others. To take the example of the food riot, it phased out in France by 1848, in Germany by the 1860s (though recurring briefly in 1917), while in Italy it lingered on in the center and south until 1898 with a later revival around 1920. And, for these and other reasons, peaks of violence of all types occurred more frequently in France than in Germany, but in Italy most frequently of the three.

But there were also important similarities, and it is from them, of course, that the authors have drawn their conclusions. These are (briefly): (1) there was no concordance between protest (or collective violence) and crime, or between criminals and protesters; (2) there was a close concordance between protest and urbanization in the long run but not in the short; (3) industrialization and economic development were important in shaping the nature and incidence of protest, as in shaping the nature of society, but protest did not arise in automatic response to economic conditions alone; (4) the essential progenitor of protest was the power structure, and collective violence at every stage and in every country was a by-product of the political process and, therefore, however primitive its appearance, had a logic and a historical purpose of its own. (Thus the authors reject the distinction sometimes made between "prepolitical" and "political" protest.) In this respect, the means of repression at the state's command (or at the command of its satellites) was always a matter of prime importance. Repression could act in one of two ways: as a brake or as a stimulus, according to the intensity with which it was applied. For the first, they give as examples the almost total containment of collective violence in France under Louis-Napoleon, in Germany under Bismarck, and in Italy under Mussolini; for the second, they cite the relatively peaceful internal development of Britain (excluding, of course, Ireland) after the 1840s, which they attribute, in part at least, to the "greater British constraints on military and police violence" (p. 280).

In all, Marx emerges from their survey as a far surer guide than Durkheim or Weber; the breakdown theory is found to have little to recommend it; the solidarity theory, on the other hand, stands up well to the test. It is,

however, to a modified version of the latter, the "political-process" model, that the authors feel most willing to subscribe.

Some problems arise from the choice of the term "collective *violence*" to denote the point at which collective action and protest yield to the stage of violent confrontation. One can understand why the choice was made: "violence" is presented as not identical or coterminous with, but a projection of, "collective action"; and the term "protest," favored by others, would apply to the protesters alone and not take account of that part of collective violence which has been the affair not of the protesters but of the authorities and their henchmen, such as Fascists and Nazis. Yet the term is apt to be confusing; not least because strikes, an important part of the protest record, have tended (except, possibly, in "backward" Italy) to be peaceful rather than violent since 1848. This dual focus on popular and executive violence has also had the effect of blurring the "great divide" between the periods of reactive and proactive protest, which coincides roughly with the transition from a nation's "preindustrial" to its "industrial" stage. Yet, essentially, it is popular protest, rather than executive reaction and repression, that this book is all about; and I, at least, would have thought it a happier solution if the authors had fixed on popular protest (whether violent or nonviolent) as their focal point and made repression and nonprotesting violence subsidiary to it.

Perhaps it is this focus on "violence" that has led the Tillys to neglect popular ideology as an element in protest. This is a serious omission because the protesters' view of themselves and of the world in which they lived must have been an important element in the transition of protest from its predominantly reactive to its predominantly proactive form. (It may even be a concomitant of violence itself, as Franz Fanon conceived it in his discussion of anticolonialist warfare.) Moreover, the persistence of an older, traditional or "inherent," ideology may well help to explain the reluctance during this transitional period to abandon old forms of protest for new.

These are, however, comparatively minor blemishes when set against the general excellence of the book. The authors are, indeed, to be warmly commended for their novel and rigorous methods of research; for their careful observation and enumeration of the facts; and, not least, for their critical assessment of old "truths" and their substitution of something new. No doubt their work will provide a valuable model for others to follow. Meanwhile, it may be hoped that the Tillys will not dissolve their partnership until they have brought their own record up-to-date by projecting their inquiry into the past 45 years.

Rebellion, Revolution, and Armed Force: A Comparative Study of Fifteen Countries with Special Emphasis on Cuba and South Africa. By D. E. H. Russell. New York: Academic Press, 1974. Pp. xiv+210. \$14.50.

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According to C. Wright Mills, a major lesson of the Cuban Revolution is that "... guerrilla bands, led by determined men, with peasants alongside them, and a mountain nearby, can defeat organized battalions of the tyrants equipped with everything up to the atom bomb" (quoted in Russell, p. 14). Some would argue that recent events in Vietnam and Cambodia lend strong support to Mills's position, but not South African-born D. E. H. Russell. Professor Russell has written a book challenging the view—held by Mills along with Mao Tse-tung, Che Guevara, and Régis Debray among others—that mass rebellion can prevail against the united armed forces of an existing regime. Russell asserts that, on the contrary, "armed-force disloyalty is necessary for a successful outcome of rebellion, but it is not a sufficient condition. [p. 79] . . . when the regime and its armed forces remain willing to use their coercion resources to suppress the rebels, they can" (p. 81).

Russell reaches these conclusions by analyzing 14 20th-century rebellions, seven of which were victorious (Mexico 1911, Albania 1924, Afghanistan 1929, Brazil 1930, China 1949, Bolivia 1952, Cuba 1958), while the other seven were defeated (Cuba 1912, Italy 1914, Honduras 1933, Austria 1934, Spain 1934, Colombia 1948, Burma 1954). These 14 rebellions constitute a stratified random sample drawn from a slightly larger population of events (12 successful, 16 unsuccessful) which satisfy Russell's definition of mass rebellion.

Russell rightly criticizes many past analysts for their failure to consider unsuccessful rebellions, for their tendency to discount the importance of military factors, and for their tendency to draw global conclusions on the basis of one—usually victorious—rebellion. His distinction between rebellion and revolution is quite useful, and his insistence on treating the condition of the armed forces as an autonomous variable will guard against future neglect of military considerations. A systematic comparison—such as Russell has attempted—of rebellions occurring in a variety of social contexts is an approach that can, and I hope will, offer many fruitful insights.

Despite these virtues, Russell's study is marred by serious shortcomings which not only limit the applicability of his findings but also pose grave questions as to the accuracy of his conclusions even within the narrowest possible confines.

The problems begin with the author's concept of mass rebellion and the population of mass rebellions to which this concept gives rise. To quote:

... as a result of including in my definition of mass rebellion the requirement that the upheaval occur within an *autonomous political system*

[author's italics], violent power struggles that occur in the following social contexts do not qualify for inclusion in the universe.

1. Societies that are not structurally independent of a foreign power at the time of the conclusion of the struggle. . . .
 2. Societies without a real central government.
 3. Societies in which there is substantial external intervention in that people from another country . . . assist in fighting on behalf of the rebels or the regime. . . .
 4. Civil wars, where the struggle is over "self-determination," "autonomy," or "secession," not the overthrow of the regime.
 5. Wars, which involve more than one "autonomous political system."
- [Pp. 66, 67]

These are quite drastic limitations on the universe of mass rebellions. Among the violent power struggles which they exclude are the Russian Revolution of 1905, the Chinese Revolution, vintage 1927, the Greek Civil War of 1944-47, the Algerian Revolution, the Hungarian Revolution of 1956, and the recently triumphant Vietnamese Revolution.

Russell's concept of mass rebellion is problematic on theoretical and methodological grounds as well. In this age of imperialism with its multinational corporations, world market, complex system of military alliances, international communication systems, and emergent world culture, it seems rather naive to emphasize the notion of an autonomous political system. In fact, resentment over lack of political autonomy has been a major motive for mass rebellion since the end of World War II at least. To exclude anti-imperialist upheavals only because they fail to occur within autonomous political systems is to eliminate the most representative and significant mass rebellions of our time, many of those which inspired the proposition Russell seeks to refute. These exclusions, therefore, raise important questions about the value of Russell's findings. If his conclusions cannot be applied to the upheavals in Vietnam, Cambodia, Mozambique, Angola, and Palestine, of what significance are they?

Furthermore, the intervention of foreign powers is strongly influenced by the character of the relationship between the rebels' and the regime's armed forces. In many cases a foreign military power will intervene on behalf of an established regime when a rebellion is successfully defeating the *loyal* armed forces of the regime, but not when the regime's armed forces are *disintegrating* in the face of a rebellion. The foreign power may reason that effective intervention is possible only if the endangered regime has not experienced overwhelming internal decay and therefore retains the capacity (with appropriate external aid) to reestablish its political hegemony.

If this is indeed the process by which intervention occurs, the exclusion of violent power struggles with foreign military involvement could produce a sample which seriously distorts the relationship between the outcome of mass rebellion and the disposition of the armed forces. Moreover, the distortion would coincide exactly with the conclusions that Russell reaches.

The issue of foreign intervention creates still further complications. Precisely what should be considered a case of foreign intervention? There

are certainly many instances in which foreign interventions short of direct troop involvement have been decisive.

Even if we disregard the problems just mentioned, this study suffers from seriously inadequate causal analysis. The author seeks to establish that armed-force disloyalty is causally necessary for successful mass rebellion. His data show that at least some troop disloyalty occurred during the course of all seven successful rebellions, while in four of the seven unsuccessful rebellions no troop disloyalty was evident. Russell's analysis fails to establish whether troop disloyalty is causally prior or causally subsequent to successful rebellion. In other words, it fails to indicate whether troop disloyalty is an essential cause of successful rebellion or merely one of its manifold consequences. The history of warfare is replete with examples of disloyalty on the part of defeated troops. Sometimes troop disloyalty is responsible for the defeat, but more frequently it appears to be a consequence of losing.

It is hardly surprising that as a rebellion approaches a successful conclusion many soldiers will read the writing on the wall and join the rebels, or at least stop fighting them. The soldiers' reasons may vary from a desire to avoid retribution, or to use the opportunities created by successful rebellion to advance their own position in life, to a genuine ideological conversion. In any event, these actions should be seen as a consequence rather than a cause of the rebellion's successes, although they may well hasten the victorious denouement. Most troop disloyalty in the Chinese Revolution of 1949 was apparently of this sort.

Studies which apply a quantitative methodology to historical events must proceed with utmost caution, especially if, as in the present case, the sample is small and the events of interest are enormously heterogeneous. When situations governed by a variety of causal structures are lumped together without adequate appreciation for their differences, the analysis which results can obscure rather than illuminate the relationships of interest. Russell's quantification methods are not very reassuring in this regard. For example, his index of mass-rebellion scale fails to distinguish between rebellions involving 501 deaths and those involving over 1 million fatalities, or between rebellions with 1,000 participants and those with 10 million, or between rebellions lasting seven months and those lasting 20 years. As a result, the great Chinese Revolution receives the same scale score as the ouster of the Mexican ruler Diaz in 1911. The author's method of determining the extent of troop disloyalty is also questionable. At the very least, this scoring should have been done by someone not apprised of the hypothesis Russell wished to demonstrate. For several rebellions (Honduras 1933, China 1949, Bolivia 1952, Cuba 1959, and possibly Spain 1934) Russell's scoring may have erred in a direction which supported his hypothesis.

Having taken great pains to quantify troop disloyalty and scale of rebellion, Russell does surprisingly little with the measures he has obtained. To illuminate the relationship between rebellion and military disloyalty further, we divided the successful-rebellion category into large-scale successful rebellions (Mexico 1911, China 1949, Cuba 1958) and small-scale ones

(Albania 1924, Afghanistan 1929, Brazil 1930, Bolivia 1952) using the scale index devised by Russell. With these two subcategories and the unsuccessful-rebellion category, we examined whether troop disloyalty occurred early or late in the revolutionary process, again using the indices provided by Russell. Some elementary but rather revealing computations are presented in table 1.

TABLE 1
AVERAGE DISLOYALTY SCORES FOR REBELLIONS OF VARIOUS TYPES
BY STAGE OF OCCURRENCE

TYPE OF REBELLION	STAGE OF REBELLION AT WHICH DISLOYALTY OCCURRED		
	Early Disloyalty	Middle and Late Disloyalty	Total Disloyalty
Large and successful	4.0	12.8	14.8
Small and successful	19.0	5.5	24.5
Unsuccessful	8.3	0.6	8.9

According to these figures, large-scale successful rebellions tend to generate substantially less troop disloyalty than do small-scale ones. Moreover, the bulk of troop disloyalty occurs early in small-scale successful rebellions, but at a middle or late stage in large-scale ones. Unsuccessful rebellions apparently produce less troop disloyalty than either of the two successful types, and almost all such disloyalty occurs early in the rebellion.

Several conclusions are suggested by these data. The later troop disloyalty occurs, the more likely it is to be a consequence rather than a cause of the outcome. Therefore, the outcome of large-scale successful rebellions seems less influenced by troop disloyalty than is the outcome of small-scale ones. Why is this?

In chapter 4 of *Rebellion, Revolution, and Armed Force*, a distinction is made between successful and unsuccessful revolution: ". . . *successful revolution is substantial social change following a rebellion; unsuccessful revolution is nonsubstantial social change following a rebellion* [author's italics]" (p. 58). Interestingly, all rebellions included in the large-scale successful category would probably fall under the rubric of successful revolutions, while all small-scale successful rebellions might well be classified as unsuccessful revolutions. This observation may help clarify the data presented in the table, as well as the overall relationship between rebellion and armed-force disloyalty.

Rebellions with revolutionary aspirations cannot rely for their success upon the very institutions they propose to transform drastically or perhaps obliterate (among which the military establishment is typically pre-eminent). To be successful, such rebellions must accomplish extensive mass mobilization and must build alternative structures—including alternative military structures—with which to combat the existing regime.

Only after revolutionary rebellions have achieved considerable successes, have exhibited indestructible determination, and have demonstrated reasonably good prospects of victory will extensive troop disloyalty occur.

This view, we might add, is supported by Leon Trotsky's detailed analysis of military disloyalty during the February Revolution. Trotsky offers the following hypothesis:

... one conclusion emerges with irrefutable clarity: the more the soldiers in their mass are convinced that the rebels are really rebelling—that this is not a demonstration after which they will have to go back to the barracks and report, that this is a struggle to the death, that the people may win if they join them, and that this winning will not only guarantee impunity, but alleviate the lot of all—the more they realize this, the more willing they are to turn aside their bayonets, or go over with them to the people. In other words, the revolutionists can create a break in the soldiers' mood only if they themselves are actually ready to seize the victory at any price whatever, even the price of blood. And this highest determination never can, or will, remain unarmed. [*The History of the Russian Revolution*, vol. 1, *The Overthrow of Tzarism* (New York: Simon & Schuster, 1932), p. 121]

Hence, successful revolutionary rebellions are likely to involve large numbers of people, to be of long duration, and to entail many casualties. When it does happen, troop disloyalty—be it disintegrative disloyalty or late-occurring loyalty transfer—is at least as much a consequence of the rebellion's previous successes as a cause of its ultimate victory. The history of warfare suggests that the battlefield successes of an internal rebellion are far more corrosive of troop morale and discipline than would be equivalent success on the part of a foreign enemy. We are not suggesting that the disloyalty of the armed forces is unimportant. Successful revolutions have always involved extensive agitation within the armed forces. This agitation is, however, part of the overall process of mass mobilization rather than, as Russell would have it, the single *sine qua non* for the success of a revolutionary rebellion.

Rebellions which do not seek a revolutionary transformation of society present a quite different picture. Usually, the social content of such rebellions will not be sufficiently intense to make possible extensive enduring mass mobilization or the building of alternative power structures. Nor do the political preferences of the rebellion's leaders incline them in these directions. On the other hand, since the rebellion's objectives are not revolutionary, the prospects of winning over significant sectors of the regime's military forces, including substantial segments of the officer corps, may be reasonably good. In fact, the best way of insuring that a popular rebellion does not challenge the hegemony of the ruling class might well be to hitch the success of the rebellion to the actions of troops disloyal to the regime but nonetheless under the control of this dominant class. In the absence of support by significant elements of the ruling class, it is hard to see how a nonrevolutionary rebellion could prevail.

Successful nonrevolutionary rebellions are usually small in comparison

with their revolutionary counterparts because of the absence of sustained mass mobilization and the absence (or drastic reduction) of military forces willing to defend the regime. Russell's data suggest such rebellions do not generate much additional disloyalty after their early phases. Perhaps this is because the bulk of the military has already been disloyal, or because mass participation is insufficient to encourage additional disloyalty.

Both revolutionary and nonrevolutionary rebellions, it goes without saying, can be defeated, but the reasons for their respective defeats are quite different. When nonrevolutionary rebellions are defeated, it is almost always because they fail to bring about sufficient loyalty transfer on the part of the regime's armed forces. On the other hand, when revolutionary rebellions fail, it is usually due to insufficient mass mobilization, inadequate organization, or the crushing opposition of an imperialist power. Only rarely can these failures be attributed to an absence of troop disloyalty.

The achievements of Russell's study in focusing attention on the inadequacies of previous studies, in suggesting the value of a comparative approach, and in emphasizing military considerations are quite considerable. But the value of his conclusion is largely vitiated by his desire to establish a simplistic relationship between armed-force disloyalty and successful rebellion. He seeks to replace the dogmatic faith in the capacity of rebels to triumph over all military obstacles, expressed in the quotation from C. Wright Mills given at the outset of this review, with an equally dogmatic assertion that rebels can never prevail over a regime with a loyal and disciplined military apparatus. Dogmatism, be it optimistic or pessimistic about the prospects of rebellion, is unlikely to shed light on the dynamics of violent struggles for power. On the contrary, dogmatism of the sort evident in Russell's book will inevitably fuel the polemical fires which already hinder serious investigations of the revolutionary process.

Russell, as we mentioned at the outset, is South African born, and it appears that the case of South Africa weighs heavily on his mind. He is pessimistic about the chances of successful rebellion in South Africa and develops a persuasive argument to justify his pessimism. Apparently, Russell undertook his research on rebellion largely to clarify the dynamics of repression and resistance as they occur in South Africa, and he seems to view all rebellions in terms of concepts and problems derived from the so far unsuccessful struggle against the racist tyranny of "apartheid."

It may well be that South African social structure creates an unbridgeable gulf between any conceivable army of black rebels and the white military establishment. Under these circumstances, the South African white military might well display tenacious discipline and unrelenting ferocity in opposition to black rebellion and in defense of white supremacy. Under these circumstances, a black rebellion faces enormous obstacles, greater obstacles perhaps than those confronted by rebellions in other parts of the world. But the critical factor in South Africa seems to be the dualistic nature of social structure, of which armed-force loyalty is a mere

reflection. The ultimate outcome of rebellion in South Africa will be determined by the capacity of South African society to maintain this division in the face of contravening forces and by the willingness of the international community to tolerate the continuation of South African tyranny.

In any case, we do not require a general theory asserting the universal impotence of rebels in the face of a regime with loyal armed forces to explain the situation in South Africa. In his delineation of the mass-rebellion universe, Russell identifies 12 successful and 16 unsuccessful rebellions, thus indicating a success rate of over 40%. If taken at face value, these odds would justify considerably more optimism than revolutionaries usually feel when embarking upon a serious enterprise of rebellion. Russell does not comment upon the relatively high incidence of successful rebellions, but it certainly does not jibe with his dictum on the necessity of military disloyalty. The most unfortunate—we could even say dangerous—aspect of this book is the defeatism which permeates virtually every page. Russell might do well to ponder a statement by Trotsky which he quotes, but evidently does not really comprehend: "There is no doubt that the fate of every revolution at a certain point is decided by the break in the disposition of the army. Against a numerous, disciplined, well-armed, and ably led military force, unarmed or almost unarmed masses of the people cannot possibly gain a victory. But no deep national crisis can fail to affect the army to some extent. Thus along with the conditions of a truly popular revolution there develops a possibility—not, of course, a guarantee—of its victory" (*The History of the Russian Revolution*, 1:120).

Literacy in Colonial New England: An Enquiry into the Social Context of Literacy in the Early Modern West. By Kenneth A. Lockridge. New York: W. W. Norton & Co., 1974. Pp. xii+164. \$6.95.

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Students of the colonial history of the United States have increasingly been attracted to the theory and methodology of the social sciences. Kenneth Lockridge is one of the dominant figures in this attempt to create a "new" social history. Moving away from his more traditional doctoral work, Lockridge searched for quantifiable data upon which to employ sociological techniques. Embarking upon a study of charitable behavior in America and England based upon the analysis of wills, he discovered that the variable he had coded as "literacy" was more interesting than charitable behavior. Hence the work under review, which in character and length resembles two articles more than it does a book.

Lockridge summarizes his findings about the "reluctant revolution" in early American literacy succinctly:

It appears that New England experienced several generations of mass illiteracy before achieving nearly universal male literacy toward the end of the colonial period. The eventual advent of universal male literacy

transformed the social structure of literacy by erasing correlations between social status and this basic skill. Yet there is no evidence that literacy ever entailed new attitudes among men, even in the decades when male literacy was spreading rapidly toward universality, and there is positive evidence that the world view of literate New Englanders remained as traditional as that of their illiterate neighbors. Women's literacy improved, but female illiteracy remained quite common and women were always at a distinct disadvantage in obtaining basic education. [P. 4]

Lockridge's raw data for New England show a rise in male literacy from 60% in 1660 to 70% in 1710, 85% in 1760, and 90% in 1790 (p. 13). Female literacy moved from about 30% to nearly 75% during the same period.

Lockridge explains the rise in male literacy primarily in terms of the emergence of effective compulsory public schooling in response to (Puritan) religious imperatives (pp. 65-69). He finds that sex, wealth, occupation, and geographical location account for only about 25% of the variation in literacy rates, and he attributes the dismal performance of females to their deliberate exclusion from the public education system (p. 42).

At the same time, he attacks the commonly held position of leading educational historians (Bernard Bailyn in particular) that schools were the modernizing response of colonials confronted with a hostile environment (pp. 43, n. 63; 78). On the contrary, he argues, since schooling did not have such a remarkable impact on literacy elsewhere in the colonies, it was "intense Protestantism" (in combination with efficient administration of school laws) that was the causative factor (pp. 45, 83). The centrality of Puritanism is further emphasized by contrasting New England's literacy with that of Pennsylvania and Virginia (which never rose much above 67%) (pp. 78-83) and by likening New England to the other contemporary radical Protestant bastions of literacy, Scotland and Sweden (pp. 97-98). Lockridge concludes by admitting that American performance in literacy was not very different from that of England and that "[b]eneath New England's burning Protestantism and consequent universal male literacy, Anglo-America was a world in which literacy moved glacially at a middling level" (p. 97). Thus, literacy in colonial America did not have the modernizing attitudinal impact discerned by Alex Inkeles and his associates in the developing nations of the current era (H. Schuman, A. Inkeles, and D. Smith, "Some Social Psychological Effects and Non-Effects of Literacy in a New Nation," *Economic Development and Cultural Change*, vol. 16, no. 1 [October 1967]).

Two sorts of difficulties are apparent. First, methodologically, Lockridge's use of signature-mark techniques for determining literacy raises a number of problems which will be sufficiently familiar to sociologists. His attempts to control for bias in his sample are careful and thoughtful (pp. 7-13) and less troublesome than his data base, testamentary wills. The bias probably inherent in such a base (we know little about legal behavior in the period) is serious and might have been overcome by using

depositions rather than wills as a source of signatures and marks. Depositions were taken from anyone remotely involved in civil litigation, and a sample of depositions requiring signatures would seem much less likely to be biased as to wealth and social status than wills.

Second, as Lockridge acknowledges, there is little scholarly theoretical support for analysis of literacy in the early modern period. Lockridge is most convincing in criticizing the work of historians who emphasize American uniqueness, competence, and inventiveness, but he is much less convincing in his argument for the causative role of radical Protestantism. Perhaps when data are available on other European societies, we shall be able to agree that religion is the critical variable, but at this point Lockridge's argument is pure speculation. Why, one wonders, did not the "intense Protestantism" of the Carolina back country produce high literacy rates? If the answer has to do with the social structure of "Puritan" society (as the argument as to the enforcement of compulsory school laws suggests), then we will have to look elsewhere than to "religion" for an explanation.

The Participatory Economy: An Evolutionary Hypothesis and a Strategy for Development. By Jaroslav Vanek. Ithaca, N.Y.: Cornell University Press, 1971. Pp. vii+181. \$2.45.

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Industrial democracy as an ideal is even older than modern industrialism, and innumerable examples of efforts to realize it are to be found in the history of the Western world. With the partial exception of Yugoslavia all have failed, more or less, and yet the dream lives on. The Scanlon Plan or some variant of it that would give workers a share in profits is under virtually continuous discussion in the United States. In Europe the corresponding emphasis is on finding ways to give workers a greater voice in running the firms where they are employed. Evidently there is a broadly felt need in the West to bring workers at least part of the way into both ownership and management. And if Liebermanism and similar decentralist movements in the Socialist bloc are an indication, there is a similar sense of need there to broaden the management of firms and incorporate the workers into it.

Yugoslavia has enjoyed exceptionally rapid industrial growth over the past quarter century, during which its economy has been almost fully "participatory," or labor managed. This growth has provided grounds for the claim frequently made in that country that its way to development has been proved to work and ought to be adopted by other less developed countries. Now the author of *The Participatory Economy* proposes that the industrial giants as well as the industrial fledglings should read their destiny—and their salvation—in the logic of Yugoslavia's peculiar institution.

The model participatory economy, generalized from the Yugoslavian case, is neither "command," Soviet style, nor "administered," American style, but "market." The operating unit firm is self-"owned" (it holds its capital on an indefinite loan and usufruct basis), self-managed, and fully decentralized. The whole body of employees is constituted as a workers' collective that hires; fires; determines production scheduling, purchasing, and pricing; and in general performs the entrepreneurial function through a manager whom it employs and controls. Since it is free of banker or state oversight, the labor-managed firm is free to determine what its own self-interest is and how best to pursue it. The decisions it makes on product mix and scale of production and pricing will reflect the place maximizing profits holds in its particular scheme of priorities. And on the distribution side, decisions about how much is to be paid out in wages and salaries and how much retained for investment or other uses is equally unconstrained. The market sets conditions, but what the firm finally decides to do is determined by its own individual assessment of those conditions and the set of values peculiar to it. It may trade off higher income for better working conditions and plant amenities, if it chooses, or plant expansion for community development projects. Finally, the individual employee is free to sever his connection with the firm at any time without loss of benefits or equity capital.

The participatory economy can work in developing countries, as the Yugoslavian experience demonstrates. Annual growth in per capita GNP in Yugoslavia has been among the highest in the world in recent decades, and it is at least arguable that overall modernization has been at a correspondingly high level (and without drastic Stalinist totalitarian regimentation). It seems reasonable to assign much of the credit to the inherently flexible labor-managed firm, with its capability to adapt both to local circumstances and to changing economic conditions in the nation or the region.

Countries still at the bottom or on the lower rungs of the ladder of development need to modernize their institutions, not just to industrialize—more precisely, they need to do the first in order to achieve the second. It may well be that making their economies more "participatory" would conduce to this end. But what of the countries at the top of the ladder, the postindustrials? Is it possible that the Yugoslavian model is relevant for them too?

The Soviet-style command economies of the Socialist bloc and the American-style modified capitalist or semisocialist economies of the West and of Japan do work, but not well. Both suffer from Veblen's "lag, leak and friction" and "conscientious withdrawal of efficiency" (sabotage) in amounts that are massive and apparently growing. The "evolutionary hypothesis" Vanek proposes (counterpart to his "strategy of development") has it that the alienation of youth and of industrial workers in the West and the frustration and rebelliousness of technical and managerial echelons in the Socialist countries are due to forces of growth and change moving the two blocs toward convergence on the new institutional ground

of the participatory economy. After "transition from the right" for the one and "transition from the left" for the other both will achieve the equilibrium toward which they are now tending, in which the key institutional features will be the labor-managed firm and the free market.

A model economy necessarily projects its own utopian image, and the anarcho-syndicalist coloring of this one suggests that the point of departure for a critique of its propositions is the old question of scale. How much management decentralization and worker participation is really possible in the case of national monopolies like ATT or the British Coal Board? Or the multinational giants almost wholly in the private sector, Exxon and the petroleum "sisters"? Or indeed any fairly large, complex, multiple-sited industrial entity? Enough to make meaningful labor-management possible? Except for small or marginally industrialized countries, and the stagnant or declining sectors of the economies of the major countries, would "worker control" be in practice any less mythical than "stockholder control" is in the United States today?

This is a modest book, not the sort in which the author claims he has seen the future and it works. Its value is that it requires a fundamental question to be raised that has been largely neglected since World War II because of the Cold War and other such phenomena: Is the "fix" our industrial order needs after all more institutional than technological? The participatory economy may not as yet be an idea whose time has come, but that it will continue to have a prominent and growing place on any agenda for the future seems certain.

The Beliefs of Politicians: Ideology, Conflict, and Democracy in Britain and Italy. By Robert Putnam. New Haven, Conn.: Yale University Press, 1973. Pp. xii+302. \$10.95.

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Robert Putnam's book pursues one of the most elusive quarries of the contemporary political scientist: the concept of political culture. The hunt for political culture has faltered in recent years, partly because of earlier inferences that were inappropriately drawn from superficial survey instruments, partly because of a continuing inability to place political culture convincingly within a larger conceptual scheme. Only Harry Eckstein has advanced the notion very far in recent years.

Putnam avoids the deficiencies of his predecessors. Instead of relying on a mass sample survey he draws his data from extensive in-depth interviews which he conducted with 176 parliamentarians in Italy and Britain during 1967 and 1968. He reasons—quite correctly, I think—that political culture involves such fundamental orientations toward politics that only comparatively unstructured methods of investigation can bring them to light. Then, out of the wealth of material he has collected, he ingeniously constructs quantitative indices for the three major components of political

culture that concern him: ideological thought, predispositions toward conflict, and democratic beliefs. And, withal, he writes with a style and grace that are nicely complemented by sensitivity to the limitations of his data.

Of course, one cannot help worrying about the validity of interviews conducted with powerful politicians, who might have good reason to conceal their real views or even to prevaricate. Putnam attempts to neutralize these concerns by constantly cross-checking his findings and by estimating the "frankness" of his respondents. He reposes greater faith in his British sample than in his Italian respondents, but he concludes that both groups were generally candid. This is a judgment about which reasonable people could disagree, but, for me at least, the analysis as a whole coheres nicely enough to remove substantial doubt. There are enough loose ends in the findings to convince us that the respondents were not attempting to purvey spuriously neat sets of ideas to an outsider whom they distrusted. Instead, they reveal themselves as fallible politicians making the best of fallible political systems.

But it should not be thought that Putnam is concerned primarily to advance the concept of political culture for its own sake. Rather, he wishes to use the concept as a source of illumination for three particular substantive questions that concern him. First, can pluralistic politics in Western democracies develop a sense of vision? Second, what are the major differences between the political cultures of Britain and Italy, and what consequences do these differences hold for the future of the two polities? Third, what has democracy come to mean in the contemporary West? Let me discuss each of these themes as they emerge in the book.

Putnam begins his study with a long analysis of ideological thought among his respondents. His fascination with ideology unites him with other contemporary political scientists, most of whom believe that pluralistic politics and ideology are incompatible. The resulting unpalatable dilemma has haunted political science for a long time: Must we choose either a polarized politics in which ideological conflict rules the day or a pluralistic group politics lacking any underlying moral foundation? A less pleasing choice would be hard to imagine.

But is such a choice really necessary? This is what Putnam attempts to discover. His conclusion is seductively melioristic. Contrary to the conventional wisdom, he argues that pluralistic politics is compatible with ideology. He bases this revisionist argument upon evidence which suggests that ideological thought among his respondents is unrelated to feelings of partisan hostility or to beliefs about the worth of political compromise. Indeed, he goes further and argues that ideological thought will increase in the future without endangering a politics of pluralism.

To some this important conclusion might appear panglossian. One immediate question, of course, is whether Putnam loads the dice in his favor by defining ideology in such a way as virtually to exclude conflict. He does not. His index of ideological style comprehends such forms of thought as generalization, references to familiar political ideologies, induc-

tive thought, and allusions to salient political traditions and possible utopias. Surely nothing in this index would rule out a politics of pervasive conflict. If there is reason to question Putnam's argument, it is not because of his conception of ideology itself.

Instead, the argument can be questioned on substantive grounds. Two points in particular bear discussion. First, Putnam admits (p. 114) that politicians who see latent conflicts of interest in political issues often turn to ideology, and second, he puzzles about an *aggregate* relationship in Italy between hostility to compromise and proclivity to ideology.

These two anomalies raise questions about Putnam's conclusion that ideology and pluralism can survive nicely together. First, might not ideology serve as an important spur to conflict when, for whatever reason, conflict becomes rife in a society? Second, might it not be true that political culture, when conceived purely as a pattern of individual beliefs, misses much of importance in "national character" and ideology? Putnam is quite sensitive to the second of these questions, less so to the first. But, in any case, he does not lay to rest the possibility that, however subtly, ideology might still work against the politics of pluralism.

What of Putnam's comparison of Italy and Britain? Here the argument is less problematic, for the findings confirm most of our preconceptions about these two political systems. Italian parliamentarians are more ideological, more conflict oriented, and more divided in their conceptions of democracy than are the British. The Italians are attracted either to a social/egalitarian model of democracy or to what Putnam terms "authoritarian" democracy, a model which stresses the citizen's duties to the state and the desirability of self-control. The British, in contrast, are satisfied with the less divisive notions of representation, party competition, and electoral democracy. The ghosts of Locke and Rousseau still haunt the very different political cultures they helped create.

Putnam traces these differences to salient historical events that have shaped national development in the two societies. One cannot help being disappointed at so familiar a set of explanations in so innovative a study. Nor is the disappointment lessened by the fact that only a few of the respondents' background characteristics predict their beliefs at all well. Still, a respondent's class of origin *does* prefigure his views about conflicts of interest in society, and, at least in Britain, parliamentary experience reduces faith in egalitarianism. But Putnam is longer on description than he is on explanation when it comes to comparisons of countries.

Putnam's most important comparative finding is that in both countries younger politicians are less hostile to each other and to the political world in general than are older ones. Basing his interpretations of these findings on a generational view of change, Putnam foresees reduced political polarization in the two societies as time proceeds. His predictions seem generally to have been on the mark. After all, contemporary Italian politics is marked by the Communists' attempt to make themselves parliamentarily respectable, while in Britain all three major parties have come together, despite themselves, on important wages-and-hours policies. Eight years

after Putnam conducted his interviews his predictions of convergence seem to have been borne out.

Putnam's final concern is with the meaning of democracy in these two societies. Unfortunately, his consideration of this subject is occasionally flawed. For example, he has trouble explaining the fact that, despite Britain's long history of democracy, 37% of British MPs seem to have trouble conceptualizing the system within which they operate. What he does not consider is the possibility that democracy as a concept may have little resonance in British culture, while Italians, precisely because of their less democratic past, may find the concept of democracy particularly salient. After all, Italians cannot afford to take democracy so fully for granted as can the British.

More interesting is Putnam's discovery that in Italy a strong commitment to popular participation coexists with conflictual and even authoritarian sentiments. One is led to wonder whether "participatory democracy" must always risk an authoritarian reaction. Is there inevitably a trade-off between democracy, as a set of liberties and procedures, and democratization, as a process by which benefits and opportunities for participation are extended throughout the society? This rather nasty choice is clearly one that Putnam's melioristic view does not comprehend.

It should now be clear that this is a study riven by cross-currents. This is not meant as a criticism. It is precisely because Putnam reveals what other investigators have overlooked—the contradictions in political culture—that his study is so roundly satisfying in so many ways.

Politics and Social Equality: A Comparative Analysis. By Robert W. Jackman. New York: John Wiley & Sons, 1975. Pp. xiv+225. \$14.95.

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The increasing availability of comparative statistics concerning nations ranging broadly in space and time has made possible a macrosocial empirical analysis in which nations are units and characteristics of national behavior are variables. A good survey of the data available was presented by Stein Rokkan and his coworkers ("Nation Building," *Current Sociology*, vol. 19, no. 3 [1971]).

Jackman is concerned with the difficult problem of the measurement and explanation of social equality. The problem is difficult because relevant data are unavailable for most nations at most times regardless of indicators used and because the correlation between equality indicators may be modest. Thus Communist nations seemingly operate with much lower income inequalities than non-Communist ones, in particular for persons outside the leading stratum. On the other hand, power inequality must be many times greater in Communist nations. Jackman chose to define social equality in terms of the distribution of material goods, with primary emphasis on a measure of income equality. As a result only nations for which this

measure is available are in his 60-nation sample, and among the excluded ones are all Communist nations.

A main criticism of the study is that the indicators of social equality used are all of them only indirectly, if at all, measures of the income and wealth discrepancy between rich and poor. Jackman's measure of income inequality compares not individuals, but economic sectors. Although the correlation between sector measures and individual measures is reported from other studies as .8, it may be problematic that one-third of the variance is unexplained.

His measure of social insurance indicates the efforts of nations to establish a safety net or minimum of welfare but says nothing about the welfare span. His third and final equality measure is entitled a "welfare index." It is a combination of information on physicians per capita, chance of being live born, calorie intake per capita, and protein intake per capita. It will be seen that the welfare-index components are all summary measures for the entire nation. They do not provide information on inequality of treatment except under special circumstances.

In his systematic and well-designed analysis of possible causes of social equality the author draws again on comparative statistical data. The study is centered on 1960. It might have been highly desirable to incorporate data on historical changes in social equality, but then the sample of nations would have diminished rapidly with increasing distance from the present.

The author identified six major determinants of social equality. Two of these variables are indicators of rates of growth, respectively, of population and per capita energy production. Both showed high growth rates to be detrimental to social inequality. Possibly the most important determinant identified was the consumption of energy per capita expressed in logarithms, which is the dominant positive influence on both the insurance measure and the social welfare index and which also correlated above .6 with the measure of sector income.

A fundamental problem is the destructive capacity of man. This problem is of increasing importance because constructive and destructive advances tend to go hand in hand, and this in a world characterized by deficient solidarity both within and between nations. Jackman takes up a part of this problem by inquiring into the relationship between his equality indicators and indicators of violence. He seems to consider the causal direction between equality and violence unproblematic: violence creates inequality (p. 99) according to the equation $\text{inequality} = a + b \ln \text{violence}$. His tests showed that the milder forms of collective "antisystem" violence bore no relationship to his measures of inequality. More violent behaviors, however, acted according to the posited semilogarithmic equation, although R^2 was only .10. However, what Jackman has not verified is the direction of influence. I tend to conceive of outbreaks of violence as symptoms of deficient solidarity, which again seems more often to characterize social systems with more than average inequality.

The construction of the book bears some similarity to that of a detective

story. As Simenon proceeds from one chapter to the next, one hypothetical culprit after another is introduced, made credible, and then dismissed or retained until the final revelation. So in this book the author introduces his hypotheses not simultaneously, but sequentially, first carefully pointing out previous backing for each theory, then leading it to the crucial significance test, where it either survives or does not; if it does, it will join the noble set of final survivor hypotheses in the final path-analytic model.

A reasonable deduction from the author's work is that social equality as measured by him will become more prevalent with increasing industrialization. In spite of some hesitations in regard to his operationalizations of equality, I think he has arrived at a most plausible conditional prognosis as far as economic equality is concerned.

A more comprehensive analysis of social equality should include the distribution of power and of information, but Jackman has made a useful contribution by concentrating on aspects of equality most amenable to quantitative study.

American Society in Tocqueville's Time and Today. Edited and with essays by Richard P. Taub with Doris I. Taub. Chicago: Rand-McNally & Co., 1974. Pp. xii+582. \$6.95.

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By now the rediscovery of Alexis de Tocqueville, as a sociologist rather than political essayist or philosopher, has become a continuing affair. As early as 1960 Don Martindale's book on sociological theory pointed to Tocqueville, along with Dahlmann, as signifying an important turn toward detailed empirical study of political and other social processes. It is a new variation, however, to find Tocqueville's early 19th-century commentaries used as a central point of reference for an undergraduate text in sociology.

Growing out of a course taught at Brown University, the book was developed to fill what the editors regarded as a gap—the lack of a volume to serve both as a general introductory text and as a more specific introduction to the study of American society. The stated aim is to fulfill this dual purpose through discussions of data and of the data-gathering techniques used by social scientists (p. vii). No mention is made of the possible relevance of several synoptic works on American society that have been used for undergraduate courses in the recent past.

There are 11 sections written by the editors (comprising 66 of the 562 pages of main text) that comment upon the readings; the latter consist of excerpts from Tocqueville and from various contemporary sociological writings. The main topics treated are demographic and cultural backgrounds, the small town, public associations, bureaucracy and centralization, power in America, "the masses and democracy," art and culture,

social stability, stratification, and racial-cultural relations. A wide range of interesting and important topics is thus brought to the reader's attention in terms of the contrasting perspectives of the 1830s and the present period.

The mode of presentation has several advantages. It continually reminds the student of continuities and contrasts; it raises persisting issues; it possibly encourages a skeptical attitude toward plausible insights until "hard evidence" can be examined.

I found the book interesting to read, and I imagine that many students will share that positive experience. But, perhaps inevitable in such a text-reader, there are gaps and problems of emphasis and balance.

For all the merits of Tocqueville's *Democracy in America* as an evocative and insightful commentary, it is difficult to share completely the editors' enthusiastic appraisal of it as "perhaps the best [sociological treatise] ever written . . . on the nature and functioning of democracy . . . not surpassed by even the best contemporary American social scientists" (p. 5). Neither in Tocqueville's nor in contemporary collections of essays on American society will the student find an explicit, systematic, conceptual scheme in terms of which critically sifted data are organized to describe and analyze the major structures and processes of a total society. The remarkable progress of sociological analysis since the 1930s (let alone since the 1830s) deserves a better hearing than most readers will find in the materials assembled in this book.

A second reservation that must be recorded is that several of the editor-authors' essays did not seem to bear out the explicit intention to introduce sociology through discussion of data and techniques for gathering data. One or two examples may help to convey these occasional problems of "introducing sociology."

There is a question as to whether students who seek models of careful sociological work are well served by such undocumented asides as the following: "In the 1950s, the National Institutes of Health were heavily funded by Congress. N.I.H. [which parts?] began to distribute money, sometimes uncritically, to social scientists interested in health-related problems" (p. 11). At best this is a vague statement with ambiguous implications; to some readers it may imply sweeping defects, although no evidence is cited. (Many serious observers who closely followed the research funded by NIMH, at least during the 1950s and early 1960s, surely would strongly dispute the charges thus implied.) In any case, where are the facts in point?

Again, is it a well-documented truth that "often only someone outside the society (e.g., Myrdal 1962) can perceive clearly the suppressed value premises on which the society operates"? Myrdal's *An American Dilemma* (1944) was based on more than 30 monographs by American scholars, most of whom were sociologists. It is not at all evident that they were less perceptive than Myrdal. It is reasonable and useful to show students that blind spots and biases affect sociological work. But to laud Tocqueville as "unsurpassed" while emphasizing limitations of contemporary American

sociology is to run a high risk of leaving unintended and unwarranted impressions.

Perhaps it is inevitable, too, in a text-reader, that unevenness will be apparent among the various selections in relation to the criterion that analysis of data be well illustrated. In the present volume, a section on "the military-industrial complex" unfortunately lacks any review of the critical analyses by Ritchie Lowry, Stanley Lieberman, Charles Moskos, Sam Sarkesian, Kurt Lang, Lawrence Klein, and Albert Szymanski. Instead the students are given the 10-year-old piece by Marc Pilisuk and Thomas Hayden.

These examples are not intended as carping objections but rather to indicate a generic problem of an introductory text-reader. The felt need to arouse and hold interest often leads to a search for vivid and relatively simple materials. But there is a thin line between "keeping it simple" and a too-ready acceptance of the merely plausible—which often has just become a New Conventional Wisdom. In happy contrast to the last example above is the book's strong section (part 10) on stratification which does not hesitate to draw upon relatively rigorous recent studies.

None of the difficulties noted are unusual, and all can be handled by adequate supplementation. Thus, for teachers who want a flexible set of interesting readings that contrast historical continuity and change in the United States, this work can be a highly useful text.

Symbolic Communities. By Albert Hunter. Chicago: University of Chicago Press, 1974. Pp. 253. \$12.95.

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The career of the neighborhood as a focus of social analysis has been somewhat mercurial—it frequently rises to prominence only to fall to obscurity, often within the span of a few years. The following discussion is intended as an overview of critical research concepts and social theory issues. In turn, I shall relate these concerns to several social policy considerations pertaining to neighborhood intervention and citizen action.

Much of the research on neighborhoods by American and British sociologists centers on the processes of spatial sifting and sorting of populations that cause particular kinds of individuals and economic units to cluster together in metropolitan areas. The first empirical work on urban subareas was done by Robert Park and the Chicago School, where the leading figures included E. W. Burgess, R. D. McKenzie, and J. A. Quinn. By plotting indexes of correlation between, on the one hand, the physical features of the city and, on the other, delinquency and mental disorder rates and racial composition, they delimited "natural areas" of the urban community. This mode of analysis is illustrated by the well-known concentric-zone hypothesis. Perhaps the best-known studies of this type are *Mental Disorder in Urban Areas*, by R. Faris and H. Dunham (Chicago: University of Chicago

Press, 1939), and *Social Area Analysis*, by E. Shevky and W. Bell (Stanford, Calif.: Stanford University Press, 1955). Their methods have been continued in the study of human ecology and urban geography, although a more sophisticated quantitative approach is now used.

In recent years there has been a critical review of the social intervention potential of local residential neighborhoods. This has accompanied a series of significant experiments in political and administrative decentralization in several large urban areas. As yet no definitive evaluation of these experiences has been produced, nor has there been constructed any cumulative or generalizable knowledge base on which to develop such an assessment. As a reaction against the overzealous optimism of the late 1960s and as a result of the restricted flow of governmental funds into new programs of local control and action in the 1970s—coupled with a research thrust devaluing the role of territoriality in urban community integration—the position of the neighborhood is presently in a state of partial eclipse.

Three questions which have dominated both the theoretical and empirical research interests in neighborhood work express the ambiguous role of local residential areas. They may be summarized as follows: (1) What are the elements of a sound and viable neighborhood? (2) For what problems and for which populations is the neighborhood the most appropriate means to social participation and social program intervention? (3) In the urban context, what is the future of the neighborhood as a unit for planning and as a base of community? In fact, all of these concerns are really facets of the same problem of attempting to gauge and control the rise and decline of territorial units within the urbanized areas of American metropolitan centers. Such a basic focus often is translated into language about "social indicators" and "quality of life" of a local area; these are elusive dimensions whose measurement has become of major concern to federal, state, and local public service agencies. In humanistic terms this problem is of growing concern to urban dwellers themselves—whether suburban or inner city, low-income or relatively affluent.

Albert Hunter's work is one of several recent community studies representing a recrudescence of the Chicago School at its home base (see William Kornblum's *Blue Collar Community* [Chicago: University of Chicago Press, 1974] and two books by Gerald D. Suttles, *The Social Construction of Communities* [Chicago: University of Chicago Press, 1972] and *The Social Order of the Slum* [Chicago: University of Chicago Press, 1968]).

This volume reports on an effort to update the seminal studies of Chicago's "natural areas" carried out by Robert Park and Ernest Burgess in the 1920s. Given this rich and lustrous background, the author provides a series of census data analyses from the period 1930–60, coupled with survey interview sampling carried out in 1966 and 1967 with populations of varied size. Hunter focuses mainly on "differentiation and fusion" in the role of local communities resulting from growth and sequential change. Critical to the development of the data analysis is the view that the boundaries of neighborhood and local community are voluntaristically selected

as "definitions of the situation." These areal units provide, in turn, a hierarchy of sizes and functions which are ascribed to such "collective representations" (as the author indicates in his allusion to Durkheim).

Hunter's book is divided into three parts. The first consists of a single chapter reviewing the sampling procedures and the ecological patterns of the neighborhoods in Chicago which form the basis of the analysis. In part 2 Hunter develops his concept of the "symbolic community," compares this with Burgess's "natural areas," and provides the historical framework of his comparative change approach. In part 3 Hunter draws upon data regarding the major voluntary association and organizational patterns of participation in the local community sample. He focuses on federations of neighborhood groups and then reviews the findings on the four decades of demographic change.

The reader is presented a review of various approaches to the meaning of local community. This is then coupled with a series of empirical validations of the thesis that local communities have become "fused" or incorporated "within a larger and commonly shared symbol system . . . one which is markedly inferior to its more varied predecessors" (p. 14).

When Hunter draws the important conclusion that Chicago's "natural areas" show more variability in both how community and neighborhood are defined and what social characteristics are correlated with such perceptions, he provides no benchmarks for assessing how, given the multivariate techniques used, one could have found the opposite. The effort to assert the validity of his central argument is weakened by his not providing the reader with any criteria for determining when "symbolic" social units are *unimportant*. In fact, many of his data indicate only that many people still can name the boundaries of their residential neighborhood; that a goodly portion shop nearby; and that a significant number recreate, worship, and find friends in their proximate social milieu. What meaning should we attach to the findings that blacks and low-income people use a smaller unit of neighborhood than others? Does the finding that high-status blacks have more localized associational memberships than whites of equivalent status suggest neighborhood is more likely to be "community" for this urban population?—a pattern which I found in Detroit.

There is a sterility of analysis in this book and an overuse of conceptual enumeration in place of conceptual development or synthesis. These faults appear to be an outgrowth of a dissertation format. More disturbing perhaps is the retreat from the admittedly nettlesome complexity of the empirical findings and the diversity of local patterns which the author has presented. My discomfort with the final product very much reflects, I believe, its unsatisfactory attempt to fuse traditional community observation studies with survey interview techniques. Such a task is a very difficult one, and in this case the very stilted and premature attempt falls short of the mark.

In pointing out the need to recognize and examine the variability which still persists in local residential environments, Hunter has asked those who take seriously such a thesis not to rest with a handful of case analyses or with immersion in the exotic and ethnically unique. Rather, we must con-

sider the role of neighborhood in a broad and comparative fashion, using a full range of methodologies. We cannot merely bemoan the passage of the "urban villager" or of the ethnic enclave; we must investigate the forms of neighborhood that now predominate—and treat them in multidimensional ways. This is for me the critical, albeit garbled, message of Hunter's analysis.

Governing Greater Stockholm: A Study of Policy Development and System Change. By Thomas J. Anton. Berkeley: University of California Press, 1975. Pp. xxii+237. \$12.75.

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This is the third in a series of books being published by the University of California Press which analyze major metropolitan political systems in the world. (The preceding texts are Donald L. Foley, *Governing the London Region: Reorganization and Planning in the 1960's* [Berkeley: University of California Press, 1972], and Albert Rose, *Governing Metropolitan Toronto: A Social and Political Analysis* [Berkeley: University of California Press, 1972].) Having read these first three books, let me merely say that, as a group, they constitute an excellent empirical resource on urban and metropolitan political systems by providing systematic social histories of metropolitan reorganization. I would strongly recommend their use as supplemental texts for courses in urban sociology, community power, metropolitan government, and urban planning.

The creation of metropolitan government in Greater Stockholm has often been considered a prototype of successful regional planning. Anton correctly points out that the historical growth pressures and problems of political fragmentation in the Stockholm region are similar to those of many large urban areas in the United States. Yet Stockholm's metropolitan government structure is far in advance of any which has developed here. Why is this the case, and how can we account for this particular pattern of reorganization? To address these questions, Anton presents a historical perspective of the political structure of metropolitan planning.

Through integrated longitudinal analyses of housing, transportation, and general planning issues the reader is given important insights into the structure and process of decision making. Anton's study makes it quite clear that reorganization was not a facile system accommodation to pressing mutual problems, but a complex political game in which a variety of individual and collective interests were in sharp opposition. Anton carefully describes these conflicting interests but also depicts a series of cultural and structural conditions which facilitated successful reorganization—conditions which do not have readily documented parallels in the United States. These facilitating conditions included Swedish social values which, upon close inspection, did not reinforce "affective investment" and high citizen involvement in local community affairs but did stress convenience

in housing and accessibility to work and entertainment; a dominant, long-tenured, and politically innovative Stockholm government elite; a strong public sector involvement in the land market; a clear penetration of the national government to local community affairs; and an asymmetry of power resources in Stockholm's relations with suburban municipalities. These and other conditions are given systematic treatment in the text, and metropolitanization is depicted largely as their evolutionary by-product.

Anton is quick to add that, because these facilitating factors were specific to the Stockholm experience, the planning policies which resulted should not be interpreted as guidelines for planning efforts in the United States or other areas where the constraints are likely to be different. What then, for theory or practice, can be gleaned from research of this type? On the applied side, Anton observes that we need more intensive case studies so that comparative themes may be uncovered and potential guidelines offered. Of course, that is one important justification for the present series. In carrying out this research effort, Anton suggests, a number of important political issues should be addressed: the limits of centralized metropolitan administration in democratic systems, the assumptions about local control in complex societies, the role of national governments in accommodating local and regional interests, and the costs/benefits of power transfers in light of cultural and structural constraints. There are admittedly no easy prescriptions.

On the theoretical side, Anton argues quite correctly that planning models which link land-use patterns to organizational and interorganizational relationships are needed. For example, William Form's useful organizational conceptualization of urban land-use markets (cited by Anton) provides, at best, a loose framework for comparative research (William H. Form, "The Place of Social Structure in the Determination of Land Use: Some Implications for a Theory of Urban Ecology," *Social Forces*, vol. 32 [May 1954]). More systematic theoretical work is clearly required. Although not tied to land use, there is a growing body of literature of complex organizations which treats interorganizational power processes directly, and urbanologists are well advised to begin examining this literature for conceptual insights.

In summary, I found this book well worth reading. The general topic of metropolitan reorganization is of considerable theoretical interest to a number of disciplines and of obvious public importance. Given the complexity of the political processes involved, we need the firm empirical foundation that carefully developed case studies can provide. Anton's research is an informative social history of the metropolitanization of Greater Stockholm.

Easter Hill Village: Some Social Implications of De
Cooper. New York: Free Press, 1975. Pp. xxiv+337. \$1

Joel M. Margolis

University of Chicago

It has been said that architecture is a profession that little social consciousness, especially regarding the ne the more modest classes of the population. Though th true in the past, there are signs that such a situation is Cooper's book is definitely a part of this change; it ma tribution to advancing both the trend itself and our und is needed to design residences to better meet human ne

The book approaches the subject through the method tudes. Says the author: "It was to investigate possible tween designer's assumptions about what residents wan and what residents actually want, that this study w: Spring of 1964" (p. 9). The comparison began with a : to determine what the architects of a 300-unit developm people thought were the future residents' needs and, sp sign features they chose to try to meet those needs. architects felt each family would want a "home of its (assumptions involved in this included the ideas that house, not an apartment, and that the dwelling units visualized as possible, not standardized and identical. T chosen in an attempt to reach these goals included the t of row houses (rather than apartments) and slight va appearance and setback from one house to another.

A series of questions was then asked of the residents their desires in regard to such issues and attitudes about features. The author found that the overwhelming majo spond very favorably to the idea of a home of one's ow saw individualization as an integral part of this concep various attempts at individualization of exterior appea architects went largely unnoticed by the residents, wit variation in color.

The author has constructed quite a comprehensive se parisons, although some of the observations, it is tru mentary ones (e.g., the fact that the overwhelming ma expressed a preference for a detached, single-family hou is not an indictment of the author, who has done a thor ing residents' attitudes on a variety of issues; rather, it the very modest state of the art in the sociology of resid It is to be expected that a number of observations whic reader as rather elementary will be made in the cours or early steps in any field.

Much more significantly, as a result of her many co:

thor has produced a great deal of very useful and practical information as to how designers might better meet the needs of future residents of multifamily housing developments, particularly in the case of lower-income residents. Among her major conclusions on the general level one in particular contributes significantly to developing a more sophisticated awareness of the problems of design from a social standpoint. She points out that architects already know a great deal about the requirements of interior home design and overall physical structuring; yet she quickly adds that at Easter Hill Village the major spatial-social problems often lay outside the home: the lack of properly designed children's playlots, the incorrect planning of certain pathways and intermediate outdoor spaces, and the like. She argues, therefore, that one of the greatest needs in the field of multifamily design is that of better understanding of the functions and requirements of outdoor space. In view of her substantial evidence on the problems at Easter Hill caused or exacerbated by outside ground arrangements, it is a cogent as well as insightful argument.

The list of suggestions is too long for reiteration, and, moreover, many of them contain little that is of explicit sociological import. However, it is appropriate to note what was the nature of the architects' most frequent type of mistake. They were correct in many of their general assumptions, for example, that the residents would want a noninstitutional setting, and that each would like control over a piece of private outdoor space. When they did fail, it was often because they were mistaken or uninformed as to specific features of poor people's life-styles, their daily habits and tastes—for instance, their tendency, as compared with the middle class, to use the kitchen much more than the living room. The author points out that the socioeconomic background of the designer is (at least in the case of multifamily housing for low-income people) very often different from that of the residents and, on top of that, given the way the architect-client relationship is structured today, the architect has little or no incentive to do follow-up work, that is, to investigate how well (or poorly) his design solutions worked according to the standards and judgments of the occupants. One cannot help but speculate that there is some sociologically interesting data in the fact that this call for greater attention to (and protection of) the residential consumer comes at a time similar demands are being made with regard to numerous other types of products.

In the course of analyzing user evaluations, the author makes a number of points that speak to various questions in the sociology of housing and to wider sociological issues as well. For instance, the residents' reactions to a variety of features led to several pieces of evidence supporting Rainwater's thesis that the lower classes see the home as a haven or refuge from the threats of the outside world. On a broader level, the book suggests there are limits to using physical design as an influential or "corrective" factor in human behavior; in doing so, it contributes to the long-standing debate over whether the behavior of the poor is culturally or situationally determined.

The reader who is not architecturally inclined may be frustrated by the

fact that some of the more sociological material is scattered. However, the author's primary purpose was the investigation of user evaluation of the physical environment, and she does an excellent job of it; she cannot be expected to provide lengthy, separate explorations of peripherally related sociological points. For the reader who is both architecturally inclined and sociologically oriented, though, the book is a valuable piece of research that skillfully blends two areas and in the process makes a contribution to each.

Gypsies: The Hidden Americans. By Anne Sutherland. New York: Macmillan Co., Free Press, 1975. Pp. x+330. \$13.95.

William C. McCready

National Opinion Research Center

I was sitting in the emergency room of our local hospital, waiting for my three-year-old daughter to have the results of a recent adventure repaired, when I noticed a commotion at the desk behind me. A thickly accented voice was saying, "His name is Mitchell and we will pay in cash." Just as I turned to get a better look, the emergency room was inundated with Gypsies; real, live Gypsies. I had just finished reading Anne Sutherland's book for this review, and these people seemed to have walked in right off the pages. The use of American-sounding names; the readiness to deal in cash; the way they intimidated the staff without really upsetting things—all of these were typical of the Gypsy behavior reported by Sutherland.

Gypsies: The Hidden Americans is definitely not an "apologia for why gypsies steal a lot," as it was described in the *New York Times* book review of November 8, 1975. In fact the opposite is true. Sutherland goes to great pains to explain that much alleged Gypsy stealing is mythical and exaggerated. In the incident mentioned above the Gypsies were reputed to have taken three crucifixes from the hospital during their brief stay. Subsequent inquiries failed to verify the fact; people see what they expect to see.

Gypsies are a social enigma. Like the recently explained enigma of the so-called Bermuda Triangle, facts about Gypsy life are more than outweighed by the rumors and half-truths that help to preserve the enigma. Sutherland has written a book filled with facts, and it is a good beginning toward an understanding of a way of life that has successfully remained hidden within our society.

The information in this book comes from two years of fieldwork in Barvale, California, during part of which Sutherland was the principal of the Romany school, and intensive interviews with a Gypsy leader. Gathering data among Gypsies is like trying to get a straight answer from an Irishman. For a Gypsy, the only kind of lie that is really a lie is one told to another Gypsy; those told to non-Gypsies are not really lies at all. Therefore it is useless to think of surveying Gypsy attitudes. Sutherland overcame this obstacle by developing a complex system of cross-checking

her informants. It seems that although Gypsies take to lying as a bear takes to honey they seldom bother to keep track of each other's lies. When the author would catch them in their deceptions and confront them with her discovery, they would usually applaud her gleefully and praise her developing sense of "Gypsy" deviousness.

Sutherland reports on many aspects of the life she found among the Gypsies of Barvale, among them family and marriage customs, economic relationships with the outside world, and leadership within the Gypsy community. There is a detailed explanation of the social organization of the *kumpania* which provides many insights into Gypsy behavior and motivation. The underlying drive in Gypsy life is the preservation of the heritage and traditions of the *Rom*, or the Gypsy people. Their pattern of isolation within society is directed to this goal. Confounding the police and other outside authorities, and weaving through the welfare bureaucracies with the skill of a Monopoly champion, are highly prized Gypsy talents. The following passage exemplifies the way in which Gypsies will *use* approved social authorities:

Also the police have been, on several occasions, very helpful in collecting brideprices. Many of the girls are married very young, in non-recorded Gypsy ceremonies. If the husband's family reneges on the agreed upon brideprice, the bride's father has the recourse of going to the police and swearing that his minor daughter has been kidnapped for immoral purposes. He, of course, will not mention the fact that his daughter was married in a Gypsy ceremony with his consent. The daughter will be located and returned to her father, and the husband will be charged with statutory rape. If the husband's family then pays up, the charges will be dropped, the girl will be returned to her husband, and all parties will generally disappear, much to the mystification of the Police Department. [P. 108]

An essential point that emerges from Sutherland's observation and analysis is that Gypsies consider all *gaje* (non-Gypsies) to be so much Gypsy fodder. We are like the natural resources of the earth as far as they are concerned. As the bison were by the Indians, we are meant to be used, exploited, conned, and generally battened on. We are essential to their survival in this sense only. Increasing problems with literacy and our regulated society will continue to threaten the Gypsies, but one gets the feeling that they will continue to find new ways to use us and our institutions.

This is a competent and interestingly written book about a fascinating subgroup in our society. It should interest the general reader as well as the specialist in subgroup research. It would also be a useful supplementary addition to a reading list for a course in ethnicity and race. Most of all it sheds some light on a mysterious people about whom there are more myths than facts. Sutherland's eyes are those of the outsider, like ourselves, and as such she notices things in a way that makes sense to us. We get a glimpse of Gypsy life, and it begins to lose some of its mystery yet none of its fascination. In her final chapter the author speculates that

we may see the rise of militant Gypsy groups seeking their fair share in the minority sweepstakes. If that happens, their press conference ought to be a real show.

Maltese in London: A Case Study of Erosion of Ethnic Consciousness.
By Geoff Dench. London and Boston: Routledge & Kegan Paul, 1975.
Pp. ix+302. \$21.00.

John A. Jackson

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This is the sociology of a bad reputation gained by a small immigrant population of Maltese in a period of some 25 years up to 1965. The reputation is for criminality and organized prostitution; the stereotype is the ponce, exploiting one of a number of small rackets in the East End of London. Geoff Dench uses this case study of immigrant experience to test the assumptions of the "liberal" model of immigrant behavior and its implications for the reassessment of the "open society."

The Maltese in Britain are numerically a small immigrant population; amounting to some 5,000 in 1951, they have probably never exceeded a maximum of something like 13,000 in the mid-1960s. Of these the majority have settled in London; small settlements exist in a number of other cities. Although the size of the community limits the development of an extensive organization, it certainly does not explain the remarkably low level of community cohesion that Dench finds. Nevertheless the demographic factors do indicate some of the reasons. Maltese immigration to Britain has primarily been of young and unattached men whose motives in emigrating included elements of escape and adventurism and sexual frustration arising from conditions in Malta. These background factors together with the ease with which most migrants adapt to Britain and the high degree of intermarriage with English partners give little structural support for community development. Where a Maltese consciousness tends to develop at all it is in the social activity centering on Maltese cafés. This, however, is basically anarchistic and represents a rejection of the religiously based community life in Malta. As Dench points out: "In the atmosphere of unrestrained hedonism and materialism which suffuses café society, migrants learn a defensive hardness and cynicism" (p. 46).

A basic reason for the lack of development of a community consciousness among the Maltese really lies in their "bad" reputation, arising out of a "voluntary specialization" by some Maltese in the sex trade. This stems at least in part from the situation in Malta where these young men experience considerable frustration both from the lack of integration of the bachelor in the rigid traditional structure and from the exploitation of Maltese women by the British garrison. Dench suggests that in Britain Maltese men are able to act out a Black Apollo role of retribution. They are already in a state of adolescent rebellion when they enter Britain;

they experience no particular culture conflict with the host society, and they experience release which takes the form of disorganized and individualistic criminality and delinquency.

This situation has led in turn to a collective stereotype of the ponce, which has formed the basis of prejudice and discrimination against the Maltese by the host community. Within the community it has led to a rejection of the notion of collective responsibility and a further weakening of any sense of community support. In contrast to the conventional model of minority reaction to external blame and victimization, the Maltese reaction is to reinforce those tendencies which lead to disorganization within the group. The ease of adaptation to life in Britain and the individualistic nature of the migration have the effect of preventing the Maltese from forming even the basis of a minimal solidarity and certainly not one that could exercise effective social control over its members.

A case study such as this provides an appropriate test of the real nature of the attitudes and ideology of the receiving community. In his discussion of liberal ideology and individualism in Britain Dench suggests that the liberal tradition carries with it inherently racist overtones which until recent years were concealed in Britain itself by the existence of the Empire. The focus on Britain and the impact of immigrants on British society have the effect of straining the fabric of the liberal ideology. As he suggests, liberalism both defines and creates the problem of racialism: it defines it by emphasizing individual as against collective bases of social order and releases it by giving rise to more personal aspirations than can be fulfilled, thus leading to competing group interests.

Dench has opened the ground for a very interesting discussion here, but unfortunately he has not developed it far enough in his concluding chapter. The direction the discussion follows leaves the Maltese case something of an anachronism since the Maltese immigrants are never likely to develop the kind of group basis for competition that he is referring to. The final chapter therefore sits uneasily between the analysis of the case study of the Maltese and the changing structure of relationships within Britain itself. More could have been made of the experience of other immigrant groups in Britain, especially those like the Irish and the Scots which have some of the same features as the Maltese in their relationship with Britain and also open up some of the assumptions about British identity and ideology. Such comparative reference could have served to deepen the welcome attention paid in this study to the impact of immigration on the receiving community and the interrelationship between homeland and immigrant community which this author stresses in his study of the Maltese in Britain.

Hutterite Society. By John A. Hostetler. Baltimore: Johns Hopkins Press, 1974. Pp. xvi+403. \$14.00.

Marvin P. Riley

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The Hutterites are of special interest to social scientists, scholars of diverse interests, and laymen in that they constitute a unique farm society in the midst of the modern industrial world. John Hostetler offers a comprehensive account of the oldest communal society of a family type on this continent. This ethnography of the Hutterite Brethren is well balanced, covering the sect's history, religious beliefs, contemporary social and cultural organization, and their techniques for survival. The work is especially welcome because it arrives at a time when communes have both topical and practical relevance. It is also timely because the year 1974 marked the one-hundredth anniversary of the first settlement of the Hutterites in North America.

The Hutterite Brethren are a religious sect who in 1974 lived in 229 colonies with a total population of nearly 22,000. The colonies of the Brethren are situated in the prairie states and provinces of North America. Although they originally settled in Dakota Territory when they came from Russia in the 1874-79 period, at present about three-fourths of the colonies are located in the Canadian provinces of Manitoba, Saskatchewan, and Alberta. The remaining one-fourth are in the states of South Dakota (35), Montana (27), North Dakota (3), Washington (2), and Minnesota (1).

Today the Hutterites still live communally, sharing all their worldly goods, and isolate themselves from the main currents of Western civilization. Although the strong boundary maintenance of their social system limits their social contact with the outside world, they maintain a high level of modern agricultural technology. That the Hutterites have been successful in adjusting to the geography and the economy of the region is borne out by the fact that their population and colonies are flourishing to the extent of doubling every 14-20 years.

Hostetler's *Hutterite Society* provides the reader with a reliable, detailed guide to the religious basis of the Hutterite communal life, their colony organization, their contemporary life-styles, and their strategy for survival.

At the outset, he traces the historical development of the Hutterites from their early sectarian beginnings in Moravia during the Protestant Reformation. Two features are particularly outstanding in his treatment of Hutterite history. First, it is one of the best organized sequential accounts of the four centuries of Hutterite history in the English language. A second noteworthy feature stems from the enrichment added to the European historical record by Hostetler's personal examination of Bruderhof sites combined with his archival research during his year in Europe in 1970. Photographs of these locations and footnotes on recent archaeological findings are skillfully blended with the historical record.

Although Hostetler next deals with the contemporary social and eco-

conomic organization of the Hutterites, this section is appropriately introduced by a discussion of the *Weltanschauung* or "world view" of the Brethren which by necessity emanates from their historical roots. He emphasizes that it is the belief system of the Hutterites that undergirds the whole of Hutterite society, providing motivation, substance, meaning, and direction for their lives. Their total belief system rests on the fundamental notion that absolute authority resides in a single omnipotent God who is spiritual, unchanging, and eternal. This spiritual realm stands in marked contrast to the *Weltgeist* or "world spirit" consisting of the transitory, changing, and temporal aspects of the material world. Man was made to worship God, not the things made by God, the material world. Thus the orientation of the Hutterites is toward life after death. Their belief system provides the goals or ends for the group, and their communal form of social organization becomes the means for achieving those ends. This belief system is the foundation for colony organization and the Hutterite way of life.

Hostetler then moves into a detailed examination of their contemporary social and cultural organization. He is well qualified to deal with this topic. His knowledge of the literature on the Anabaptist movement, both in German and English, is broad, and his earlier work on the Amish provides an excellent backdrop for the study of Hutterite society. He draws extensively upon contemporary research on the Hutterites, including his own special studies of their socialization and formal education. These findings are buttressed with insights gained from frequent personal contacts with Hutterites and their leaders both in Canada and the United States.

In the third and final part of *Hutterite Society*, Hostetler undertakes the arduous task of trying to answer the tough question, "Why do the Hutterites continue to survive as a viable social system?" He sets forth five factors which he considers fundamental to the Hutterite strategy for survival: (1) uncompromising beliefs, (2) comprehensive socialization, (3) reconciliation of delinquents, (4) biological vitality, and (5) the management of innovation. Followers of "functionalism" in sociology will be heartened to note that Hostetler's five factors in the Hutterite strategy for survival come close to the functional prerequisites often considered necessary for the survival and continuance of any society.

Hostetler has performed a yeoman's task in collecting and presenting the most up-to-date information available to him on the number, location, distribution, and operations of the Hutterites and their colonies in the different states and provinces. Combining this information with his own research, he provides a well-rounded definitive picture of contemporary Hutterite life.

Women, Wives, Mothers: Values and Options. By Jessie Bernard. Chicago: Aldine Publishing Co., 1975. Pp. viii+286. \$15.00 (cloth); \$5.95 (paper).

Ann Oakley

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The vantage point from which one approaches a new book by Jessie Bernard is, in a sense, already charted: her impressive corpus of publications on the situation of women provides a map of instant expectations for the reader (academic or otherwise), and, needless to say, the reader is not disappointed. The impossibility of summarizing her distinctive contribution forces a reviewer to try. At least in the context of the mass of literature "on women" that is being produced at the moment, her books emerge triumphant for their skillful integration of statistics, concepts, theories, and individual case histories (not too much of one or the other); for their sense of progressive dynamism (her views are constantly subjected to self-critical scrutiny); and, perhaps most of all, for their piercing sense of history. The task, for Bernard, is not to describe the position of women at one particular moment in time, but rather to see how this moment relates to others. The reader has an impression of being taken on a flight over a territory which, because it is too familiar, is also unknown. Distance, an overview, is needed to bring its boundaries and landmarks into focus.

The tendency to consider women as a separate category in academic and popular thinking obscures the notion that the "problem" of women is historical and ecological. Bernard insistently and valuably reminds us of this. The reminder is particularly apposite in the British context (I am now speaking as a British academic), since most of the available British material is either narrowly programmatic (e.g., Lee Comer, *Wedlocked Women* [Leeds: Feminist, 1974]; Michelene Wandor, ed., *The Body Politic* [London: Stage I, 1973]); limited to specific models/situations (e.g., Hannah Gavron, *The Captive Wife* [Harmondsworth: Penguin, 1966]; Alva Myrdal and Viola Klein, *Women's Two Roles* [London: Routledge & Kegan Paul, 1956]; J. M. Pahl and R. E. Pahl, *Managers and Their Wives* [London: Lane, 1971]; Rhona Rapoport and Robert Rapoport, *Dual-Career Families* [Harmondsworth: Penguin, 1971]); or concerned with the theoretical challenge that the emerging sociology of women presents to the existing state of the discipline (e.g., D. H. J. Morgan, *Social Theory and the Family* [London: Routledge & Kegan Paul, 1975]).

Women, Wives, Mothers is basically a collection of papers and articles. Its principal theme is motherhood: two central sections focus on the preparation of women for motherhood and the performance of the mother role over the life cycle. Certain crucial paradoxes are exposed: reared to dependency and weakness, girls find that as mothers they must be a source of strength; motherhood has its different ages and stages and is not the unitary experience its cultural image suggests. A review of labor-force participation rates and the new feminist critique of motherhood leads Bernard to observe that research on motherhood now faces a "paradig-

matic" crisis. Two established perspectives for analyzing motherhood—the demographic and the nuclear family—have policy orientations which are out of tune with the patterns of motherhood (parenthood) currently emerging in society.

Bernard believes that we are living through an unrecognized revolution and that before our blind eyes the entire sex-role structure of society is being reworked. Certainly some of the data she presents here support this contention. For example, it is clear that, in the United States at least, a woman with school-age children who has no paid work role is now "deviant," whereas until very recently it was her employed sister who, statistically speaking, was in a minority. Bernard's notion of a "tipping point"—a time at which what was formerly typical and modal becomes deviant, or vice versa—is a valuable addition to our impoverished conceptual armory in the sex-role field, beset as it is by a somewhat static idea of norms.

But there are problems attached to the deduction of a revolution in sex-role behavior and ideology from statistics such as labor-force participation rates. The tendency of a traditional sex-role ideology to survive changed conditions and apparently new behavioral patterns is well established. Employed mothers are not necessarily more liberal in their ideas about "proper" masculine and feminine behavior than nonemployed mothers (Harriet Holter, *Sex Roles and Social Structure* [Oslo: Universitetsforlaget, 1970], pp. 74–78). Greater domestic work participation on the part of men in dual-job marriages does not necessarily imply a revamped ideology about the sex appropriateness of domesticity (Lois W. Hoffman, "Parental Power Relations and the Division of Household Tasks," in *The Employed Mother in America*, edited by Ivan F. Nye and Lois W. Hoffman [Chicago: Rand-McNally, 1963]). And in other cultural contexts where alterations in sex-role behavior are matched by an economic revolution and an official societal commitment to sex equality, ideological chauvinism persists (H. Kent Geiger, *The Family in Soviet Russia* [Cambridge, Mass.: Harvard University Press, 1968]). Even the nature of the changes in behavior needs to be taken into account. For instance, a more active and involved performance of fatherhood is quite compatible with an enduring notion of *feminine* responsibility for child rearing and the domestic work activities that this involves (Ann Oakley, *The Sociology of Housework* [London: Robertson, 1974]). So far as labor-force rates are concerned, it is essential to dissect the kinds of jobs men and women do (has the sex division of labor in employment changed?) and to examine the financial power structure (are women still paid less than men? how do they fare in the pension market?—etc.).

In other words, just what kind of sex-role revolution are we experiencing? Part of the difficulty inheres in the common linguistically induced delusion that in talking about "women" (or "men") we are denoting identical people rather than groups of people who may or may not have interests and life-styles in common. As Bernard puts it: "It is one of the most serious casualties of our research biases that our concern with inter-sex differences has all but overshadowed our interest in intrasex differences"

(pp. 31-32). It is not easy to see how we can cease talking about "women" and "men," but more attention to the differential meaning a job, for example, may have to mothers of school-age children is likely to supply us with a more rounded picture of the kind of change that is (or is not) occurring on both behavioral and ideological levels.

In *Women, Wives, Mothers*, on either side of the central sections on motherhood, are two sections which contain many of Bernard's most penetrating insights. Part 1, which is concerned with research on sex differences, will, I suspect, be read by many people who will not consider themselves interested in the rest of the book. (It should be required reading for all researchers on sex differences.) The last part of the book, which looks at "The Wider Scene," ends with a consideration of changes in the relationships between women, work, and motherhood, on the one hand, and, on the other, the failure of society to evolve a satisfactory social and economic support system to accommodate these changes. In the traditional domestic context, women formed their own mutual aid society. The contemporary context lacks this, and Bernard suggests that women's difficulties in adapting to the new patterns of behavior (mother *and* worker) would be eased by the provision of "generalised drawing rights" to a fund of money which will supply various forms of financial help. It is, as Bernard observes, in their economic powerlessness that women are most vulnerable, but one is left at the end of her discussion with a feeling of unease: surely a sex-role revolution, to be more than simply reformist, must call for some more dramatic alteration of the economic power base of female-male relationships.

Leisure and the Family Life Cycle. By Rhona Rapoport and Robert N. Rapoport, with Ziona Strelitz. London and Boston: Routledge & Kegan Paul, 1975. Pp. x+386. \$26.50.

Max Kaplan

University of South Florida

This volume marks a pioneering contribution to family studies, to our understanding of leisure, to policy formation for related services, and—mainly—to the interaction of these spheres. The Rapoports, codirectors of the Institute of Family and Environmental Research, London, are joined by anthropologist Ziona Strelitz in this study funded by the Leverhulme Trust.

Their purpose is explicit: "To suggest a perspective that supplements rather than replaces the perspective of social class and social change. . . . This is the perspective of the human life cycle" (p. 14). The life-cycle concept, basic to family studies, has been strangely underexamined by students of leisure.

A full chapter is given to each of the four stages: young people, young adults, the establishment phase, and later years. All of these chapters follow the same form: theoretical discussion, in-depth case studies, and fur-

ther interpretation based on the concepts to be noted in a moment. The focus is for the most part on individuals from an area of study of a housing section in a South-East English industrial town. The first and third stages receive the most attention; the 290 pages devoted to them grow out of a theoretical formulation in the first chapter that is crucial to the success of the study.

The concept of need is rejected as the basis of such social changes as the reduction and new flexibility in work hours, the turn to qualitative aspects of life, the "leisure explosion," or the "palpable mass demand" for more of the same. Instead, three interrelated kinds of influence are proposed: the "planes" of work, family, and leisure. The unique combination of this "triple helix" at the critical time of transition (the "unfreezing" of established patterns) provides the character of individual life-style at any point of life. Each of these lifeline strands undergoes a "career" that affects other strands, and each is rooted in a threefold dimension of motivations: preoccupations (mental absorptions), interests (ideas and feelings about what people want), and activities (spheres of action). With regard to young people, for example, the authors see some preoccupations as autonomy, stimulation and boredom, work; interests can be summed up as "doing one's thing," or variety; characteristic activities are arts and crafts, dancing, mountaineering, and making music. From this analysis, they argue for a public policy that allows for "the development of interests appropriate to a given phase and situation, but also for the more generalized development of resourcefulness . . . to be able to adapt continuously and cultivate new interests" (p. 28). Thus the analysis of shortcomings in present public policy and the prescription of desired services and new philosophy become inevitably central to the thesis of the book.

This review cannot do justice to the rich detail and insight that are brought from the introductory theory of each life phase. Wisely, sharp lines between these phases of life are avoided, and the total family dynamic is never forgotten.

The study of leisure has in recent years produced a torrent of publications. In many countries—eastern and western Europe, Japan—there are university or academy teams whose surveys or data are closely followed by governmental and welfare agencies (adult education, community planners, personal and family counselors, etc.); in my travels, I have found that the Socialist nations and others seem to be seriously concerned with making basic choices about the proportion and nature of consumer goods (as in Poland), television programming (Rumania), or the protection of traditions vis-à-vis technology (Iran). Several years ago an international association of recreationists proclaimed a Charter for Leisure; the Van Cle "Free Time and Self-Fulfillment Congress for a Charter of Leisure" (Brussels, April 1976) will issue a similar statement to emphasize the "rights" of publics in these regards. The difficulty up to this point has been more than the strength of work values, or the seemingly undramatic reality of the "new leisure" in contrast to wars, famines, internal political struggles, or world inflation. The facts, over a long period of time, do

indeed point to increased cybernation and radical transformations in work patterns and attitudes. The leisure movement, if that is what it is, has only in recent years begun to move across disciplines, across cultures, and through such institutions or segments of interest as work, family, leisure, community, and welfare services. There has been some attempt recently, especially in the United States, to develop a psychology of leisure; such professional areas as welfare, education, and the arts have not yet recognized the issue. Now this volume from England has brought anthropology and psychoanalysis full force into the convergence of family and leisure concerns, and sociologists will find not only that their dominance of the leisure field will be complemented at the level of theory but also that more effective bridges will be provided to such fields as gerontology, community recreation, counseling, and various forms of therapy.

Popular Culture and High Culture: An Analysis and Evaluation of Taste.
By Herbert J. Gans. New York: Basic Books, 1975. Pp. v+179. \$10.00.

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Although he is perhaps best known as an urban sociologist and planner, Herbert Gans is also one of our keenest observers of popular culture. His informative movie reviews have appeared regularly in *Social Policy*; and over the past 20 years Gans's articles on such diverse topics as the functions of television in American society, the creation and changing content of popular films, the composition and perceptions of the mass-media audience, and the production of television news have appeared in a variety of publications.¹ *Popular Culture and High Culture* (PC & HC) is not an anthology of these earlier contributions (which would also be welcome), but instead grows out of an essay, now revised and expanded, originally entitled "Popular Culture in America: Social Problem in a Mass Society or Social Asset in a Pluralist Society?" (in *Major Social Problems*, ed. Howard S. Becker [Wiley, 1966]). The book is provocative, stimulating, and of major importance to sociologists concerned with the fine arts, the mass media and their enormous audience, and American culture. In some ways, it also provides an intellectual bridge between Gans's better-known

¹ See, for example, the following articles by Gans: "Mass Communications as an Educational Institution," in *American Education in the Electric Age*, ed. P. Kling (New York: Educational Technology, 1974), pp. 58-76; "The Creator-Audience Relationship: An Analysis of Movie-Making," in *Mass Culture*, ed. B. Rosenberg and D. M. White (Glencoe, Ill.: Free Press, 1957), pp. 315-24; "The Rise of the Problem Film," *Social Problems* 11, no. 4 (1964): 327-36; "Some Changes in American Taste and Their Implications for the Future of Television," in *The Future of Television*, ed. S. Donner (Stanford, Calif.: Stanford University Press, 1965), pp. 35-50; "Consumer Goods and the Mass Media: Selective Acceptance of the Outside World," in *The Urban Villagers* (New York: Free Press, 1962), pp. 181-96; and "How Well Does Television Cover the News?" *New York Times Magazine* 119 (January 11, 1970): 30-45.

work concerning the communal bases of suburban living, urban planning, poverty, and public policy, and his more occasional forays into the types of popular culture subscribed to at different levels of society.

Gans begins by observing that where a society exhibits a heterogeneous, stratified social structure one should also expect to find a corresponding diversity of differentiated "taste cultures," each correlated roughly with the class and status characteristics of its adherents. To the (large) extent that the distinctive culture of each demographic segment is a function of that group's position in the social structure, and to the extent that each culture reflects rather than transforms the predispositions of its adherents, it is unrealistic to demand that all members of a heterogeneous society subscribe willingly to a single taste culture. In part 1 of *PC & HC* Gans attacks critics of mass culture—or, more topically, of the portrayal of sex, violence, and news by the mass media—for making just such a demand. He develops the argument that, in effect, "mass" and "high" culture are loaded concepts, employed by one high-status group to assert the superiority of its own taste culture over the cultures of all those less educated or privileged. Echoing Weber, he believes that as professional sociologists we must remain evaluatively neutral about aesthetic merits and put aside our own (SES-determined) personal taste preferences. Once this is done, we see that "all taste cultures are of equal worth. . . . Because taste cultures reflect the class and particularly educational attributes of their publics, low culture is as valid for poorly educated Americans as high culture is for well educated ones, even if the higher cultures are, in the abstract, better" (p. xiii).

High culture, in short, is the class-based taste culture of an educated elite; sociologically and normatively, it differs from "popular" taste cultures only insofar as the latter require less education to appreciate and therefore attract larger numbers of followers. Members of this larger, less privileged audience actively enjoy popular culture and derive forms of aesthetic pleasure and moral uplift from its thematic content. To expect and require them genuinely to appreciate high culture implies a theory of "cultural mobility" that is naive and unfair unless it is accompanied (or preceded) by more widespread social mobility. In sum, if more people received higher education and higher incomes, popular culture would also "improve," eventually approaching high culture in quality; but in the absence of such an upgrading of social conditions one ought not to demand such improvements in the cultural arena.

In developing each of these conclusions, Gans raises and confronts a number of important issues which have long gnawed at sociologists interested in these topics. Whether or not one agrees with his position, *PC & HC* thus poses questions which merit considerable discussion, especially to the extent that sociologists come to make policy recommendations about government subsidies to the arts or regulation of broadcast content. It is also an important book because it offers throughout ideas and observations which readers (who may not share the conclusions) will find insightful and stimulating. For example, in part 1, Gans offers the novel suggestion that

much of high culture is "creator" rather than "consumer-oriented." ("This picture is good because Picasso painted it," rather than "I like it.") He elaborates on this idea in an intriguing comparison of the respective contents, ideologies, and audiences for high versus popular culture. In addition, he contends that the dramatic plots typically utilized in popular culture neither distort reality nor serve escapist functions any more than do the dramatic productions of high culture. Rather, both tend to gloss over the lives of their respective audiences by dwelling on characters from *other* class and status groups, or by treating only symbolically (as in *Waiting for Godot*) issues about which the intended audience might be sensitive. And finally, much of the distaste felt by the high-culture audience for such television entertainment vehicles as "Roller Derby" or "SWAT" is fully reciprocated by lower-class audiences, whose aesthetic sensibilities are equally offended by the casual use of nudity, four-letter words, or homosexual themes in the dramatic performances patronized by adherents of high culture.

The book is divided into three sections. In part 1, where Gans takes on critics of mass culture, high and popular culture are both treated as unitary categories. This section is of special interest to anyone wishing to pursue the debate over mass culture which emerged during the late 1950s and early 1960s. It also is relevant to some of the more recent controversies over the content of television: whereas the earlier critique of mass culture viewed all popular fare as harmful to all members of society, the more recent version emphasizes the feared impact of particular content (e.g., sex or violence) on a category of viewers (e.g., children). While there are also some clear differences among the formulations offered by such critical parties as Action for Children's Television, the Legion of Decency, and intellectual critics of mass culture, Gans is persuaded that these are essentially similar. His assessment of all empirical evidence to date is that there is no basis on which to judge mass-media content harmful to its audience. Rather, he views the controversy politically—as the efforts of elite groups in society to force changes in the content and availability of those taste cultures presently enjoyed by their social inferiors, or, more simply, as status politics.

In part 2, Gans subdivides popular culture into a much richer and narrower set of taste cultures, each correlated with the demographic characteristics of a specific segment of the population. For example, the taste culture correlates of lower-middle-class socioeconomic status include an emphasis on substance over form and a preference for dramas about ordinary people, in which problems are resolved by the end, with virtue, order, and tradition upheld. Favorite magazines for this "subculture" are *Reader's Digest* and homemaking and do-it-yourself periodicals; preferences in popular fiction tend toward paperback novels about the evil doings of more privileged people, who are ultimately redeemed by returning to traditional values. In order to achieve high ratings, television entertainment must succeed in attracting a substantial proportion of the adherents of this taste culture as well as in simultaneously providing additional con-

tent for viewers from other subcultures with alternative patterns of aesthetic preference. Gans's valuable discussion of the within-group variance exhibited by "the" audience for (undifferentiated) popular culture would make excellent required reading for students and professional sociologists of art and mass communication.

In part 3, Gans seeks to draw out the policy implications of his argument. In brief, he suggests that each demographic segment be provided with its own cultural fare instead of being forced to choose from a limited number of programs which (a) are directed disproportionately toward lower-middle-class viewers between 18 and 49, and (b) artificially dilute the cultural fare of each single group by mixing elements of several taste cultures in order to attract the widest possible audience. It is here that Gans comes closest to agreeing with part of the critique offered by high-culture proponents (that popular culture is artificial and imposed on its audience), but his solution is radically different: rather than abolish popular culture and substitute high culture in its place, he would decentralize it and provide each population segment with better access to whatever it would prefer to receive.

The "solution" proposed involves several value judgments, about which Gans is characteristically explicit. I disagree with portions of his analysis and will focus on two in particular. In my view, Gans underestimates the extent to which the very concept of popular culture is dependent on the element of exposure to the widest possible audience. What makes "All in the Family," "Kojak," or Walter Cronkite part of our popular culture is the fact that their national audiences cut across demographic boundaries and present diverse groups of Americans with a set of common symbols, vocabularies, and shared experiences. I am not as convinced as Gans is that this factor is thoroughly undermined by the phenomenon of selective perception. A conceptual question is how much of this type of widely shared popular culture would remain if each demographic segment received programs exclusively in its own taste culture and if the extent to which audience segments overlapped decreased substantially. If the answer is "very little," then the casual use of the term "popular culture" is suspect and requires somewhat more careful elaboration. Because their readership was heterogeneous, *Life* and *Look* magazines were more representative of popular culture than are either *True Confessions* (directed at a low-culture audience) or the *New Yorker* (read by members of the high-culture audience). In this sense, it needs to be made clearer that (for better or for worse) to decentralize popular culture would also serve to abolish it.

A related sociological issue is: What are the social correlates of an integrated versus a decentralized popular culture? Gans suggests, in passing, that in the cultural arena, television networks function much like the major U.S. political parties—by aggregating the cultural preferences of several groups in order to produce the winning candidates. Changes in the institutional structure supporting such a scarcity of choices in either case should be expected to yield predictable as well as unanticipated consequences. Only by contending that the mass media have no significant

effects on people or society is Gans able to suggest that few unanticipated consequences would be likely to emerge from changes in their economic, institutional, and technological environments. In fact, it is in large part *because* mass-media content has so little clearly measurable impact on its audience that Gans proposes decentralizing it. In so doing, he takes an ethnomethodological perspective (Mannheim's as well) on the content of both news and entertainment, conceiving each as socially constructed to reflect the combined aesthetic and political biases of both producers and their target audiences and as the political and cultural expression of whichever groups are important enough to warrant programs of their own. The problem with current news and entertainment, in this view, is not that it is biased or distorting, but that it is biased only in the direction of those groups with sufficient purchasing power to attract commercial advertisers. Those audience segments excluded are thereby denied equal access to whatever biases they might prefer to see. Gans does not seek to "balance" or remove aesthetic and political distortion from current programming but to distribute it more democratically to each subculture individually, in better accordance with its own predispositions and thus in a purer form.

While Gans describes the research which would be needed to get such a program under way as a "planner's fantasy," I doubt that the long-range results—cultural, political, and social—of such an experiment can be predicted accurately at this time. The empirical and normative bases for these creative suggestions remain highly debatable, and I am also less pessimistic than Gans in assessing the mass media's importance as an integrative institution in society. I would therefore be more comfortable if the book held back from stating a major contention—that such a radical change in media content and availability would have no significant consequences beyond promoting cultural pluralism—with so great an air of finality.

These reservations, however, are more than outweighed by the merits of the work. Overall, *Popular Culture and High Culture* presents a reasoned, thought-provoking, and engaging analysis, alternately scholarly and polemical, and at all times worth reading. Gans's expansion of his original essay into book form should serve to introduce his many insights and ideas to new readers and to make his presentation far more accessible than it has been up to now.

Sport and Social Order: Contributions to the Sociology of Sport. Edited by Donald W. Ball and John W. Loy. Reading, Mass.: Addison-Wesley Publishing Co., 1975. Pp. xiii+576. \$14.95.

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This book, which consists of 12 contributions from a collection of young (ages 24–44) sociologists, physical educators, a kinesiologist, and a geographer, represents a welcome addition to the developing field of sport studies. Reluctance to support serious inquiries into the nature and signifi-

cance of sport has been a long-standing prejudice of the academic community; thus, this work quite self-consciously attempts to articulate the importance of sport in modern societies, to demonstrate the utility of sport studies for a better understanding of organizational principles, and to apply recent theoretical and methodological developments in the social sciences to the study of sporting phenomena. In this light, the 12 papers (11 of which are previously unpublished) largely succeed in legitimizing sport studies as a significant investment of the sociologist's energies and in destroying any attributions of insularity regarding work in this area.

More explicitly, the work is offered by its consulting editor, John Loy, as a "handbook" (p. v) or "sourcebook" (p. vi) for use in advanced undergraduate or graduate courses in sport studies. Eschewing the traditional responsibilities of the textbook for articulating the major issues and perspectives within a substantive domain, the book comprises instead a set of analytical reviews and micro-research monographs selected largely on the basis of the active interests of the individual authors. While this editorial decision has assured a generally high level of competence and enthusiasm on the part of the contributors, it has also created problems. One is the repetition of similar themes. Discussions of identity work for professional athletes, the functions of sport in articulating collective identity, and the ideological foundations of rationalized sport pop up in more than one chapter. Furthermore, fully five chapters and 240 pages are devoted to aspects of the occupational organization of sport. On occasion, the preparatory reviews of fundamental sociological perspectives are too long for the more sophisticated reader; for example, Michael Smith prefaces his generally useful treatment of collective violence in sport with a 20-page review of divergent theories of collective behavior. Such crimes are, of course, less heinous when it is considered that the book is intended to extend to sport the application of such perspectives by a student population. In short, the editors have opted more for a showcasing of individual talent (complete with personal profiles) than for a tightly organized handbook.

Customarily, the reviewer of a collection of articles selects some of them for public praise or condemnation, thereby substantiating some judgment of the overall worth of a book. However, as I have already tipped my hand to some extent, I shall preface the more specific considerations by stating that the book is indeed a fine contribution to the sociology of sport. Proper to its purposes, *Sport and Social Order* invites the student into a broad range of theoretical issues and problems for further research. All of the chapters, while varying dramatically in intellectual style, touch on significant problems; and the commentary on a great number of contemporary references will be most helpful to the student wishing to pursue work in sport studies. However, as I have indicated, the absence of a tight editorial rein has led to certain organizational deficiencies, the analytical reviews are often cumbersome, and important themes are entirely neglected.

Eldon Snyder and Elmer Spreitzer begin the book with a rather leisurely



overview of the sociological analysis of sport. However, the chapter is distinguished by many sound observations (regarding, e.g., the rationale behind a sport sociology, the questionable disjuncture between sport and leisure studies, and the academic antipathy to sport studies) which will be rewarding to the reader. Senior editor Donald Ball seemingly just drops in to say hello with a very brief note on method. His comments on the advantages of the sport setting for studying organizational principles and on the availability of various kinds of data for research projects are very pertinent indeed; however, the reader wishing to address the implications of various methodological schemes for the study of sport is referred to other sources. John Rooney follows with a quite interesting introduction to his own work on the geographical aspects of big-time sports in the United States. Richard Gruneau, in his chapter on social differentiation and inequality in sport, offers some truly stimulating insights into the relationship of sport to meritocracy and explains how meritocratic principles are severely restricted, even in so-called mass societies. However, his supporting data on participants in the 1971 Canada Winter Games do little more than establish the fact of differential participation in socioeconomic terms; more helpful would have been an examination of the recruitment and training procedures of various individual sporting organizations which highlighted actual processes of discrimination. Brian Petrie, Barry McPherson, and Michael Smith follow with workmanlike analytical reviews of the literature on the relationship of formally organized sport to politics, economics and consumerism, and collective violence, respectively.

The remainder of the book is devoted to the occupational organization of sport. Alan Ingham (with the assistance of Gurdeep Singh) offers a rather loose-jointed but generally engaging account of the rationalization of sport and its consequences for the transcendent project of the individual performer. George Sage provides a substantial analysis of college coaching, and Rudolf Haerle uses a basically Hughesian perspective in articulating the career contingencies of the professional baseball player. Especially appealing to me was Robert Faulkner's striking comparison of the impact of age on the development of careers in professional hockey and symphonic orchestras. The final chapter is composed of Allen Turowetz and M. Michael Rosenberg's comparison of identity work for professional wrestlers and physicians, a brief essay which tends to underestimate somewhat the problematic nature of role performance for the latter group.

Thus, the book samples a wide range of topics. Loy tells us that "while coverage is not complete, it strains toward the comprehensive" (p. vi). But does it? First, it will be helpful to the potential reader to understand that this sociology of sport is devoted, in every instance, to examinations of sport in its most highly organized forms. There is little interest here in informal sporting activities, and those who seek continuities with the work of students of play, such as Huizinga or Caillois, or even Mead, will be disappointed. Just as any consideration of sporting activities as a social articulation of the expressive life is generally neglected, so the various chapters only hint at the reasons why sport has grown to such

exaggerated proportions in Western societies. The book provides few insights into the question of whether the secular organization of play activities (as opposed, e.g., to religious ritual) speaks centrally to the aspirations and status dilemmas of individuals in modern societies. Likewise, there is little emphasis on the development of sporting pastimes, and material from societies other than the United States or Canada is rarely provided. It is, of course, somewhat unfair to criticize a book for not doing what it does not claim to do. However, as many of the authors are at present quite influential in shaping the future course of sport studies and as the book itself is intended to assist the development of research interests in the young scholar, an insistence on the broadening of the field of sport studies itself must accompany my recommendation of this book.

Image and Influence: Studies in the Sociology of Film. By Andrew Tudor. New York: St. Martin's Press, 1975. Pp. 260. \$17.95.

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Andrew Tudor has written a book on film fiction subtitled *Studies in the Sociology of Film*. The subtitle is misleading, because the book neither presents a sociology of the film nor contains any original studies. Instead it consists of essays on the communicator, the audience, the content of films, film movements, and several other topics; none of the essays can be considered studies. Also, Tudor himself states (p. 18) that the book is not a sociology of the film and that it does not provide a general theory of film. He does claim that he provides a model for studying the communication process as it relates to the fiction film. This review is an attempt to determine whether Tudor succeeds in this purpose.

The book provides cases or examples for Tudor's abstract analysis and model of communication. In chapter 2, the reader is told that a sociology of communications must encompass the communicator, the content, the audience, and the situation. Not only is it necessary to identify those who interact to bring forth the content and those who use the content, but also the patterns of relations which characterize the communication process must be studied. The author goes on to state that mass culture (the terms "popular culture" and "mass culture" are used throughout the book as synonyms for film fiction) should be taken seriously as content. To do so, he then examines Hollywood as it was in its heyday, the audience of the films, and their content (both the language used and the patterns of meaning). There is also a chapter on film movements, with much space devoted to German Expressionism.

The model of communication given has much to commend it as a tool for research and analysis, as many other sociologists and communication researchers have also suggested. However, Tudor's use of it is weak and unsystematic. For instance, the chapter on Hollywood, which I do not

recommend, is simplistic. Statements are made which are not substantiated in that chapter or in any which follow. For example, on page 64 Hollywood is described as a mixture, incapable of adequate summary, and as an elaborate structure without any clear legitimation. Such statements are so general that they obscure the way in which the structure of the movie industry influences the process of putting together a film and the content generated. It is one thing to say one does not know the answers to these and other important questions and quite another to use sweeping statements to cover up lack of knowledge. Although the film industry was organized with an elaborate structure in the thirties and forties, it was not so complex and elaborate as to be incomprehensible.

Also, to research the Hollywood of that period one does not merely summarize the writings of Powdermaker and Rosten. Moreover, Tudor's data even at that level are faulty because he accepts Powdermaker's analysis without awareness that she later recanted some of what she wrote in 1950. In her autobiography, *Stranger and Friend* (New York: Norton, 1966), Powdermaker admits to some success in describing the social structure within which movies were made (agreed) but also apologizes for her lack of humility and compassion. Her lack of understanding of and sensitivity to the subtle relationships that existed in the movie industry made her analysis less informative than it might have been. More important, her failure (and therefore Tudor's as well) to inquire in depth into the financial control of the industry and the effect of such control on the content of films leaves the section on Hollywood inadequate. Also, nothing in the chapter shows that after Powdermaker did her study the industry changed, television eroding the movie audiences and the United States government altering the economy of production by separating distribution of films in an antitrust action. What is learned from the chapter on Hollywood is that movies are produced by many people who relate to one another in a variety of ways and that social relations "invented" (Tudor's word) by Hollywood work. The major point of Powdermaker's work could have been examined more carefully: Structure does influence the content of films. However, social structure changes as technology, audiences, and external controls exerted by government, unions, and business enterprises change. The old system of independent authority of the thirties and forties, built on paternalistic ownership, has disappeared, and in its place is a structure which is more bureaucratized and impersonal. It also seems to work. Tudor would have made a stronger presentation for his model if he had compared the old Hollywood with the new and showed that the content of films differs under the two structures.

Although most of this review is critical of the chapter on Hollywood, the rest of the book fares no better when scrutinized carefully. In brief, Tudor is a sociologist who has seen many films, has read a good deal about the movies, and presents no original research. Apparently, primary data in the book were obtained from moviegoing. Therefore, with rigorous and systematic content analysis unavailable (and eschewed) one must accept his belief that the popular fiction film reflects exploitation, self-interest,

and hypocrisy. The heuristic values Tudor proposes throughout the book are commendable. In order to understand both the effects of film on the audience and the larger questions of what kinds of film are possible under what conditions, one should explore in detail the relationship among the various elements of the communication process. However, Tudor does not do so. In the last paragraph, he reviews his own book: he apologizes for its "crude approximations, analogies of limited use and all sorts of cavalier attitudes to empirical detail."

Blue Cross: What Went Wrong? By Sylvia A. Law. New Haven, Conn.: Yale University Press, 1974. Pp. x+246. \$8.95.

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Sylvia Law, an attorney and law professor, has written the first comprehensive study of Blue Cross and almost the first of any component of the health insurance industry. The orientation is largely toward the legal aspects of the origins and operations of Blue Cross, and the 858 footnotes cover myriad legislative hearings, statutes, court decisions, and the complex relations between public and private aspects of the provision of health care. The book is an important contribution both to medical sociology and to the sociology of law.

Blue Cross illustrates several major aspects of the American health care system: the appropriation of public money and public power by private agencies, the failure of federal regulation and control, and the operations of a major interest group and its agents: the hospitals. The analysis in this book is superb, but the recommendations are inconsistent with its own analysis, as I shall try to show.

First, the major features of Blue Cross, as described by the author. Blue Cross is actually an agent of the hospitals, seeking stable and high levels of funding for them. Yet Blue Cross was given the responsibility of supervising Medicare and Medicaid payments by the federal government, entailing a direct conflict of interest. The result has been no real accounting of costs and no controls, thus immediate and huge escalation of hospital costs. None of the various proposed plans for national health insurance, even the most "liberal," can confront this problem directly.

Although Blue Cross is nominally a nonprofit agency, it subcontracts to profitmaking corporations in such lucrative areas as data processing. One corporation made a 48% profit in 1969 from Medicaid data processing. Also, Blue Cross itself has actually made a profit (\$5 million in 1972) labeled a "public service charge," based neither on administrative efficiency nor on financial experience. There was no requirement that this profit be used in any way related to "public service."

The consequences of simply turning over public power and funding to a private agency with no effective controls have been a series of practices by Blue Cross which the volume details. Depreciation allowances, which

do not have to be reserved for actual replacement of equipment, have become sources for the capital requirements of hospitals. In 1970 depreciation funds, which are intended as insurance for the poor and the aged, were four times those available for hospital construction under the Hill Burton act.

Another consequence is an increasing denial of claims. For nursing homes this rose from 1.5% of claims in 1968 to 12% in 1971. This practice imposes a heavy penalty on the affected individuals, because doctors, hospitals, and patients, in deciding whether or not to prescribe or accept treatment, assume the claim will be covered. An attempt in one state to place the burden for denial of a claim upon the hospital, rather than upon the patient, while seemingly a reform, has led to refusal by hospitals to accept poor and elderly patients if they believe Blue Cross will later deny the claim.

Yet another consequence is open-ended administrative costs, sometimes used for antiunion campaigns by hospitals and for lobbying Congress. Hospital charges are also manipulated to load maximum costs onto federal programs. Blue Cross advises hospitals to reduce charges not covered by Medicaid and to increase those which are.

As a result, the "reasonable cost" provision in the legislation has no criteria and no method of regulation, and so the prices of hospital supplies and drugs have skyrocketed, increasing the profits in those industries.

These details have been given to show the importance of a case study of Blue Cross to illustrate one component of the health care system, and the book is comprehensive and well done from this point of view. Unfortunately, it ends on a utopian note in its recommendations for reorganization: create a new public administrative agency to replace Blue Cross; elect boards of fiscal intermediaries to represent consumers; fuse Blue Cross and Blue Shield; require full public disclosure of information.

These recommendations—unexceptionable in principle—reflect a faith that a properly elected and administered structure could rectify the defects so amply documented. In the absence of any movement for health reform which is linked to a broader social and political movement in the United States, the author must rely upon the faint possibility of congressional action. The last chapter exhibits a fundamental ambivalence, on the one hand welcoming any movement in a reformist direction, on the other critical of even the "best" bills in Congress, such as the Kennedy-Griffiths bill.

The basic problem with the analysis, from the point of view of the sociology of politics and law, is that the reluctance of Congress to establish competing provision of services and thoroughgoing administration of regulation of private providers and its reliance instead on subsidies of private organizations are seen not as a structural feature of the American political economy but as a temporary and almost accidental or situational phenomenon, potentially changeable by good research and dissemination of the facts. Analyses such as this may contribute to the emergence of public pressure upon Congress, but such pressure is no guarantee that Congress

will act in a significant way or that even its action will have any impact upon deeply institutionalized private control over key social services such as health. The structural barriers to change must be understood, if movements for change are to be able to adopt appropriate strategies and tactics.

Hospital Bureaucracy: A Comparative Study of Organizations. By Wolf V. Heydebrand. New York: Dunellen Publishing Co., 1973. Pp. xvii+362. \$15.00 (cloth); \$6.50 (paper).

Stephen M. Shortell

University of Washington

In an ambitious effort, Heydebrand develops and tests a theory of bureaucratic organization, using precollected data (obtained from the American Hospital Association) describing the characteristics of 3,544 hospitals in the United States. The book will be of some interest to sociologists and other social scientists interested in the study of organizations and social structure at large. It is also likely to be of interest to medical sociologists. Unfortunately, in many places it reads like a doctoral dissertation; it is in fact based on the author's dissertation, completed in 1965. The result is the presence of overdeveloped background material and extraneous chapters (e.g., chaps. 6 and 8). Social scientists interested in the policy implications of research on health care delivery organizations will also recognize the limitations of Heydebrand's recommendations (outlined in chap. 11), which do not really follow from his analysis and which reveal considerable naiveté regarding the American health care system. But despite these limitations there are definite gems in this book which deserve to be highlighted.

Heydebrand builds his theoretical framework around the concepts of (1) organizational autonomy (in relationship to the environment), (2) complexity of task structure, (3) complexity of the external environment, (4) organization size, (5) technological complexity, (6) internal division of labor (both functional and departmental specialization), (7) professionalization, (8) bureaucratization (both hierarchical and in terms of administrative and clerical specialization), and (9) effectiveness (i.e., extent of goal attainment). Working from a contingency perspective, he suggests that organization size and the complexity of the external environment determine task structure, which in turn determines the degree of technological complexity, functional specialization, departmental specialization, and professionalization. The latter two concepts, departmental specialization and professionalization, are seen as modifying the effects of functional specialization and technology on the bureaucratic forms of coordination. All of the above act to determine an organization's effectiveness.

While the foregoing indicates the overall framework, the author's main concern is to examine the relationships involving task complexity, division of labor (i.e., functional and departmental specialization) and coordination. Building on the theories of Durkheim and Weber, Heydebrand sug-

gests that the greater the degree of task complexity the greater the likelihood that different forms of coordination will be simultaneously present, although the structural forms are likely to be more prevalent than the bureaucratic-hierarchical ones. Thus, he predicts that the more complex general and teaching hospitals will have higher levels of functional specialization, departmental specialization, professionalization, and a larger administrative-clerical staff but a lower level of bureaucratic-hierarchical coordination.

He also suggests that where there is a low degree of functional specialization, there will also be low levels of departmental specialization, professionalization, and administrative-clerical specialization but a high degree of bureaucratic-hierarchical coordination. The latter form of coordination is needed where the labor force is homogeneous or is composed of relatively low skill levels. In contrast, where there is a high degree of functional specialization, the reverse relationships are likely to be found (e.g., high degree of departmental specialization, professionalization, etc.).

In summary, as organizations become more complex both in terms of their tasks and their internal functional division of labor they require not only more departmental specialization and administrative-clerical coordination but a greater degree of coordination by professionals as well. Professionalization and departmental specialization are seen as being aspects of the division of labor and processes of coordination at the same time.

The operational measures for these and related variables are summarized on pages 64 and 65. For example, task complexity is based on hospital service classifications (psychiatric versus general and nonteaching versus teaching); functional specialization, on the percentage of job titles filled by the hospital; departmental specialization, on the distribution of employees among major hospital departments and units; professionalization, by the proportion of graduate professional nurses; the bureaucratic hierarchy of authority, on the percentage of nurses in administrative-supervisory positions; and administrative specialization, by the relative size of the administrative-clerical staff.

Since organizational autonomy and task complexity were respectively measured by the type of hospital ownership (federal, local government, voluntary nonprofit) and type of services provided (psychiatric versus general and teaching versus nonteaching), all hospitals were grouped according to these characteristics and the data analyzed primarily by analysis of variance and covariance. The principal findings are described in chapters 7, 9, and 10.

Chapter 7 examines the relationships among division of labor, size, and task complexity. Departmental specialization is found to be positively related to functional specialization but inversely related to size. The primary example of this is the large state mental hospital which "becomes structurally simpler as it grows larger" (p. 185). Using findings pertaining to teaching hospitals, Heydebrand goes on to note that "... functional specialization is an aspect of division of labor which is derived from both

the nature and size of the task and, in turn, helps to determine the size of the organizational labor force" (p. 185). In contrast, departmental specialization is largely under the control of administration and may be used in a discretionary fashion.

In chapter 9, the evidence to support the coexistence of both bureaucratic and professional forms of coordination is presented. However, as predicted, Heydebrand does find modest support for the proposition that professional forms would tend to predominate under complex task structures. The underlying rationale is that because professionals, in this case nurses, contribute to coordination at the operational level, administrative overhead may be kept relatively lower.

In chapter 10, evidence bearing on the more complex relationships among task structure, division of labor, and forms of coordination is discussed. The greater the complexity of hospital task structure, the greater the size of the administrative clerical staff *and* the greater the percentage of professionals, but the lower the percentage of nurses in administrative-supervisory positions (i.e., bureaucratic-hierarchical coordination). Heydebrand also compares Stinchcombe's hypothesis that a professionalized labor force represents an alternative to bureaucratic administration with the Anderson-Warkov hypothesis that increasing departmental specialization has a positive effect on the relative size of the administrative-clerical staff. Heydebrand believes he finds support for both hypotheses when there are a high degree of task routinization and a low level of internal structural complexity. But in hospitals with more complex internal structures the administrative-clerical staff ratio is inversely related to departmental specialization and positively related to professionalization (pp. 272-80). His analysis of the relationship is, however, extremely confusing because he appears to use departmental specialization as his measure of internal structural complexity, which he controls for in examining the interaction of departmental specialization itself with the administrative-clerical staff ratio. Finally, the author develops an empirical classification of hospitals in regard to task structure, size, internal complexity, and forms of coordination. The typology suggests that hospitals with simpler task structures use fewer mechanisms of coordination, with the primary method being hierarchical. Hospitals with more complex task structures use a larger number of coordination mechanisms, with emphasis on structural and administrative methods. Professional coordination appears to play its most significant role at intermediate levels of task complexity.

By further specifying the conditions under which different forms of coordination are used, in particular in relation to such variables as organizational autonomy, size, task complexity, and division of labor, Heydebrand has advanced our understanding of organizations and provided leads for future investigations. As the foreword by Paul Lazarsfeld emphasizes, he is also to be commended for his relatively creative use of precollected data. But the content could have been further enriched had the author discussed his findings in relation to other studies carried out before and after his dissertation research and published before the

publication of this book. Examples here include the work of Blau ("A Formal Theory of Differentiation in Organizations," *American Sociological Review*, vol. 35 [April 1970]) and others on the effects of size on internal differentiation, the book by Lawrence and Lorsch (*Organization and Environment* [Boston: Harvard Graduate School of Business Administration, 1967]) on the effects of environmental complexity on differentiation and integration, and the early study of Georgopoulos and Mann (*The Community General Hospital* [New York: Macmillan, 1962]), concerned with a similar issue, the different forms of coordination used by hospitals and the relationship of these forms to other organization variables. Because of this neglect and the limitations noted earlier, *Hospital Bureaucracy* is much less than what it might have been. For those interested, the book should be selectively read with principal attention given to chapters 7, 9, and 10, which have been briefly highlighted in this review.

An Ethnography of a Chiropractic Clinic: Definitions of a Deviant Situation. By James B. Cowie and Julian Roebuck. New York: Free Press, 1975. Pp. xiv+162. \$10.00.

Walter I. Wardwell

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Any book must be evaluated in the light of the authors' objectives. From this point of view *An Ethnography of a Chiropractic Clinic* is successful. However, as a work of general sociology this small book is not a major contribution to the literature, theoretical or empirical.

The authors' goals are modest: to provide "an analysis of a chiropractic clinic defined in terms of the interactional patterns characteristic of that behavioral setting" (p. 9); "to coherently reconstruct situationally limited social phenomena while intentionally keeping to the basic premises of symbolic interactionism" (p. vii); and "to contribute to the rapidly growing interest in the sociology of everyday life" (p. viii). At the same time, as the subtitle indicates, the authors believe that the chiropractor "continues to endure a deviant status in American society," and they state that "the methodology employed hopefully will add to our understanding of the chiropractor and other marginally deviant social positions" (p. 1).

The authors maintain that they intend no theoretical innovations: "We have set forth neither any major new point of view nor a minor theoretical twist in our interactionist approach" (p. vii). Despite this disclaimer, they want to improve upon previous symbolic interactionist research by avoiding what they refer to as "the bias often shown in the so-called objective approach which is normally employed in sociological studies of deviant behavior" (p. 130), and they seek to avoid bias by presenting the social situation of one chiropractor's clinic from the perspective of the chiropractor himself. Perceptions, meanings, "rules of the game" (p. 12, n. 6), even the chiropractor's interaction with assistants and patients—all of these are viewed through the practitioner's eyes. Clearly it is he whom they treat as

deviant, rather than his patients, although the authors acknowledge that "the question of why these persons deviate from the norm of seeing the establishment medical doctor when physical problems arise remains to be answered" (p. 135). They see the chiropractor not as a passive recipient of the label "deviant," but as coping actively with his problem through selecting those patients who accept his definition of what chiropractic is and can do for them while rejecting those patients who do not.

To study the clinic the senior author (Cowie) became a participant observer—with a vengeance! Besides conducting lengthy interviews with the chiropractor, his wife, several other chiropractors, and patients, he persuaded the chiropractor to employ him as his clerk-receptionist for three and a half months. Apparently perceiving Cowie as a potential future practitioner, the chiropractor took him into his confidence to a greater degree than he did the other members of his "dramaturgical team." Hence Cowie became privy to the "backstage" perceptions, conversations, and staging of the "front" that the clinic presented to patients.

The most detailed description and analysis in the book is of the "impression management" of the chiropractor and his assistants. It was designed: (1) to portray the chiropractor to patients as a competent professional person despite the criticisms and opprobrium heaped on chiropractors by medical authorities, (2) to reassure the patient that "adjustments" (manipulation) of his vertebrae will not be painful or harm him despite stories he might have heard or any fears he might have in that regard, and (3) (a point not sufficiently treated in the book, in my opinion) to persuade the patient that chiropractic treatments will cure him. My main substantive criticism of this ethnographic description concerns the authors' implication that the "impression management" was designed mainly to persuade patients to accept chiropractic and to return for additional treatments, with no attempt to evaluate its impact on patients' health status or symptoms. The absence of such data and such a concern causes me to question whether it is the authors' implicit conclusion that no benefits at all were obtained from the chiropractic treatments observed, since medically oriented writers have often suggested that patients receive psychological benefits from chiropractic treatment even if no genuine physiological relief. But the question of benefits received is not even raised. For a sociologist to spend three and a half months as a chiropractor's assistant, participating, observing, interviewing, and not gather data on patients' symptoms, diagnoses, and results obtained (subjectively or objectively assessed) constitutes in my opinion a tragically lost opportunity. Such data could have been made even more valuable if they had been related to the chiropractor's typology of patients or to such simple tabulations as the number of treatments received. Furthermore, despite the fact that they mention as an area needing study "the socioeconomic characteristics of persons who seek out the services of marginally deviant health care practitioners" (p. 135), the authors provide no data, not even impressionistic, on this topic. Yet the patients and patients' records were patently available to them.

I found the authors' treatment of the question of representativeness

unconvincing. Although aware of the range of variation between chiropractic "straights" and "mixers," they seem not to recognize that the philosophy of the chiropractor they studied (the "dynamic essential" philosophy of Dr. Sid Williams) is probably the most extreme "straight" position in chiropractic today—even more so than that of the Palmer College of Chiropractic, the "fountainhead" of chiropractic education. By citing Stephenson's "Thirty-three Principles of Chiropractic Philosophy" (originally published in 1927) as documentation they reveal their lack of awareness of contemporary trends in chiropractic education and of how unrepresentative is the chiropractor they studied.

However, the authors capably describe and analyze the process by which a deviant practitioner defines reality in a situation where meanings constructed from earlier interaction with teachers and fellow practitioners are continually being revised as a result of current interactions with patients. The practitioner demonstrably plays an active coping role instead of passively accepting a negatively labeled one. I believe that this is what the authors mean when they refer to the practitioner's "methodographic" activity (p. 25), in which he experiments with and refines his techniques of impression management. *An Ethnography of a Chiropractic Clinic* successfully illustrates a variant of symbolic interaction analysis using a case study, but it does not present a broad coverage of chiropractic clinics or of chiropractic practice in general.

Black Managers in White Corporations. By John P. Fernandez. New York: John Wiley & Sons, 1975. Pp. xi+308. \$16.95.

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Ever since the civil rights movement began, large numbers of whites have felt that "blacks are pushing too hard." Initially, this sentiment seemed largely confined to southerners and, in lesser numbers, to the lower-middle and working-class whites of the North. But as momentum gathered, and as the arenas of conflict moved from lunch counters, public schools, and trade unions to universities, government bureaucracies, and executive suites, another voice was heard: favoritism, reverse discrimination, a violation of the principle of equal opportunity. These cries against the quotas and guidelines of Affirmative Action have emanated from highly respectable sources not usually associated with racism or prejudice. Indeed, the usual hallmarks of prejudice are utterly absent from the rhetoric of the new defenders of order. But however cloaked in the language of procedural fairness and universalism, the defenders of equal opportunity are clearly saying: "blacks are pushing too hard." It is wise not to forget that equal opportunity and equality are not simply variations on a theme; in profound ways they are antagonistic conceptions.

Consider matters from a different angle. The data on occupational attainments of blacks do not unambiguously indicate rapid entrance into

heretofore restricted occupations. There were, for example, proportionately fewer blacks in medicine and university teaching in 1970 than in 1960; in law and salaried managerial positions, only slight increases occurred. (Sar A. Levitan et al., *Still a Dream* [Cambridge, Mass.: Harvard University Press, 1975], pp. 48–49.) In short, the recent alarm over lowered standards and the unfair advantages of blacks are cruel distortions in the face of continued inequalities.

John P. Fernandez's *Black Managers in White Corporations* allows us to see more clearly the pattern of continuing discrimination, subtle and not-so-subtle, in one realm of the upper-middle class, the world of the corporate executive. His study embraces six West Coast firms: two banks, two utilities, and two manufacturing corporations. All fall under federal guidelines for Affirmative Action and all have increased the number of blacks in their managerial cadre, but their compliance with federal guidelines varies considerably. One public utility has a comprehensive affirmative action plan and an energetic executive overseeing its implementation; the other firms suffer from modest plans, executive lethargy, and, not infrequently, staunch refusal to see any problem, any need to make special efforts to recruit or promote minorities. The letter of the law is minimally adhered to; but its spirit is rarely in evidence.

The consequences are predictable. Black managers are concentrated at lower levels. In none of the firms studied was a black in a senior position. Moreover, few blacks had serious command over organizational resources—most had no budgets to oversee and few subordinates to direct. Many were in positions that amounted to organizational cul-de-sacs. This hunch is supported by comparisons of white and black career paths. Fernandez shows that many fewer blacks in the firms studied came to their present positions from within the firm—they were hired from government or other businesses. By comparison, many more white executives' careers were within the firm currently employing them. This is vivid demonstration of the oft heard complaint that blacks are passed over for promotions. Equally predictably, Fernandez found that black managers earned less than their white counterparts, contrary to the prevalent assumption that the recent expansion of demand for blacks has driven up their salaries. Their bargaining position may well have been improved, but the gaps were so huge that equity is still a long way off.

Fernandez's analysis also takes him into an exploration of the attitudes of white and black managers toward each other and their perceptions of how well businesses in general and their own firms in particular are doing in reducing racial inequality. There are few surprises in this aspect of the research. White managers see more progress, less racism, in managerial policies than do blacks. Most white managers see black people's characteristics—cultural differences, lower levels of education, lack of "entrepreneurship," etc.—as the major impediment to further gains in equality. Most blacks see white prejudice and/or indifference as the major hurdle. All attitudes, however, are muted—there are few militant black managers and there are even fewer out-and-out racist white managers, even though

a considerable number of white respondents do report hearing antiblack sentiments expressed in their offices. In short, what is documented by this research is that the color line is more flexible now but that there still is a color line. It is also clear, especially to the black managers themselves, that without steady pressure from the federal government on the one hand, and blacks themselves on the other, there would have been little or no change. Moreover, it seems clear, if the firms studied by Fernandez are at all typical, that the momentum toward racial equality is far from having reached the point of being self-sustaining, a point where "benign neglect" might make sense. The plain fact, as Fernandez's data suggest, is that qualified members of minorities continue to be denied equal access to managerial jobs, protestations of favoritism and reverse discrimination notwithstanding.

In the face of the data Fernandez presents, especially given the context of current erosion of liberal support for programs that at least promise serious racial integration, it is curious and regrettable that he did not choose to treat in depth the particular forms that racial discrimination takes in managerial ranks. Apart from a cursory discussion of institutional racism and an equally brief treatment of individual racism, the reader is left to wonder whether there is anything unique or particular in the manner in which large-scale, bureaucratic enterprise functions to sustain racial hierarchy. The failure to explore the ways in which, and the motives for which, equal opportunity is deferred and attenuated makes the recommendations that frame the concluding chapter unpersuasive. To call for job enrichment, closer relationships between super- and subordinates, and clear, impartial performance ratings is fine, I suppose. But too much finally rests on the assumption that corporations (or universities or any other organization) are willing to promote racial equality beyond the minimal guidelines now in force. His own data suggest that more than relying on good faith, itself in limited supply, is needed.

The Unequal Elites. By Robert P. Althauser and Sydney S. Spivack, in collaboration with Beverly M. Amsel. New York: John Wiley & Sons, 1975. Pp. xvii+204. \$12.50.

Robert L. Crain

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The Unequal Elites is an analysis of six matched samples of college graduates: matched samples of black and white male alumni from two northeastern state universities with each pair of samples in turn matched to one of two samples of alumni from a black university. Data on religious and social participation, political attitudes, job satisfaction, home ownership, social visiting, marital status, and family and individual income are all presented, but the main analysis is of income.

Matching was done by locating black alumni and randomly selecting a white match for each, with the same year of graduation, same grade

point average, and same father's occupational status. Matched sampling provides a valuable data set to complement the various studies of race and income based upon national samples.

Black college graduate incomes are considerably below those of whites. The authors' main conclusion is that this difference does not result from an unequal distribution of income-producing resources or from black preferences for occupations with lower incomes, but reflects chiefly the fact that at each stage at which a white accumulates an additional unit of income-producing resources, the marginal return for that additional unit is greater than it is for a black in the same position. The basis for this conclusion is a complex and intriguing effort to break down the white-black income gap into three components: (a) that due to differences in resources, (b) that due to differential return on investment in resources, (c) all other (including racial) differences. The breakdowns for each comparison of white and black matched samples are difficult to interpret and sometimes give inexplicable results. One difficulty in interpretation is caused by the definition of resources: the regression analysis combines resources which are clearly background characteristics (such as father's occupation, or college grade point average) with ones which are simple characteristics of the respondent's present occupation (such as occupational prestige). Perhaps more important, in the analysis of cross-sectional data, respondents ranging in age from the twenties to the fifties are treated as a synthetic cohort, for the members of which age becomes an additional resource. The results are heavily influenced by the fact that the oldest group in the sample of whites from one of the state universities has a mean income in 1958 dollars of \$25,000. There are probably only about 20 respondents in this cell, although the exact number is not given. The effect of this is to lead the regression analysis to indicate that age is an enormously valuable resource for whites. This in turn produces several results: resources have much higher marginal utility for whites than for blacks; differences between blacks and whites in average level of resources explain only a small fraction of their differences in income; and, exclusive of these factors, the net income difference resulting from all other differences between the two groups is a sizable one in favor of blacks. We are led to conclude that blacks begin their careers with a sizable advantage over whites, which they lose rapidly because of their considerable disadvantage in marginal return for investment in additional resources. This breakdown of white-black income differences is performed four times, since there are four comparisons that can be made among matched samples; although the raw income differences are similar in the four comparisons, the breakdowns produce wildly different results; the raw differences in mean income range from \$1,100 to \$2,400, but the component resulting from differential return on resource accumulation ranges from -1,700 (favoring blacks) to +6,200 (favoring whites).

Unfortunately, the book does not include very much raw data. A distribution of samples by age and a zero-order correlation matrix would have been a useful appendix, for example.

The sample design was made by Sidney Spivack before his death, and according to the preface the original intent of the research was to "study the effects of education received at a predominantly black school [by comparing] black and white classmates from integrated schools with similar background in grades, and then to extend that comparison to similar blacks from predominantly black schools." Given my personal policy interests, I regret that this original research question was discarded in favor of the analysis of racial differences, although of course that analysis may be more interesting to others. The raw data show that the graduates of the predominantly black school have noticeably lower median incomes than the blacks who attended state universities, even though a considerably larger number of M.D.s graduated from the black university. The regression analysis indicates that over the normal range of occupational resources the black state university graduates have lower incomes. The text refers to the predominantly black institution as the most prestigious black school in the United States; if so, one would expect self-selection biases perhaps to favor it in comparison with the two state universities. It is unfortunate that the three schools are not named. Let us hope that secondary analysis of the data will illuminate the differences between the experience of black alumni of state universities and that of alumni of the black university in question.

Althauser's text includes extensive discussion of methodological issues and will be valuable reading to researchers concerned with the analysis of occupational prestige and income.

School Desegregation: Outcomes for Children. By Nancy H. St. John. New York: John Wiley & Sons, 1975. Pp. xv+236. \$13.95.

Reynolds Farley

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This is one of the more valuable books dealing with race and education. In recent years many claims have been made about the advantages or liabilities of racial desegregation of the nation's public schools. Often these assertions are based upon findings from a particular area at a specific point in time. Because there was no compendium of findings, it has been difficult to compare and evaluate discrepant claims.

Nancy St. John systematically reviews 120 studies of the effects of school desegregation. At the outset she delineates the characteristics of a good study. Then she organizes her presentation around three topics: first, studies of the effect of desegregation upon students' academic achievement; second, studies describing how students' self-concepts are influenced by desegregation; and, finally, investigations of the effect of desegregation upon racial prejudice. The characteristics of the 120 studies, their sample sizes, their designs, and their chief findings are efficiently summarized in nine large charts.

The reader learns that very few investigators have used the requisite

control groups or the appropriate tests which would produce a definite study and that consequently we can draw few general conclusions about the effects of desegregation. At the end of the first section, St. John observes that extant studies have not determined whether there is a causal relationship between the racial composition of a school and the academic achievement of its pupils. When summarizing studies of racial prejudice, she contends that sometimes desegregation promoted interracial friendships and reduced prejudice but other times it encouraged conflict, cleavage, and stereotyping. Studies of self-concept, St. John believes, indicate that black children in segregated schools have higher aspirations and self-conceptions than those in desegregated ones. The reductions in aspirations, St. John thinks, may be functional for some black children.

The concluding chapters present the author's views of the effects of school desegregation and her evaluation of various strategies to achieve integration. These are interesting and clearly written chapters, yet it is difficult to put great faith in her speculations since, in previous chapters, she stressed the weaknesses of the research findings.

In my view, the value of this book would have been increased if the author had considered two additional topics. First, it would be helpful to have a chapter devoted to the effects of desegregation upon years of school completed. At the end of World War II, there was a substantial racial difference in secondary school completion, but today the difference is quite small. In some communities, desegregation may have played a role in reducing this racial difference. It is particularly important to investigate this point since economic achievement appears to be more strongly linked to years of schooling than to accomplishments in school.

Second, it would assist the reader if St. John stressed which findings should be dismissed and which should be accepted. To be sure, she describes the flaws of many investigations, and thus it is easy to conclude that almost all these studies are useless. Since she invested great effort in locating and reviewing numerous studies, she could aid readers and future investigators by evaluating more critically the design, tests, and statistical models of the previous studies.

Synthesizing the research findings of others as a major endeavor has not been encouraged within our discipline. St. John, in her scholarly book, demonstrates the contribution that can be made by such a synthesis; hence her work will be frequently used in the continuing debate about the effects of school desegregation.

Process and Outcome of Negotiations. By Otomar J. Bartos. New York: Columbia University Press, 1974. Pp. x+451. \$15.00.

S. S. Komorita

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Bargaining and negotiation constitute a pervasive phenomenon that occurs at all levels of social interaction. Paradoxically, it is only in recent years

that a cumulative body of knowledge about them has begun to develop. During the past twenty years, the number of empirical studies of bargaining and negotiation has grown exponentially; since 1960 alone, more than 1,000 articles and books have been published on the subject (J. Z. Rubin and B. R. Brown, *The Social Psychology of Bargaining and Negotiation* [New York: Academic Press, 1975], p. viii).

Unfortunately, theoretical progress in this research area has not kept pace with the enormous increase in the empirical data base. Hence, at the present stage of knowledge, what is needed are theoretical models that integrate and organize the empirical findings. The present work is a step in that direction; it attempts to fill the gap by testing some theoretical models of bargaining and negotiation. Some of these models are stochastic ones and the others are based on mathematical game theory. Consequently, some mathematical sophistication is required to appreciate the author's contribution: some knowledge of probability theory is essential, and some of the basic ideas of game theory would be helpful.

A distinction is frequently made between a normative (prescriptive) approach and a descriptive (predictive) approach to the study of negotiation and decision making (cf. R. D. Luce and H. Raiffa, *Games and Decisions* [New York: Wiley, 1957], p. 63). Game theorists are almost exclusively concerned with a normative approach, attempting to determine what a "rational" man should do in a given situation. In contrast, most social scientists employ a descriptive approach, attempting to determine (predict) what individuals are likely to do, rather than what they ought to do. The author assumes that the two approaches are not antithetical but can be synthesized, and attempts to test the empirical validity of several normative models.

To evaluate the predictive validities of such models, the results of a series of laboratory experiments were used. They were conducted by the author using volunteer subjects drawn from sociology and other behavioral science classes. All of the experiments were based on a paradigm in which subjects were presented proposals for the distribution of payoffs. Each subject knew his own payoff, but not those of the others, and the task (in most experiments) was to reach unanimous agreement on one of the proposals. If unanimous agreement could not be reached within a given time limit, everyone received zero payoff. In game theory, this paradigm is called an *N*-person cooperative game without sidepayments.

The author's data base is quite large, with a total of 682 groups. However, there were many experimental conditions, and no attempt was made to evaluate their effects in a systematic way. Hence, to test the empirical validity of the various models, the author assumed that the effects would "wash out" when pooled across the experimental conditions. Also, it would have been desirable to cross-validate (or double cross-validate) the results, especially for the models which required parameter estimation to derive predictions.

One of the unique features of this evaluation of a variety of possible models is that theoretical models of the process of negotiations are dis-

tinguished from ones that predict or prescribe their outcome. Models of the process attempt to predict the demands made by the negotiators, and the changes in demands during the course of negotiations. By restricting himself to "simple" models of the process, the author manages to keep the mathematical details to a minimum. The following simple models were compared: (1) L. F. Richardson's model of the arms race, (2) R. R. Bush and F. Mosteller's stochastic model, (3) the Markov chains model, and (4) a revision of the Richardson model. It is shown that none of these models are adequate and that they fail to account for the large concessions made by negotiators toward the final stages of the negotiation process (the "end game"). In addition, these simple models ignore the negotiator's beliefs and expectations about the opponent's intentions, but a negotiator's behavior is based on such factors.

With regard to the outcome of negotiations, the author again restricts himself to simple models. They focus on how the negotiations end and prescribe a "rational" solution (agreement value) for the participants. The following models are contrasted and compared: (1) John Nash's model of the general bargaining problem, (2) the L. S. Shapley value, and (3) solutions proposed by R. B. Braithwaite and by Howard Raiffa. The results indicate that Nash's model provides the best fit.

In chapter 9, the author attempts to integrate the results of the comparisons and presents some assumptions and hypotheses for the development of a descriptive theory. This chapter is the most interesting and probably the most important contribution of the book. The author's descriptive theory is based on the concept of "toughness" (in demands), and from a set of assumptions about the "toughness" of a negotiator, some predictions about the consequences of "toughness" are derived. Although most of these predictions are intuitively obvious (e.g., tough negotiators tend to receive higher payoffs than soft negotiators, but toughness also tends to make an agreement less likely), it is important to build some theoretical structure from which such obvious predictions can be logically deduced.

In the final two chapters, the author concludes that simple models are not adequate and that more complex models which take into account the negotiator's expectations should be developed and tested. A model recently proposed by J. G. Cross is briefly described and examined, and the author concludes that Cross's model is one of the most promising ones and deserves serious attention. However, he shows also that it does not specify optimal negotiation behavior and consequently is not a normative model. This suggests that, although the author's assumption that a descriptive model can be developed from a normative model may be valid, at this stage of theory and research it may be more fruitful to start with a descriptive theory, accepting the fact that human beings may not always be "rational."

Small-Group Cultures. By Tom McFeat. Elmsford, N.Y.: Pergamon Press, 1974. Pp. v+209. \$12.00 (cloth); \$7.00 (paper).

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State University of New York at Buffalo

A troublesome book—but first, a description of its plan. The author's purpose is to understand small-group cultures. In the process he would like to become associated with J. M. Roberts who, after discovering in 1951 that each of the Navaho households he studied had its own distinct culture (microculture), suggested that microcultures be studied in their own right: they are universally distributed in human societies; they are relatively easy to observe; their features can be measured empirically; meaningful comparisons can be made; they vary in persistence and in independence from the larger society; and so on.

That was Roberts in 1951; McFeat, as of 1974, begins by comparing experimental groups in the laboratory with natural groups in the field: he uses references to Bavelas and to Bales in the lab and in the field, to soldiers crossing a river and to a whale hunt. While helpful conclusions seem to elude the author, the discussion allows him, as he says, to move on to "Natural Small-Group Cultures," the second chapter. There he presents the conceptions of three prototypical cultures: first, a microculture whose chief function for members is to screen, select, interpret, and order information about the ongoing experiential scene (a content-ordering culture); second, one whose chief functions are to define tasks and to coordinate activity for their accomplishment (a task-ordering culture); and third, one whose major function is to provide models of the group to the group in the service of group identity and possibly integration (a group-ordering culture).

In the third chapter the author reports several pilot runs in his attempt to create small-group cultures in the university laboratory, specifically exploring the vicissitudes of cultures being transmitted from old-timers to newcomers who in turn become old-timers passing the culture on to another generation. In chapter 4 he attempts to relate processes in the laboratory groups (which never become clear to the reader) to the three prototypes. The book concludes with a discussion of "indoor-outdoor anthropology" which, surprisingly, turns out to be speculation on how indoor cultures (such as the Kiva culture of the Zuni) might, through an expansion of networks, be linked with outdoor communes, forming new communities based on knowledge of social and cultural processes.

Why do I say a troublesome book? There are important ideas here: small groups do develop distinct cultures; such cultures do order content, set tasks, and create self-reflective models; small-group cultures do perform critical functions both for the individual and for societies; they can be studied empirically. The book is troublesome because these potentially fruitful ideas are smothered by a horrendously entangled overgrowth of confused thinking, irrelevant associations, disconnected abstractions, non

sequiturs, lost antecedents, misplaced paragraphs, and random verb forms. And for the toughened reader who is determined to hack it anyway, there are distracting pricks from statements such as: "all small-groups . . . are organized on an exclusive verbal and proxemic basis; therefore, they are by definition nonliterate" (p. 105); "experimental groups . . . are clear examples of primary groups" (p. 5); ". . . we can assert that while myths, though not themselves groups, can not be . . ." (p. 110); "we regard community plans (layout of dwellings, streets, etc.) to be *myth*" (p. 98); and "along the dimensions of group formation, learning, adjustment (etc.) small-groups are alike" (p. 6).

Unfortunately, the light does not get through; the ideas die on the vine. A loss to all of us: since small-group cultures actually do perform critical functions for individuals and societies we need to overcome our ignorance about them. Perhaps the author will give it another try.



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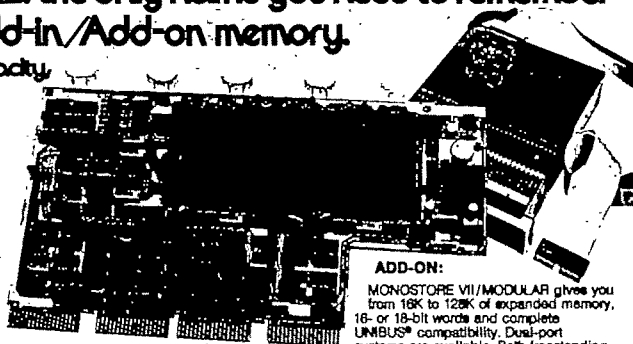
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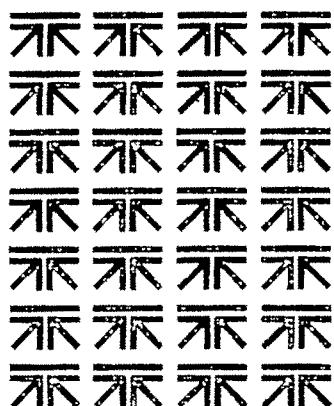
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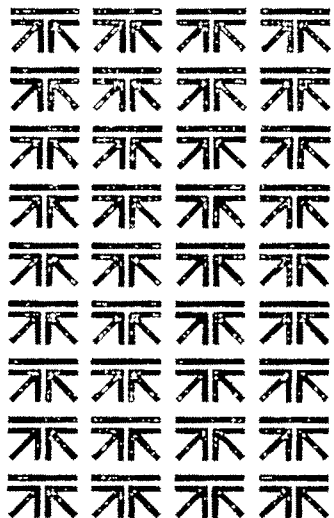


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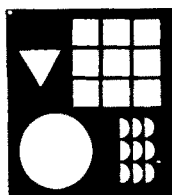
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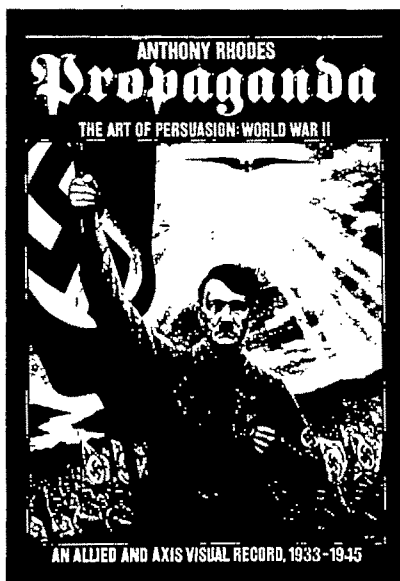


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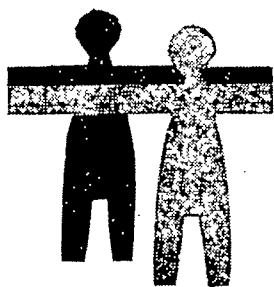
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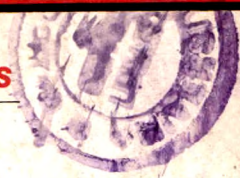
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- . 1963*b*. "The Theory of Change and Response in Modern Demographic History." *Population Index* 29 (October): 345–66.
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Structural Change in Social Processes¹

Gudmund Hernes

University of Bergen

Models of structural change should be able to explain constancy as well as change, must combine micro- and macrolevel analysis, and must encompass endogenous sources of change. It is necessary to distinguish among three levels of structure of a system: output structure, parameter structure, and process structure. With these concepts four basic types of change processes can be identified: simple reproduction, extended reproduction, transition, and transformation. These are discussed and illustrated, as well as some important special cases such as dialectical change, overintegration, and catastrophe.

The main concern of theories of structural change is how men react to conditions of their own making and in so doing change these conditions. However, only a relatively small proportion of the studies carried out by contemporary sociologists focus on the whole cycle. There are several reasons for this. To collect data as a process unfolds is often infeasible for practical reasons and because of career requirements. Data available from other sources often have deficiencies—for example, they usually lend themselves only to aggregate or trend analysis since the records ordinarily available are marginals. Survey technology is well developed and competence in practicing it widespread, and we are inclined to stay with what we can do well rather than venture into unfamiliar areas with inadequate tools. However, survey research often becomes a kind of aggregate social psychology, and when it restricts itself to “enumerating individual characteristics, [it] treats the individual as if he were detached from his environment and hence as an abstraction” (Boudon 1971, p. 48). This state of affairs has perhaps been reinforced by an ideological and professional bias against the works of Marx, which embody the most comprehensive theory of social change.

The concern of this paper is not a particular theory of social change, but logical requirements of theories of structural change and strategic considerations involved in constructing such theories. Or put differently,

¹ The draft for this paper was written while I was a Fellow at the Center for Advanced Study in the Behavioral Sciences, where Miriam Gallaher was of great assistance. Earlier and extended versions were presented at the meeting of the International Sociological Association in Toronto, 1974, and at the Mathematical Social Science Board conference on organized complexity and structural change in St. Louis, 1975. I have benefited from discussions with Hayward Alker, Jon Elster, Adam Przeworski, and John Sprague. I am especially grateful for the detailed comments of an anonymous reviewer of the *AJS*. None of them are responsible for defects in the argument.

our concern is with the properties that models of change must have. In this analysis I draw on theoretical procedures, designs, and models used in other disciplines, notably population theory, ecology, and economics. This is helpful in clarifying the logic of theories of change, much in the same way as the organismic analogy is helpful in elucidating the logic of functional analysis, since organisms are self-regulating systems (Merton 1957*b*; Hernes 1971).

Three of the desiderata of theories of social change are the following. First, we should be able to use the same basic approach in studying both constancy and change. This implies that a stable structure should be seen as a process in (temporary) equilibrium. It also means that the theory must include an explanation of why the parameters governing the process are themselves regenerated or not changed. This has terminological implications as well, since it is natural in this case to talk about the "reproduction of a structure," meaning *inter alia* that the system in question regenerates the conditions of its own persistence. In short, structural change must be described in terms of the processes that generate the change; structural stability in terms of the processes which not only maintain the stability but also maintain the processes which maintain the stability.

Second, we should be able to incorporate intrinsic sources of change into our model of a system. Changes in social systems may be induced from the outside, but not all changes can be thus induced (cf. Elster 1971).² Moreover, if an item is borrowed from another culture, we should be able to use the same kind of explanation for its adoption as if it had been invented at home. The sources of change should also be intrinsic to the theory in the sense that effects of actions provide or change premises for further actions.

Finally, social change is mediated through individual actors. Hence theories of structural change must show how macrovariables affect individual motives and choices and how these choices in turn change the macrovariables. This point is elaborated in the next section.

²The same point has been made for some time in structural analysis. For a recent review, see Merton (1975). In several works Nisbet has taken the opposite view and argued against the position that the sources of structural change can be immanent: "There is no evidence whatever that crisis is ever self-engendered in institutions; ever the result of immanent factors alone, no matter how 'explosive' long used metaphor makes these seem to our eyes" (1972, p. 34). However, in other places in the same work, he puts his position more moderately, e.g., by stating that very few of the major changes in human history can be understood save in terms of the impact of external events (p. 30). There are several problems involved here, such as system reference and system boundaries. I will not pursue this point further here; I will only repeat that it is logically impossible that all changes in or of systems are always exogenously generated.

MICRO- AND MACROANALYSIS

In microanalysis one is ordinarily concerned with the preferences, attitudes, and actions of small decision units such as individuals, households, or firms. In macroanalysis the task is to investigate phenomena which cannot be distributed among individuals or which appear as average or aggregate values for groups or populations (cf. Carlsson 1968, p. 706). For example, institutional arrangements such as the legal corpus cannot be divided among small decision units, while crime or suicide rates are fairly stable over time when calculated for a whole population. One could say that macrophenomena correspond to what Durkheim (1938) called "social facts," which are external to individuals and either have a power of coercion over them through the existence of sanctions or can be recognized by rates of occurrence which are distinct from their individual manifestations (pp. 3 ff.). Hence microtheories as constituents of theories of structural change often incorporate macrophenomena as constraints or incentives, and macrophenomena are generally the intended or unintended results of aggregates of individual actions or of collective decisions. In a complete theory of structural change, both the micro- and macrolevels and the interrelationship between them have to be incorporated.

It is useful to try to list key properties of individual actors at the micro-level and properties of populations or societies at the macrolevel. I shall keep them as few as possible in number.

Of individuals let us assume that they are purposive actors. This means that they have a set of preferences or priorities; that they seek the best or, at least, satisfactory ways to realize their goals; that this is done under bounded rationality, meaning that they act on uncertain expectations and partial knowledge; and that their actions are "result controlled," meaning that when the outcome of an action deviates from the intended result, the actor may modify his conduct. It also means that if an actor accidentally hits upon a better solution to his problems, he will adopt it. The preferences of the actors are in large part determined by their socialization, and they have socially determined capacities such as rights, abilities, and competences. Both the ends men pursue and the means at their disposal are decided largely by their past and present locations in the social structure—by their biography and their social position. But in pursuing their goals men may modify the constraints under which they choose: actions may change the parameters of choice. Another way of stating this is to say that the plans individuals seek to carry out are determined by their expectations about the future and about the distribution of possibilities, by their interests, and by the means they control. All of these are dependent on preceding outcomes and are revised or undergo change from time to time, mainly as a result of previous actions—both one's own and those of others.

The actions of others are especially important when they make one realize what his interests are, or when one can learn from and adopt their strategies, or when their plans are incompatible with one's own so that the actual outcomes diverge from the planned ones and interests are thwarted. From the individual point of view this process can be represented as in figure 1.³

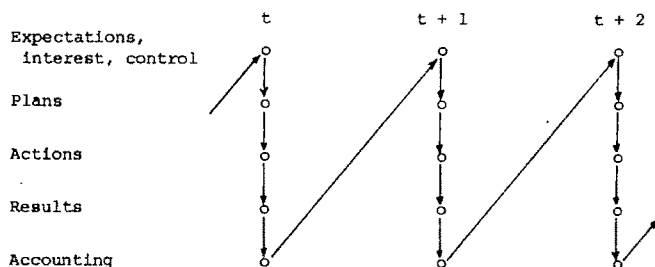


FIG. 1.—Temporal interdependence from the individual point of view

The macrostructure has two sets of characteristics. One is the institutional or collective set, which includes, for example, reward structure or stratification system, language, legal rules, and material structure in the form of tools, transportation system, products available, etc.—in short, it consists of structural constraints on available alternatives. The other set consists of the aggregative or distributive outcomes of choices of alternatives, such as marriage rates, prices, sex ratios, age structure, and so on. These are partly under human control and partly the result of chance processes; in part they can be affected by conscious action, but to a considerable extent they are unintended. In any case they are properties not of individuals but of populations.⁴

In theories of social change we are interested in the relation between the micro- and macrolevels. As in other models of structural change, there is a circular relation between the two levels. Norman Ryder (1964) writes of the basic population model:

The circle of formal analysis moves from (1) individual acts of procreation and death to (2) cohort processes of fertility and mortality, and thence to (3) cohort age structures. These are translated into (4) period age structures which combine with period translations of the cohort vital

³ This diagram and conceptualization was inspired by Bentzel and Hansen (1954) and goes back to the so-called disequilibrium method for period analysis of the Stockholm school. In the diagram t denotes an interval of time; hence the relations within a period may well be relations through time.

⁴ It may be added that although aggregate values or rates depend on *all* individuals, they do not depend on *any* (one) individual. This distinction, which is common in philosophy (see for example the article, "Any and All," in the *Encyclopedia of Philosophy*), seems to have provided a difficulty for Durkheim and a source for his tendency to reify "social facts."

Structural Change in Social Processes

processes to yield (5) the births and deaths which change the size of the population. This mode of analysis presents the problem of structural transformation in terms of the processes that shape and reshape the structure. Thus it is attuned to the tendency of present-day science to regard events rather than things, processes rather than states, as the ultimate components of the world of reality. [P. 449]⁵

The basic population model generally uses a probabilistic model of man rather than a purposive one—as a kind of conceptual shorthand. However, in a complete theory, people's purposes and premises have to be taken into account explicitly. Therefore we have to substitute individual choices for individual occurrences and social processes for cohort processes. But then it is also clear that the macrolevel provides a context for individual choices by its reward structure, incentives, and constraints. Hence theories of structural change have to "specify the ways in which macrovariables are translated into individual motivations" (Ryder 1964, p. 458). Examples of this abound: criminal law affects personal conduct, the divorce rate may enter as a premise into individual decisions whether or not to stay married, roads affect where and how one travels, etc. Or, in the terminology of industrial economics, market structure (number of sellers and buyers, size of their businesses, barriers to entry, cost structures, etc.) affects market conduct (pricing behavior, product strategy, legal tactics, etc.), and conduct may in turn modify market structure (cf. Scherer 1970, pp. 3 ff.).

So not only do such social parameters affect the choices of individuals, individual choices of action also change the parameters of choice by opening or destroying alternatives. This may occur through a cumulative series of choices, as when members of a cohort marry and thereby reduce the number of available candidates for those not yet married (cf. Hernes 1972). Or it may be the aggregate result of independent decisions. In hog cycles, for example, the sum of individual choices modifies expectations: a high price for pork encourages farmers to increase production; the increase forces prices down the next year, leading to reduced production which pushes prices up the following year, and so on. Or actors may impose new constraints on themselves by collective action or joint decisions in order to improve their welfare. Preferences and expectations may change by similar processes.

The mutual dependence of individual choices and actions on the one

⁵ A similar argument is made by Coleman (1964): "The most fundamental approach to the development of structural measures is through the use of the process imposed upon the structure. For as indicated earlier . . . , structural measures should be measured by aspects of sources of the structure or aspects of its consequences. The process imposed provides, if it is a process related to the inquiry for which the measure is to be used, an appropriate integrating device for compressing the complex structure into one or a few measures" (pp. 459-60).

hand, and macrovariables or parameters of choice on the other, can be illustrated as in figure 2. Material conditions, social constraints, and cultural norms evoke and induce the interests that the members of various groups seek to promote; but in their efforts to promote these interests, they modify the social structure, alter capacities, generate new interests,

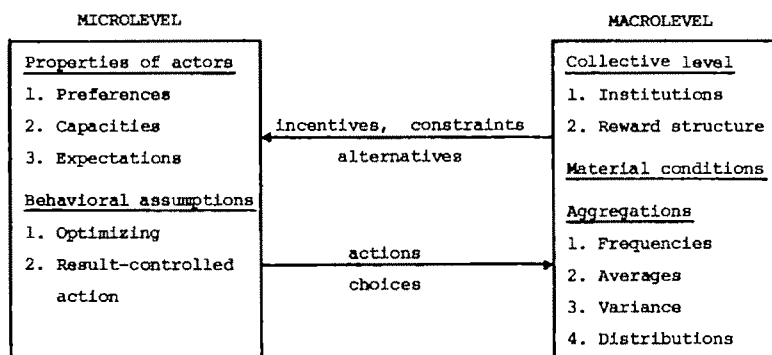


FIG. 2.—The relationship between microlevel and macrolevel

and revise expectations. There is another interrelation between the micro- and macrolevel as well. We make general assumptions about the actors—that they have preferences, resources, information, and causal theories. But the actual content of these categories is historically determined, so that the actors as real subjects represent an intersection between these general assumptions and the specific social and temporal conditions under which they act. In a similar fashion we can construct general models for macrophenomena, but applications of them must of course include historical content.

Not only may a group change the conditions of its own existence in the pursuit of its interests, it may also change the conditions of existence for another group by opening new possibilities or imposing new constraints—by destroying or providing alternatives. It is important to note the asymmetries that usually exist in the capacities of groups to change the choice parameters for each other, and particularly their impact on the probability of social conflict.

Having argued that analysis at both micro- and macrolevels is necessary in theories of structural change, I now have to state more clearly what I mean by structural change. When this is done, it will be possible to identify different types of change processes. In illustrations of them, the interrelations between micro- and macrolevel analysis will again be seen.

STRUCTURE, PROCESS, AND CHANGE

A structure is a configuration of parts, and a structural description is a characterization of the way the components in a set are interrelated. Ord-

narily we consider an observed structure, such as the proportions of a population found in different social strata, the result or outcome of a process. A standard procedure in sociological analysis is therefore to identify and construct models of the generating process in order to explain the resulting structure. However, in analyzing structural change it is crucial to note that we are dealing with structures at different levels, and that these levels are interrelated in such a way that the structure at one level is the output of a process which itself has a structure. Hence we must distinguish between output structure and process structure.

These notions can be clarified by an example. A simple output structure is the composition of a population by age and sex, which may be represented by the proportions of the total population found in each category. But this population structure is the outcome of a birth and death process which can be specified in a set of equations, so that the functional form of the process has a definite structure. The parameters of the process also have a structure, given by the age-specific fertility and death rates. The population composition may change even when the fertility and death rates remain constant. Hence it is clear that structural change of this composition "is caused by a discrepancy between the extant structure and the processes which are responsible for creating that structure" (Ryder 1964, p. 461).

Thus it seems useful to distinguish among structures at three levels. The first is the *output structure* or distribution of results, such as that given by a population pyramid. The second is a specification of the logical form of the process generating these results, which I will call a *process structure*. The simplest and most compact way of expressing a process structure is by a mathematical model or formulas which give its functional form, such as the equations for the birth and death process of a population. But many process structures, for example that for the generation of a compulsive neurosis, are intractable to mathematical expression and may also be hard to express precisely in verbal terms. Finally, the parameters governing the process, such as age-specific birth and death rates, take on definite values in concrete situations, and their configuration may have a certain constancy. Hence it is natural to speak of a *parameter structure*. The functional form and parameter values of a process can also be called an *operator*, and what it acts upon, the operand (cf. Ashby 1963, p. 10). Two examples of the three levels are given in figure 3.

Survey analysis usually takes a snapshot of an output structure, and analysis is often carried out on the implicit or explicit assumption that the total system is in equilibrium for the time period considered. For example, if a poll is taken, it may be assumed for analytical purposes that the relative sizes of the different segments of opinion are stable, that is, that the numbers of individuals entering and leaving each seg-

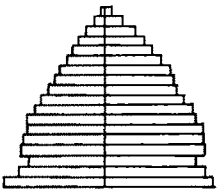
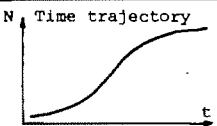
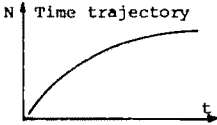
OPERATOR STRUCTURE		OUTPUT STRUCTURE
FUNCTIONAL FORM	PARAMETER VALUES	
Population model with survival probabilities and age-specific birth rates, e.g. $N_t = \sum_{x=0}^{\infty} N_0 l_x m_x$	$[l_x] = ?$ $[m_x] = ?$	
Contagious diffusion $\frac{dN}{dt} = kn(N - n)$	$k = ?$	
Constant source diffusion $\frac{dN}{dt} = c(N - n)$	$c = ?$	

FIG. 3.—Two examples of operator structure (divided into process structure or functional form and the structure of parameter values) and output structure: (1) the population model (N_t is the number of offspring produced by a single generation N_0 born at some earlier time, l_x is the probability of survival from birth to age x , and m_x is the age-specific birthrate); (2) two diffusion models with different functional forms (k and c are diffusion parameters, N is the total number of potential adopters, and n the number who have adopted an item at a given point in time).

ment cancel out so that there is no net gain or loss in any of them. On this assumption the relative forces operating on the individuals, or the parameters guiding the process (the transition rates), may be estimated. If a process is in equilibrium, and the output structure therefore constant, it is usually much simpler to estimate the parameters than when the output structure is changed by the operator. In the latter case data at different time points or additional assumptions are needed to identify the process structure.⁶

The time trajectory of an output structure may also vary, even when the process structure and parameter values remain unchanged. It can move toward an equilibrium. For example, it was shown by Lotka (1922) that the structure of the population pyramid (given by age composition, sex ratios, etc.) reaches an equilibrium shape, with the proportion in each category becoming fixed for a fixed set of birth and death rates, and that it returns to the same equilibrium if the population is disturbed. Another example is found in the equilibrium sizes of social strata that result from

⁶ For a thorough discussion of the concept of structure, see Boudon (1971).

an ordinary Markov chain model of social mobility. Equilibrium can also result from optimizing behavior, as when the ratios of workers performing different operations on a production line are stabilized after trial and error or an analysis of work flow—what Marx (1906) called the “iron law of proportionality” (for anthropological examples, see Barth [1966]). In all these cases it has to be assumed—if the models are meant to describe real situations—that another outcome of the process is that of preserving its nature and parameter values.

But the time trajectory need not move toward equilibrium. It can move toward extinction, as for example in Marx’s description of the evolution of the capitalist class. Or it may fluctuate as in the hog cycle, in which the aggregate result of the actions taken last year modifies the probability distribution of farmers taking the same action this year, everyone doing now what would have been right for an individual in last year’s situation. In all cases, change or stability of the output is the result of an operation or a process with a specific structure. Stability and change are both process outcomes generated by the dynamic interrelations of the component parts of the system.

In short, even when process structure and parameter values are assumed constant, we have a powerful tool for the analysis of output structure, since a model of the process may allow us to explain how a resulting pattern is generated, maintained, changed, or destroyed. Empirical analysis of structural change in the social sciences is preponderantly analysis of change in output structure with a fixed process structure or operator form and fixed parameter values. Theory construction or model building usually starts with an observed output structure or time trajectory and is followed by a search for a process which can account for the observed result, usually by eliminating alternative formulations or competing theories.⁷ The model of the process finally settled on can often be used for conditional projections, as when economists in input-output analysis use the “technological matrix” to evaluate the repercussions for all industries of a change in the final demand for the products of one of them.

When the parameter values guiding a process are independent of time, the process is considered stationary. A process can become nonstationary in different ways. For example, the parameter values may change as a function of time but independently of the output of the process, as is assumed in nonstationary Markov models of social mobility. Parameter values may also change as a result of changes in exogenous variables.

However, the output of a process may affect its structure, either by affecting parameter values—as when increasing crowding raises the death rate—or by affecting the functional form of the process—as when crowd-

⁷ See, for example, Stinchcombe (1968) and Lave & March (1975) for expositions of this procedure.

ing leads to emigration where there was none before, or to other social inventions. Therefore, to have a complete paradigm for analysis of structural change we must be able to account not only for changes in the configuration of parameter values, but also for change of the process structure. And we must in principle be able to explain such changes endogenously—that is, by variables already included in the model and by their interaction. To accomplish this, we must have on the one hand a feedback connection from output structure to parameter values, and on the other a feedback connection from output structure to operator form. Only then will we be able to explain how a generating process itself is changed or maintained.

There are of course many examples of the consequences of processes leading to their alteration. For instance, the liberal marriage rules and divorce laws introduced after the Russian Revolution not only changed aggregate marriage and divorce frequencies, but also created great problems in the care of dependents and increased the housing shortage because of decreased cohabitation; hence the marriage laws were altered again, and the "bourgeois" family system was reintroduced. Exclusion from suffrage in many countries led to revolutionary threats which made those in control extend voting rights. Anticipated benefits made the incumbent party repeal the Corn Laws in England in 1846. Cutthroat competition makes firms reach agreements in order to reduce it, thereby changing the structure of the market. These examples illustrate the fact that we must ask for whom the output structure is beneficial or costly—how structure affects motives—and who, if any one, has the power to change the generating processes. Furthermore, they illustrate the fact that the consequences that some want to modify may be the outcomes of either collective decisions or aggregates of individual decisions (cf. Stinchcombe on "Marxian functionalism" [1968, pp. 93 ff.]).

The feedback from system output to the setting of parameter values and to the determination of process form necessary for an endogenous model of social structure can be represented in simple symbolic form. If we write the time trajectory of the output structure Y_t as a time function T of the process or operator with the form f and parameter value a , and of the input x at time $(t - 1)$, we have

$$Y_t = T(f_a; x_{t-1}). \quad (1)$$

(These and the following symbols may also represent vectors.) The parameter values may be affected by the output Y_t according to the relation

$$a = g(Y_t). \quad (2)$$

The form of the process structure is also a function of the output:

$$f = h(Y_t). \quad (3)$$

Finally, the input at time t may also be determined in part by the output at an earlier point, for example,

$$x_t = q(Y_{t-1}). \quad (4)$$

Hence the interconnections among the three levels of structure can be diagrammed as in figure 4. One may say that these equations represent

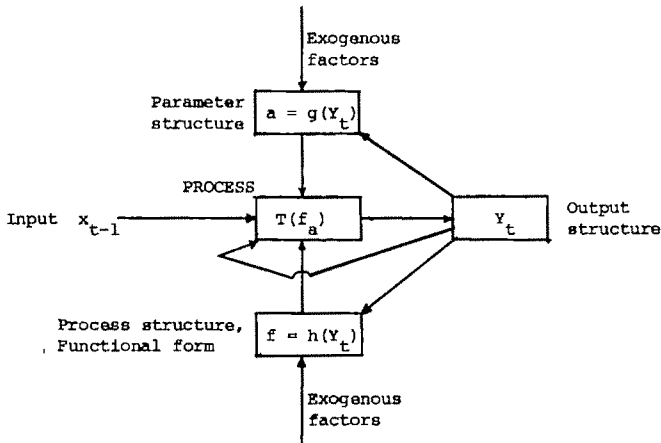


FIG. 4.—The interrelationships among input, process structure, parameter structure, and output structure.

a complex hierarchical control system in which parameters and parameter values may change within the closed loop. After a change in or of parameters—in or of the structure of the process—the system performance may be more optimal or less so from a human point of view, or more optimal for some groups and less so for others. The figure also indicates where we may find exogenous sources of change.

TYPES OF CHANGE PROCESSES

Given the possibility of change or stability at three levels, we may ask some questions and answer them in the manner of a Guttman scale (see table 1). The four combinations of answers identify processes at the macrolevel to which I will give the following names: (1) simple reproduction, (2) extended reproduction, (3) transition, (4) transformation. The four other possible combinations can be considered variations of these. Hence I shall comment on these four types but also note some important special cases.

TABLE 1
FOUR POSSIBILITIES FOR CHANGE OR STABILITY AT THREE LEVELS

ITEM	TYPE			
	1	2	3	4
Does the output structure change?	No	Yes	Yes	Yes
Do the parameter values change?	No	No	Yes	Yes
Does the process structure change?	No	No	No	Yes

Simple Reproduction

In production the used up means of production must be replaced to keep the process going. Marx used the term "simple reproduction" to describe this process of replacement for production on the same scale, that is, with zero growth rate. However, to maintain a steady output, not only raw materials and worn-out machines must be replaced, but also the number and quality of workers—labor must also be reproduced. As Oskar Lange writes: "The production process in which the means of production are fully replaced and in which the amount of labour is constant (and, moreover, as we have assumed above, there is no technical progress) is called *the process of simple reproduction*. Under conditions of simple reproduction, the national economy does not expand: this means that it is stationary" (1969, p. 3). However, Marx's notion of simple reproduction went further than this: he placed in the foreground not only the reproduction of goods and labor, but also "the reproduction (i.e., maintenance) of the capitalist-class character of the entire process of production" (Marx 1967, p. 396; cf. Morishima 1973, chap. 9). Men reproduce not only products, but also the conditions under which they produce. Under simple reproduction, then, the system operates in such a way as to maintain not only the output structure but also the functional form of its structure and parameter values. The system produces the requisites for its own continued existence. However, instead of staying at this abstract level, Marx ties the process to individual motives and the constraints under which choices are made, for example, by pointing out that self-preservation requires daily consumption, which in a money economy requires payment and hence wage labor—but that wage labor tends to maintain these very constraints.

We have an example of simple reproduction in the model of a stable population, which has fixed proportions in the different age/sex categories and is the outcome of a birth and death process with constant age-specific fertility and death rates. The stable proportions are states of equilibrium which represent "latent structural propensities of the process characterized by the [parameters]" (Ryder 1964, p. 456), since they are the values

toward which the proportions move when parameter values remain constant. However, in this model the causes of the stability of the parameter values are most often implicit rather than explicitly stated.

A better example—precisely because it takes into account how social relations are reproduced through the incentives and constraints they provide for the actions of individuals—is found in Bhaduri's (1973) study of agricultural backwardness under semifeudalism. The least privileged sharecroppers in West Bengal, called *kishans*, have virtually no land of their own, provide little or no capital for production, and typically have security for only one production cycle. *Kishans* have an incentive to work even without supervision, since they have a legal share in the harvest. However, they are in a perpetual state of indebtedness because they make their payments on the lease at harvest time, and at interest rates which necessitate borrowing before the next harvest. Their landowner is also their lender of consumption loans, so that each *kishan* borrows from the same source from which he leases land. He cannot move without settling his debt, which is a constraint on movement, and he is not credit worthy outside this relationship, which is an additional incentive to stay. Interest rates are usurious since the *kishan* has no access to commercial banking (having no assets to borrow against), so the landowner has a monopoly and can set the terms on which loans can be obtained. Nor has the *kishan* access to the commodity market so as to take advantage of prices when they are high. Instead he is the victim of price fluctuations, having to borrow paddy from the last harvest at a time when prices are high and repay with interest just after the next harvest when prices are low. A sudden rise in his consumption level (e.g., due to religious ceremonies, weddings, etc.) plunges him deeper into debt. The key question is why technological innovations are not introduced into this system and social relations changed. Bhaduri finds the answer in the dual exploitation system—the simultaneous operation of the landowner's property right and of usury based on the *kishan's* having to borrow regularly for consumption. The continuation of the system requires that the *kishan's* available balance of paddy fall short of his consumption requirement. Hence technological improvements which increase his productivity and reduce his dependence on consumption loans also weaken the semifeudal system based on debt. And the landowner will be discouraged from introducing technological improvements bringing gains which fall short of his loss in income from usury due to the reduced or eliminated consumption loans: "Indeed, in certain circumstances . . . the semi-feudal landowner may be caught in an almost paradoxical situation: he is rationally (i.e., on economic grounds) put off from a *small* improvement . . . which perpetuates indebtedness but reduces his overall income, while he is also put off from a *big* improvement because it makes the *kishan* free from perpetual debt

and destroys the political and economic control of the landowner over his *kishan*, even though on exclusively economic grounds, it may be profitable to him" (Bhaduri 1973, pp. 135-36).⁸ In general, simple reproduction either implies circular causation (Myrdal 1957) in which two or more social forces mutually maintain each other over time (such as white prejudice and black low standards of living), or can be characterized as Stinchcombe characterizes "historical explanations," according to which the effect becomes a cause of the same effect in the next period. Simple reproduction has also been the object of many functional studies. But one should be careful to note the role played by microlevel assumptions about individual motivations in the above explanation of reproduction.

Extended Reproduction

If the means of production are not fully replaced, output will fall and we can talk of contracted reproduction. If, on the other hand, the means of production are increased and labor is increased correspondingly, we can talk of expanded reproduction (cf. Lange 1969, pp. 3-4). Generally we can talk of extended reproduction when the output structure changes but the functional form of the process which generates the output, as well as the parameter values, remains constant. And we can talk of reproduction, simple or extended, as long as the output structure maintains or leaves unaltered the process structure and parameters generating the output. Note, however, that a stable functional form of the process structure and constant parameter values can lead to dramatic changes in the time trajectory of the output. For example, a population adding new members as a constant proportion of its present size (growing exponentially) may overshoot the carrying capacity of its environment, resulting in a population "crash" (a drastic fall in population due to starvation)—the population has been unresponsive to approaching the carrying capacity of its environment. Expanded reproduction can also be illustrated by cyclical fluctuations in output structure, whether they be constant, convergent, or divergent, such as can be found in economic cobweb models or in ecological models of prey-predator relations.

Transition

Transition was defined as a change not only in output structure but also in parameter values. An example is found in demography: "the theory of

⁸ Bhaduri continues: "This sort of reasoning leads me to argue that the semi-feudal production relations operate as a barrier to the introduction of improved technology that results in higher output per *kishan*. This also gives a precise form to Marx's general idea that the relations of production may become a constraining factor on the release of the 'forces of production' and, according to any materialistic interpretation of history, such a situation is ripe for historical change" (p. 136).

demographic transition" is precisely about change in the parameter values and output structure but not in the functional form of the generating process. This example is good also because it illustrates the interdependence between analysis at the macro- and microlevels in a complete model of structural change.

At the macrolevel, the theory is intended to interpret the demographic changes in Europe in the last century. It describes three stages in the development of fertility and mortality levels. Stage 1 is characterized by high fertility and mortality rates, the fertility rate being sustained by strong pronatalist norms while mortality rates are not under effective control. In stage 2, death rates have come under human control and are reduced. Combined with the high fertility rates, this results in rapid population growth. In stage 3, after a lag or response time, the birth rate begins to fall as a consequence of conscious intervention, resulting in a new equilibrium in which both fertility and death rates are consciously controlled and both low.

The output structure, as summarized in the population pyramid, is also changed as a consequence of these changes in the vital rates or parameter structure: the resulting age composition and sex ratios are altered. (There are many statements of the demographic transition theory; for a recent and succinct one, see Teitelbaum [1975].)

These statements about the changes at the macrolevel are useful for descriptive and comparative purposes, for example, for investigating the extent to which developing countries follow the same pattern. But this theory of change is not a complete one, since we have only stated *that* the vital rates change, not *why* they change. The why can be broken up into two questions, each of which has to be answered by assumptions about what happens at the microlevel, in the hearts and minds of the people. We must ask why mortality rates go down as an aggregate effect of individual actions and what causes family practices to change so that fertility rates decline.

The mortality question is the simpler one to answer. If we assume that people desire longevity and that their behavior is result controlled, any reasonable procedure for prolonging life will be tested. Those that work (and some that do not) will be adopted (as witness people nowadays who stop smoking, go on diets, etc.). Most important for the decline of mortality in the last century were improved diet and measures of public health, particularly the former, it seems (McKeown, Brown, and Record 1972). Some of these improvements were the result, not of individual choice, but of collective actions. In any case self-interest is used to explain why people adopted new practices. But we also have to explain why babies, who cannot choose for themselves, also had lower mortality rates. This can be attributed to the value attached to children. In short, it is assumed

that people will do what they can to prolong their own lives and those of their children, bringing about lowered death rates when they find effective means.

Decline in fertility rates is harder to explain (for reviews of theories from different perspectives, see Leibenstein [1974] and Namboodiri [1972]). At the individual level the question to be asked is why fathers or mothers or both find it beneficial to conceive fewer children to an extent that aggregate rates fall. It is assumed that if fewer children are desired, the means to reduce their number will be found. For decline of fertility within marriage to take place, it must be assumed that the number of children conceived is left to families to decide and that such choices are not considered immoral. Samples of individual-level theories are the following:

A. *The ideal number theory.*—It is assumed that parents desire a target number of grown children. When parents learn that infants die less frequently than before, so that more children grow up, they need conceive fewer children to assure themselves of having the desired number and can postpone marriage without risking all children dying before the fertile period is at an end. It is important to note that the target number is family size, not population size (the ideal of zero population growth is recent). As Bogue (1969) states:

The theory of demographic regulation is premised on the assertion that every society has a set of norms that guide population growth. *These norms are not explicit opinions about the desired population size or the optimum rate of growth. Instead they are opinions concerning what constitutes the ideal size of the completed family, or the number of surviving children a couple ought to have when it reaches the end of the reproductive period.* Completed family size is an entity that may be translated directly into population growth and is therefore a cultural value reflecting the "demographic cultural policy" of the society. Any society whose average members believe that it is good or desirable to have four or more surviving children either will grow rapidly or must face very high mortality. A society whose members agree to bear no more than two children is one that expects to suspend further growth, and expects very low mortality. [P. 52]

In short, the analysis must focus on the relation between the microlevel decision units and the incentive structure under which they act.

B. *The no-split-plots theory.*—If families are large and land is scarce, a high birthrate leads to poverty—population growth may overshoot the carrying capacity. Hence parents will attempt to delay marriage for those offspring who can claim rights to their land, but to encourage marriage of those who have no claims to land (e.g., farmers may try to marry off their daughters young but delay marriage of their sons, or may institute primogeniture to avoid dividing the land to such an extent that none can

be sustained on it, etc.). From this it is clear that the strategies pursued will depend on the actual constraints of the legal arrangements and the property system.

C. *Cost-benefit analysis of the nth child*.—Parents have a target standard of living, in relation to which they calculate the utility and disutility of an additional child (e.g., cost of housing, feeding, schooling or the opportunity cost of income and pleasure forgone, burdens of childbirth, etc.). The results of such calculations can vary systematically among social groups.

D. *The wage-labor theory*.—Opportunities on the labor market make a large family a handicap, particularly for neolocal families, and particularly for women. But neolocal family structure also means that family pressure to have more children is reduced. This theory can be considered an example of a very broad class which could be called "theories of structural incompatibility." Other examples can be found in many disciplines; for instance, the theory that a large or extended family is incompatible with industrialism because it imposes an opportunity cost, and hence that those two modes of social organization cannot function coextensively for any period of time.

Several things should be noted in this connection. First, each of the microtheories makes the theory of demographic transition logically complete as a theory of social change since each, along with the theories explaining a lowered death rate, can account for the sequence of the three stages of development. Transition theory has been criticized for not having any forcing principles in the sense of producing the inference that stage 3 will follow stage 2, "or for that matter that any stage will follow any other stage" (Loschky and Wilcox [1974], p. 215; see also Kammeyer and Skidmore [1975], and Loschky and Wilcox [1975]). Second, the microtheories can substitute for each other, or all can be considered simultaneously either as alternatives or to account for part of the observed variance. They are theoretical modules at the microlevel, so to speak. Third, these specific microtheories have different empirical implications. For example, if there is joint village ownership of the land (commons), we would not expect the individual peasant to be as motivated to limit his family or delay the marriage of his sons as would be peasants who own their own plots. It is probably fair to say that there have been too few studies of the demographic effects of such constraints and incentives embedded in the social structure, though some have been carried out (e.g., Mueller 1972; Sklar 1974).

Once we have accounted for why spouses desire to limit family size, the next question is what means they will choose to accomplish this end, aside from infanticide or inadequate child care. Davis and Blake (1956) have listed 11 possible intermediate variables through which any personal or

cultural factor that affects fertility must go; they divide them into three groups of factors that affect, respectively, exposure to intercourse, conception, and gestation and successful parturition. In a concrete analysis one has to explain why a specific combination of "intercourse variables," "conception variables," and "gestation variables" is chosen.

The purpose of this extensive example has been to demonstrate and underline the need for and interplay of macrolevel and microlevel analysis in a complete theory of structural change. Note may be taken of the fact that changes of parameter values at the macrolevel can be explained by constant preferences but changing constraints (e.g., constant desire for longevity exerting a selective pressure on diet practices, or the target number of children remaining constant after a decline in mortality), or by changing preferences (e.g., altered opinions about the ideal size of a completed family), or by concomitant changes of preferences and constraints (desire for smaller families due to the establishment of a market for wage labor, made simple to realize by new means of birth control). The changes in preferences must also be explained (as economists do, for example, when they argue that increase in income may lead to altered consumption preferences which persist even if income is reduced to its former level. For a recent technical discussion, see von Weizsäcker [1971]).

Transformation

By transformation is meant change in the functional form of the process structure or operator, generally leading to a change in output structure and its time trajectory.

A simple example of such a transformation is change from "contagious diffusion" (in which the rate of adoption is proportional both to those who have adopted an item and to those who have not, i.e., $k(N - n)n$, where N is the potential adopters of an item, n those who have adopted it, and k the parameter), to "constant source diffusion" (in which the rate of adoption is proportional only to those who have not yet adopted an item, i.e., $c(N - n)$, where c is the parameter) (cf. fig. 3 above). Such a transformation was found in a study of the diffusion of a new drug (Coleman, Katz, and Menzel 1966). Not only did the processes described distinguish between doctors who were integrated into diffusion networks and ones who were not, it was also established that the contagious process operated only in the early months of the drug's availability, and constant source diffusion later took over. So the nature of the process changed as the diffusion went on, contagion being most operative at the outset and then dropping off rapidly. At the individual level this was explained as being due to reduction of uncertainty: professional relations were most important in the early stages when objective knowledge about the effects of the

drug was most lacking and the doctors were most in need of information and reassurance from their colleagues.

Another example of transformation would be Ronald Dore's analysis of land reform in Japan (1959). In many ways it is complementary to Bhaduri's study of agricultural backwardness under semifeudalism. Dore's work also focuses on rents and interests paid by families of tillers, but goes on to describe what it took to make land reform viable, in the sense of what characteristics of the operation of the old order had to be changed to make peasants' budgets balance; that is, what it took to increase productivity (fertilizer), lower interest rates (access to banks), stabilize price levels (governmental support sustained by competitive elections), and ameliorate irregular consumption expenditures (change in wedding rituals). As Stinchcombe (1974a, p. 3) says in an analysis of Dore's argument: "The viability of land reform depends on the wedding ritual in Japanese villages."

Still another example is to be found in Scarlett Epstein's study (1962) of the changes resulting from the introduction of irrigation in two Indian villages. (However, irrigation was not initiated by the villages but was due to governmental efforts.) An interesting aspect of her study is the fact that the effects of the new technology and altered market conditions on economic roles and relations were quite different in the two villages. One village diversified and was more closely integrated into the regional economy, and the hereditary relations between Peasant masters and Untouchables disappeared. However, in the other village, Wangala, she found that irrigation raised the whole economy to a higher level in one stroke, but that it nevertheless remained wholly agricultural. Therefore its employment structure remained unaltered because the labor requirements for cash cropping could be met easily under the traditional system of hereditary relations:

The unilinear economic development in Wangala set up no incompatibility, no friction between the new wants and old ways, in the indigenous employment structure. . . . If irrigation of Wangala lands had brought about a reallocation of economic resources, this would have undoubtedly caused friction in the traditional economic organisation and led to its change. But far from upsetting it, irrigation in fact emphasized the existing economic differentiation and economic relations could continue unaltered. . . .

The theoretical point of interest . . . is that economic development may occur without any change in economic roles and relations, provided it does not result in a reallocation of resources or in an increased range of economic relations. Far from undermining the economic structure of any society such economic development may even strengthen the existing pattern of economic relations. [Pp. 316-18]

The only part of Wangala's economic structure that was changed by the

advent of irrigation was the economic role of wives: it became a matter of prestige for husbands to relieve their wives of the duty of helping them cultivate the land. At the microlevel, the differences between the villages are explained in terms of the differing preferences and constraints the technological changes were grafted on.

If we allow that change in technology or of "forces of production" is a change of process structure, this example is interesting because it did not change key aspects of the output structure. Borrowing a term from topology, often used in mathematical ecology, we can call such a system *structurally* stable, since the basic character of the output structure remains the same after the change of the process structure.⁹

Succession and Dialectics

Dialectics is one of the most nebulous concepts in the social sciences. Schneider has identified seven clusters of meaning with the concept: "These have to do with (1) unanticipated consequences; (2) goal shifts; (3) adaptations that, once made, inhibit more effective ones; (4) development through conflict; (5) phenomena of the type of contradiction, paradox, negation; (6) the "contradictory logic of passion" in particular; (7) dissolution of conflict in coalescence of opposites" (1971, p. 667). In addition one might mention a more "Hegelian" notion of conceptual advance: "wringing from [the facts of history] concepts which come closer to reality" (Stinchcombe 1974c, p. 31).¹⁰ But concepts not only serve to mirror reality, they are also tools in reorganizing it.

In this section I want to describe a particular kind of transformation which represents one interpretation of what is meant by "dialectical social change," since it can incorporate several of the meanings mentioned above. Some social processes can be described as simultaneously destructive and generative, or self-destructive and self-generative if we are talking of endogenous change. In looking at systems which while they destroy themselves are pregnant with new ones, it is possible to draw on a logical

⁹ It must be added that this formulation is less precise than the mathematical definition. Rosen (1970) defines structural stability in this way: "The persistence of the topological character of the trajectories with respect to sufficiently small perturbations of the equations of motion of the system is the province of the kind of stability called *structural stability*: we say that a system is *structurally stable* if the topology of its trajectories is preserved under sufficiently small perturbations, structurally unstable otherwise" (p. 85). He also defines conditional structural stability. Maynard Smith (1974) writes: "The dynamical system described by the original equation . . . is said to be *structurally unstable*, because the smallest arbitrary change in the equation in any direction will alter the whole topology of the trajectories in the phase space" (p. 11; cf. Thom 1969).

¹⁰ A good example of this kind of analysis is described in Steven Marcus's *Engels, Manchester and the Working Class* (1974), e.g., pp. 45, 61, 66.

isomorphism between "ecological succession" and "dialectical transformation."

The concept of ecological succession has been used for a generation in sociological analysis to describe the orderly replacement of one neighborhood population or land use by another, ever since the "Chicago School" borrowed it from biological ecologists (Park [1936]; for a recent review, see Aldrich [1975]). Let us first see how biologists use the concept.

If a system can attain its goals within some range of environmental conditions, we say that it is adapted to the environment. But adaptive behavior may change conditions so much that the system can no longer function under them.

In biology the tendency for "species to modify their own environment out of existence" and "create conditions under which later successional species are better competitors" (McNaughton and Wolff 1973, p. 345) goes under the name of "ecological succession." Usually such processes result in an ecological climax, in which populations reach some equilibrium size with growth rates becoming zero, and at the same time reproduce their environment or the conditions for continued existence. As Garrett Hardin writes: "It is not only true that environments select organisms; organisms make new selective environments. The conditions produced by a winning species may put an end to its own success. Grape juice favors yeast cells more than all others; but as the cells grow, they produce alcohol which limits their growth and ultimately results in a new predominant species, the vinegar bacteria" (1969, p. 284). Ecological succession thus corresponds to a process whereby the output causes transitions and transformations of the process structure that produced it, resulting in a sequence of different output structures and process structures. At the climax stage, the output structure maintains the process structure it is generated by—or put differently, the process reproduces itself. At the climax stage there may be one dominant species, but movement toward it can be characterized both by increased diversification through the generation of new niches with concomitant stabilization and by reduced diversity as in erosion and growth of deserts.

The logic of such a process can be represented both by differential equations and by diagrams. For example, if one assumes that the growth of a species is proportional to the size of the species ahead of it in the succession line and inversely proportional to its own size, the transformation chain can be represented in a series of simultaneous differential equations of the form

$$\frac{dS_j}{dt} = k_{j-1}S_{j-1} - k_jS_j, \quad (5)$$

where the left-hand side indicates the rate of change in species j . We would

need in addition to specify the equations closing the system (e.g., by setting the last of the equations equal to zero). For a graphical representation of a similar system, see McNaughton and Wolff (1973, p. 344). But the simplest illustration of the logic of such a process is that given in figure 5,

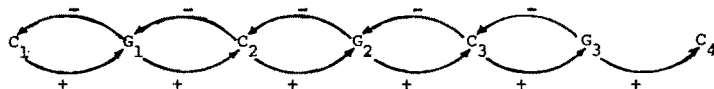


FIG. 5.—Simple representation of the logical structure of a dialectical process (C_j 's are social conditions; G_j 's are social groups).

which uses an arrow diagram to show the causal connections between the rise and fall of social groups G_j and the generation and destruction of environmental or social conditions C_j . From the individual point of view, "conditions" mean the structure of available alternatives as well as incentives and constraints. Group G_1 first changes its environmental conditions C_1 . This has a detrimental effect on its own reproduction at the same time as the changed environmental conditions are conducive to the growth and expansion of another group, G_2 , which in turn makes its own environment inhospitable; these altered conditions promote the growth of a third group, G_3 , etc. Such effects can also take place at a remove, for example, by G_1 being adversely affected by C_2 rather than by C_1 .

The best-known model in the social sciences for the situation in which social groups create the conditions which generate new groups while these very conditions undermine their own existence is Marx's historical materialism. Social classes successively produce conditions favorable to new classes but in the long run adverse to their own survival. The most pregnant summary of this point of view is found in *The Communist Manifesto* (Marx and Engels 1959), where Marx describes how the means of production and exchange on whose foundation the bourgeoisie built itself up were generated in feudal society; how the feudal organization of production became increasingly mismatched to the already developed productive forces and became fetters on further development which had to be "burst asunder." A similar movement takes place under capitalism: the bourgeoisie, by advancing industry, precipitates entire sections of the ruling classes into the proletariat and simultaneously produces increasing riches, increasing poverty, and an increasing proletariat. By concentration of industry the workers are also concentrated, become conscious of their own exploitation and potential, and organize as a class: "The weapons with which the bourgeoisie felled feudalism to the ground are now turned against the bourgeoisie itself. But not only has the bourgeoisie forged the weapons that bring death to itself; it has also called into existence the men who are to wield those weapons—the modern working class—the proletarians." The logic of the explanation, therefore, is like that of ecological

succession: not only are social conditions or environments selective of social groups, social groups also create new selective social conditions. Groups successively and simultaneously produce conditions for their own unmaking and for the generation of other groups. Their actions are simultaneously productive and counterproductive.¹¹

The outcome of Marx's model—conflict and revolution—is not planned or desired by any of the capitalists. But in their individual adaptations they unwittingly inhibit more effective adaptations for themselves as a class. They find themselves in the paradoxical situation of being forced by the social structure they have set up to act in ways which, though individually advantageous, prove harmful to all and hence bring about their own undoing as well as that of their social structure. The final collapse is due to new consciousness in the expanded proletariat—that is, new concepts with which to apprehend reality as well as appropriate the power of the bourgeoisie. Whereas change *within* the capitalist system is the unintended aggregate result of individual actions, change *of* the system is due to collective action based on a theory which is revolutionary in a threefold way: it is new, it gives a form and distribution of understanding which makes it possible to transcend the existing conditions or process structure, and hence it substitutes conscious historical transformation for unintentional change.

There are, of course, others who have employed the same basic paradigm for describing structural change. Whereas Marx envisages the "ecological climax" of human history as a self-reproducing classless society, Milovan Djilas (1957) describes another dialectical process in the rise of "the new class" after the revolution. The social origin of the new class lies in the proletariat, for whom it performs administrative functions in achieving industrialization. It is moved to power by the proletariat, but comes to monopolize this power and to exercise it over the proletariat. "In this case a new exploiting and governing class is born from the exploited classes" (pp. 38 ff.).¹²

Such processes can be called dialectical. It is interesting to note that at the aggregate level, the essential logic of description is the same whether it is yeast cells or capitalists that unwittingly produce their own decline and fall.

However, as Toulmin (1974) puts it, there is a "crucial distinction between the intentional (teleological) activities of human beings and the

¹¹ The reader will remember that the theory of demographic transition was criticized for containing no forcing principles and hence no inherent rationale for why the three stages would follow in sequence. Marx's theory for the development of industrial societies was intended to be a forcing model in this sense.

¹² The logic of Djilas's analysis is on many accounts close to that of the *Communist Manifesto*. One could also argue that the Chinese Cultural Revolution provided empirical support for Djilas's thesis.

mechanically self-correcting (teleonomic) processes of biology."¹⁸ In sociological analysis of change, we therefore have to ask what makes it rational for purposive actors, under the given constraints, to behave in ways which may prove their own undoing, or how short-run rationality may prove irrational in the long run.

In order to carry out a dialectical analysis we must therefore be able to show that the same item or conditions can be both functional and dysfunctional, in other words, that the functional response of the system to a specific task can lead at the same time to the dissolution of the system (cf. Merton 1957*b*; Elster 1971). The output structure may lead to the destruction of its process structure and the generation of another process structure. The distinction among the three levels of structure also makes it possible to give precise meaning to such Marxist concepts as "correspondence," "basis and superstructure," etc.

But not only social classes or groups succeeding each other can be described by a logical scheme like the one in figure 5. It can also represent how a group changes itself by modifying its environment, or how characteristics of group members affect interpersonal relations but in turn are affected by them, or how personality traits give rise to actions whose results modify these traits, etc.

One more point should be noted. The simultaneous destruction of one process structure and generation of another makes it necessary in concrete analyses to show how the destruction of one (or more) produces the constitutive elements of another as well as how the relations between them are established. Marx wrote: "The economic structure of capitalist society has grown out of the economic structure of feudal society. The dissolution of the latter set free the elements of the former" (1906, p. 715; cf. Balibar 1970, p. 279), and then went on to argue that they were recombined into a new economic order. Here again we see that the mediation of structural change at the macrolevel through processes at the microlevel becomes crucial. And in such analyses the opportunities men face and the problems they try to solve become a strategic point of theoretical attack.

Problem-generating Structures

In describing dialectical development, the Hegelian terms of antithesis, negation, and transcendence are often used, but the content of these terms is hazy. I shall therefore describe how some process structures or operators generate problems by the very operations they allow, leading to modifications of both operators and operands. In this way it is possible

¹⁸ The terms "teleonomic" and "teleological" were used in these senses by Monod (1971) but were introduced earlier.

also to attach some more definite content to the Hegelian notions which may be useful in sociological analysis.

An enlightening example is found in the development of numbers, which can be described by the following cursory history. In the beginning there were the natural numbers and the operations addition, subtraction, multiplication, division, and root extraction. Subtracting two from four created no problems, but the reverse was meaningless until negative numbers were defined and the elementary operations allowed for them. Hence the operation "subtraction" performed on the natural numbers created a problem, the solution to which was the expansion of the operands and the allowed range for the operator (e.g., clarifying the meaning of $[-4 - 2]$). Operating on the natural numbers thus led to their "negation" and the synthesis of the two sets of numbers into the integers comprising both. Dividing four by two caused no problem, but the inverse was problematical until fractions were defined so that the elementary operations could be used on them (e.g., dividing or multiplying fractions by each other, but not allowing division by zero). Hence the operation "division," applied to integers, led to the expansion of the number system and the incorporation of integers and fractions into the set of rational numbers. Taking the root of four is unproblematic, but taking the root of two causes a problem, since the result is neither an integer nor a fraction. Hence the operation of extracting roots of rational numbers led to irrational numbers, both of which are incorporated in real numbers. Taking the root of a negative real number created problems, since the outcome was undefined. So the imaginary numbers were defined and added to the set of real numbers to constitute the set of complex numbers. The genealogy of this development (which does not correspond exactly to the historical sequence) is summarized in figure 6. It highlights a problem-generating structure: the operations that are allowed generate problems whose solution often lies in the redefinition of the operands and in the modification of the operations so that they can be applied to the new set of elements. (This in a way corresponds to the

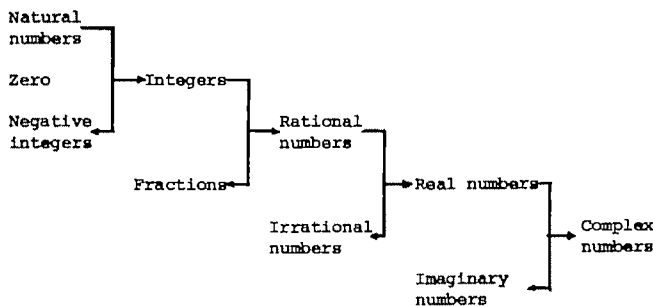


FIG. 6.—The logical structure of a problem-generating structure, illustrated by a number diagram.

Hegelian term *Aufheben*, with its celebrated double meaning.) In other words: they give rise to transformations.

Another illustration of problem-generating structures is Kuhn's description of scientific revolutions. In his view an established paradigm allows "normal science" to proceed by "solving puzzles," that is, by means of its conventional concepts and instruments. But a paradigm may also allow or indeed provide questions for which the paradigm either gives no answer or gives anticipations which turn out to be at variance with observations. Hence "research under a paradigm must be a particularly effective way of inducing paradigm change. That is what fundamental novelties of fact and theory do. Produced inadvertently under one set of rules, their assimilation requires the elaboration of another set. . . . Assimilating a new sort of fact demands more than additive adjustment of theory, and until that adjustment is completed—until the scientist has learned to see the world in a different way—the new fact is not quite a scientific fact at all" (1962, p. 52). Thus, incorporating an anomaly implies conceptual adjustment, and the result is a new paradigm which can account more successfully for the observed facts.

There is another interesting aspect to Kuhn's model. He argues that paradigms are resistant to change, a fact which guarantees that scientists will not be easily distracted by trifles or mishaps, "and that the anomalies that lead to paradigm change will penetrate existing knowledge to the core" (p. 62).¹⁴ He further argues that the emergence of a new paradigm leads to a battle over its acceptance and that the "competition between paradigms is not the sort of battle that can be resolved by proofs." So its acceptance is to a large extent a question of power and a conversion process which shifts professional allegiances over time.

Other social processes can be looked at in the same manner. For example, when men started to create organizations difficult legal problems arose, since most law applied only to persons. Large social entities such as tribes,

¹⁴ An example of an anomaly in present-day social science is found in the neoclassical concept of the marginal productivity of labor: "Although it substantially undercuts the usefulness of marginal productivity, economists, when asked to explain the existence of unemployment, will often argue that marginal productivity is a theory of employment rather than a theory of distribution. This occurs since unemployment cannot theoretically exist in a competitive world where every factor is paid its marginal product. When marginal productivity is used as an employment theory, wage rates are determined in some sociological manner, but factors are then hired until their marginal products equal this sociologically set wage or price. Factors are paid their marginal product, but wages or prices determine marginal products rather than the reverse. In this case, marginal productivity determines a group's income by affecting a group's unemployment. Despite the problems associated with this idea of marginal productivity, economists are extremely reluctant to abandon it, since they do not know how to replace it and since it is essential to much of the theoretical apparatus of economics. If it ever had to be abandoned, much of economics would have to be abandoned with it" (Thurow 1973, pp. 71-72).

the Roman church, and universities had of course existed for a long time, and the concept of them as entities was not new. But with the waning of the Middle Ages untraditional legal concepts began to emerge: "What *was* new was the idea that [these] entities had rights to engage in transactions on their own, so to speak—rights to determine on their own what they would own, buy, or sell" (Coleman 1974, p. 24). A conceptual innovation was made, encompassing both humans and organizations by treating them alike as juristic persons who could make or break relations, enter into binding agreements, and the like.

When analyzing an operator or process structure one must therefore ask not only what output structure it produces, but also what problems are generated by the process structure. If trade and exchange take place between two communities, that process does more than provide regularity for their transactions. If goods are not divisible, something is needed to make up for differences in value, and a commodity everyone will accept is needed if one is not to be restricted to bartering only with those who desire the good one can offer. A medium of exchange which is divisible can solve these problems. But it can lead to further problems if it is not easy to partition and add to, if it is perishable, if it is difficult to store, if it is of uneven quality, if it fluctuates widely in quantity, etc. In similar fashion, trade generates legal problems (Who is responsible for goods in transit? How are promises to be made into enforceable contracts? How is money to be guaranteed a uniform value over space and time?) and problems of standardization (the need for calendars making it possible to meet or deliver at a time agreed, for weights and measures making it possible to know what one gets, etc.). In these cases the problem is not just one of creating intellectual consistency but one of stabilizing expectations in sequential interdependence. At the microlevel the motives are the desires to reduce transaction costs and risks; the solutions have been to establish institutions in which more trust can be placed than in fallible men. But even if better solutions are selected over time and incorporated into a tradition or into the institutional order, they generate new problems requiring further modifications, as when the establishment of markets results in conditions which necessitate restriction of their scope. Since established solutions represent sunk costs, they can be difficult to change: switching to the metric standard is an enormous undertaking in the United States, and changing to a European voltage is infeasible because of the capital costs involved.

Note that an implicit standard for what constitutes an improvement or a solution is assumed for problem-generating structures: greater consistency, greater efficiency, greater equality, more benefits, reduced costs, and the like. But the source of the problems may also be a change of standard or of reference group (cf. Merton 1957a).

The main point is that in theories of structural change the key question to ask will often be what intellectual problems are created by a given order or social arrangement. Marx argued that at the societal level one has to focus on the problems generated by men going about making a living under different institutional arrangements, what strategies they pursue in solving them, and how this in turn leads to change of the institutional order.

It is important to note that such problem solving need not be dramatic. In a recent study of industrial administration, Stinchcombe sets out to destroy the notion that routine administration and innovation are radically distinct. In order to keep a steel plant running, administrators must search for major and minor innovations that will improve their control of routine production. The problem-generating structure in this case is the high technological interdependence in hot rolling, which means that a breakdown or interruption in one part of the mill can paralyze the entire production process. It is for this reason that "only by innovation can the routine problems of producing steel pipe be solved" (1974*b*, p. 30). The research strategy he follows is that introduced by Weber: to conceive of the degree of rationality as a variable and as a property of a system, rather than primarily as a property of individual psychology. The crucial point then becomes what structural and normative devices tend to isolate economic and administrative action from irrational influences and how effective they are in that respect. But the problem of rationality, as I have stressed above, is also systemic in the sense that different social arrangements generate different intellectual problems, are modified by their solution, and in turn generate new ones. The way activities are organized affects the frequency with which something is defined as problematic. In general, the more interdependent the components of a system, the more a disturbance in one part will create problems in another. Or expressed in a different terminology: the nature of the process structure—particularly its degree of coupling with others—affects the extent to which its output at the microlevel is found to create problems or the extent to which individual adaptations generate problems when aggregated. It is therefore informative to take a look at the polar cases of system interdependence: those that are loosely coupled and those that are overintegrated.

Loosely Coupled Systems

Above, I have mostly described simple processes—one process structure creating a specific output structure, two interacting systems, or a set of process structures supplanting each other. Needless to say, many process structures are going concerns simultaneously and can therefore interfere with each other's operation. Maurice Godelier writes: "Between a cause and an effect there always intervene all of the properties, known and un-

known, of one or more structures. It is this causality of the structures which gives an event all its dimensions, whether conscious or not, and explains all its effects, whether intentional or not" (1972, p. 99). But the repercussions and ramifications of an event will be determined by the extent to which a larger system is hierarchical or decomposable; that is, by the extent to which interactions are stronger within subsystems than among them, thereby permitting the subsystems to operate relatively unaffected by the details or inner workings of each other.

An important characteristic of hierarchical systems is that subsystems or subroutines can be altered without much effect on the others. The extent to which they do affect each other is described as their degree of coupling, which depends on the activity of the variables they share. Two systems are relatively independent when they have few variables in common or when the common variables are weak in impact relative to others influencing each system: "It is convenient to speak of such a situation as loose coupling and also to note that insofar as one system, *A*, is independent of another, *B*, we may speak of the persistence of the behavior of *A* in the face of the behavior of *B*. Speaking in these terms helps to direct attention to the fact that the stability of systems may be due not only to immediate compensation for imposed input, but also sometimes to lack of communication" (Glassman 1973, p. 84).

This has several implications which are important for the analysis of structural change. First, since a perturbation in one of the component parts does not reverberate throughout the entire system, each part can be modified or transformed without changing the others and can remain locally stable as the others undergo change. That is, structural change may occur endogenously in different subsystems without causing great repercussions for the others. Second, since they are relatively unaffected by their interrelations with others, they can reproduce themselves and still enter into new combinations with each other. Being loosely coupled, they can be grouped as elements into different wholes, but also perturbed or changed without affecting the whole of which they are a part. This is illustrated by a statement in an old anthropological text:

Under this simple form . . . the inhabitants of the country have lived from time immemorial. The boundaries of the villages have but seldom altered; and though the villages themselves have been sometimes injured, and even desolated by war, famine and disease, the same name, the same limits, the same interests, and even the same families, have continued for ages. The inhabitants give themselves no trouble about the breaking up and division of kingdoms; while the village remains entire, they care not to what power it is transferred, or to what sovereign it devolves; its internal economy remains unchanged. [Raffels 1817, p. 285; quoted by Marx 1906]

In other words, loosely coupled systems may combine reproduction of the

parts with transformation of the whole or may combine transformation of the parts with reproduction of the whole. For loosely coupled systems it may also be simple to show how the destruction of one overall process structure produces the constitutive elements of another.

Change in the component parts of a loosely coupled system corresponds to adaptation in the biological world. An important aspect of such changes is precisely that "some adaptations become safely insulated while others take place" (Glassman 1973, p. 86) or that "partial successes can be conserved and accumulated" (Ashby 1966, p. 152). It is in this fashion that one describes how the teleonomic systems in nature become better adapted over time through selection. One can argue that learning by doing is for teleologic systems what selection is for teleonomic ones. (For models of social selection, see Alchian [1950]; Winter [1964-65]; Farrell [1970].) However, there are also important differences between learning in general and selection, in that learning can take place by dramatic conceptual advances—as illustrated by the scientific revolutions mentioned above—and rational actors who have reached a local optimum can "descend" from it in order to reach a higher one, whereas this is highly unlikely in natural selection.

For loosely coupled systems, then, we can often compartmentalize our analysis of structural change. However, it must be noted that the independence of the parts can be temporary, so that the time interval over which the coupling is evaluated is critical. For example, Marx argued that in the short run "relations of production" (or, roughly speaking, organization) are loosely coupled to the "forces of production" (or, roughly speaking, technology). But "over the long run the influence of one subsystem on another may be stronger than it appears within any given short interval" (Glassman 1973, p. 85), for example, if the effects accumulate. This was precisely what Marx argued with respect to the interdynamics of technology and social organization: as a relatively fixed social structure becomes increasingly outmoded relative to its developing technology, revolutionary potential builds up. Expressed differently, the different rates of change create "structural incompatibility," macroproblems mediated through individuals which have to be resolved. Such models as this "are intrinsically dynamic: the causes are conceptualized in terms of the changes they bring about" (Coleman 1964, p. 106).

Overintegration and Catastrophe

Changes within systems may increase their degree of coupling over time. By extended reproduction, transitions, or transformations of the parts, the whole may become more integrated. Integration can also come about by learning, for example, economizing can be achieved by letting the same item serve multiple functions (cf. Merton 1957*b*).

Structural Change in Social Processes

Increased integration can have two very different effects. First, as a social system becomes more tightly coupled it also becomes more resistant to change. An increasing number of its elements come to have multiple functions, making it harder to change one subsystem without changing the others. Indeed, a society may approach the type of total integration described by the classical functionalists. It is not unlike the connectivity and resistance to change of a scientific paradigm. This means that the progressive binding of subsystems may lead to "overintegration" in the sense that a desirable change in one subsystem may require too many concurrent changes in others to be practicable.¹⁵

Second, the increasing complexity, which from one point of view makes the system more stable and resistant, from another point of view makes it more prone to catastrophe, in much the same way as anomalies that lead to change in paradigms penetrate existing knowledge to the core. A very interesting anthropological example of a catastrophe is given by Lauriston Sharp in his "Steel Axes for Stone-Age Australians" (1952). Sharp shows how the traditional stone axes were very tightly coupled with almost every aspect of the life of the Yir Yoront group: the production system, by the skills required to produce them and by their being the most important tool for extracting food; the relationships with neighboring groups, because of their role in intertribal trade; the authority system, because women and the young had to borrow stone axes from the men; and the culture, through language, sentiments, values, ceremonies, and symbols. When the missionaries started to distribute steel axes indiscriminately and in large numbers, they were easy to adopt because they provided no major technological difficulty. Needless to say, their introduction had profound effects, affecting everything from trade and authority to psychological stress and identity. Something of the same sort, one can imagine, would happen if oil were taken out of the economies of industrialized societies.

Increasing compatibility, multifunctionality, and connectivity within a system may therefore lead not only to greater stability but also to a greater potential for catastrophic change. Therefore, when analyzing structural change one must not only look at "structural incompatibility" but also

¹⁵ Consideration of the flexibility and rigidity of social systems has a long tradition in sociological analysis. Here the concept of "overintegration" has been borrowed from the biologist Richard Levins (1968), who writes: "Under constant macro-conditions, there will be a tendency toward increased integration among functions that form a convex fitness set. This can come about through selection for cost reduction by making the same variable do multiple duty. Further, the fact that a given character is more or less constant makes it available as a signal to initiate other processes. Thus what is there will become necessary in other ways. The possibility arises that after a very long time in the same environment an organism can become over-integrated in the sense that a given change which may be advantageous in one respect may require too many concomitant changes to be feasible" (p. 108).

explore whether the system is composed initially of integrated and mutually adjusted process structures which contribute to each other's reproduction—an interdependence which heightens the risk of extensive ruptures or transformation of the system as a whole.

CONCLUSION

In this paper I have argued that when analyzing structural change it is useful to distinguish among three different levels of structure—output structure, parameter structure, and process structure—and that doing so enables us to identify four basic types of change processes—simple and extended reproduction, transition, and transformation. Some important special cases, such as dialectical change, overintegration, and catastrophe, can also be identified. This basic framework can therefore describe constancy as well as change and can encompass endogenous sources of change.

Needless to say, in concrete analyses one will often be confronted with nested processes, so that one set of structures is embedded within another. It may then be tempting to talk of “deep structure,” but such terms, when popularized, often add more to jargon than to insight. Hence, even in these more complex cases, it is probably best to stay with the basic distinctions made above.

The need for a combination of micro- and macrolevel analysis has also been emphasized. Whether we study constancy or change, we must always ask how they result from human motivations, even when the actual outcomes deviate from the intended. Change in or of process structure and output structure can come about intentionally or as unintended consequences of purposive action, by aggregation of individual decisions or by collective decisions, by cumulation of benefits or by cumulation of costs. Hence factors which tend to change or stabilize the output, the parameters, or the processes at the macrolevel are of particular interest. Although this topic cannot be pursued here, the examples used above point in two directions in which search may be useful, one of them centering on learning and the other on contrariety.

We have seen that death rates may fall when better practices are selected over time, or that a steel axe is simple to substitute for one of stone. When the same task has to be performed repeatedly, and hence the same problems have to be solved recurrently, a selection of better solutions may take place over time. Such “learning by doing” will therefore be a central concern when one wants to understand factors which tend to stabilize or change macroparameters. But solutions may have unforeseen side effects, as for the Stone Age Australians, or solutions to different problems may be incompatible, so that actors get caught in paradoxical situations, like Bhaduri's semifeudal landlords. Moreover, actors may be at cross-purposes, or un-

wittingly pursue their individual interests in ways which prove harmful to all, like Marx's capitalists. Such notions are associated with the term "contradiction," but there are other connotations as well. To be useful in concrete analyses, more precise content must be given to the different meaning clusters associated with the terms "learning by doing" and "contradiction." To identify such meaning clusters is a first task on the agenda when one wants to analyze factors which tend to stabilize or change the structure and results of processes at the macrolevel, and it starts at the microlevel.

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History and Social Change

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In *Social Change and History*, Robert Nisbet challenged the new evolutionary theory on epistemological, methodological, and substantive grounds. This paper examines each of Nisbet's charges and concludes that his oft-praised volume provides a misleading view of the new evolutionism and a less than satisfactory guide to the study of social change. Above all, Nisbet's charge that the new evolutionism is based on metaphor and analogy ignores entirely the increasingly substantial empirical grounding of theory in archaeological, historical, and sociological research. Also, his effort to force scholars to choose between the nomothetic concerns of evolutionists and the idiographic concerns of historians and some historical sociologists invites conflict where cooperation is needed.

When Robert Nisbet's volume (1969), *Social Change and History*, first appeared, many reviewers expressed their admiration in strong terms. John H. Plumb (1969), writing in the *New York Times*, called his arguments "convincing" and his conclusions "wholly admirable." Dennis Wrong (1969) wrote in *Commentary* that "Nisbet brilliantly exposes the essential fallacy of all developmental theories." Norman Jacobs (1970), in the *American Journal of Sociology*, called the book a necessary attack on "one of the most sacred cows in sociology," and Julius Gould (1970), with typical British restraint, called it a "monumental" work.

Although some reviewers did not share this high opinion of Nisbet's book, the overall response has been sufficiently positive to justify a more detailed examination and critique than is possible within the framework of the usual book review. Such a study is especially desirable because *Social Change and History* offers what appears to be a devastating attack on recent efforts to revive evolutionary theory. Moreover, since Nisbet pulls together in one place most of the basic charges leveled at the new evolutionary theory, a critical examination of his book provides a good opportunity for examining the merits, or lack of merits, of this new development in theory.

BRIEF SUMMARY OF NISBET'S ARGUMENT

Nisbet's basic thesis in *Social Change and History* is that evolutionary theory, as applied to social and cultural phenomena, rests inevitably on a foundation of metaphor and analogy—the metaphor of growth and the

analogy between the life cycle of organisms and the development of human societies and cultures. Nisbet (1969, p. 3) makes these points vigorously in the opening pages when he writes that no one has "ever seen—actually, empirically seen, as we see these things in the world of plants and animals—growth and development in civilizations and societies and cultures, with all that is clearly implied by these words: change proceeding gradually, cumulatively, and irreversibly, through a kind of unfolding of internal potentiality, the whole moving toward some end that is presumably contained in the process from the start." What we see, instead, are "migrations and wars, dynasties toppled, governments overthrown . . . human beings born, mating, child-rearing, working, worshipping, playing. . . ." But in cultures and societies we do not see "genesis," "growth," "development." These are metaphors, and metaphoric knowledge, he tells us (1969, p. 4), "is the very opposite of the type of knowledge that comes to us through mere additive experience, through patient dissections of observations, through elaboration, deductive or inductive, of meanings already contained in a proposition, through either analysis or synthesis as these terms are best known. Metaphor is none of these. Metaphor is our means of effecting instantaneous fusion of two separated realms of experience into one illuminating, iconic, encapsulating image." He goes on to say (1969, p. 6) that, although metaphors are indispensable in language, poetry, philosophy, "and even science," they are "dangerous." "To look at the whole universe and say it is like a machine or organism is one thing: forgivable in proper time and place. But to seek to build rigorous propositions of scientific analysis upon either metaphor, mistaking attributes of analogy for attributes of reality, can be, as the history of science teaches us, profoundly limiting and distorting. . . . Nowhere is this more evident than in the study of social change."

In the first two-thirds of the book, Nisbet traces the history of the metaphor of growth from ancient Greece to 18th- and 19th-century Europe and America. When he comes to the latter period and discusses the theory of social evolution, he sets forth in detail what he believes to be six basic premises of this theory: (1) change is natural; (2) change is directional; (3) change is immanent; (4) change is continuous, that is, cumulative; (5) change is necessary; and (6) change proceeds from uniform causes (1969, pp. 166–82).

One other feature of evolutionism is sufficiently important, in Nisbet's judgment, to merit special attention, namely, the Comparative Method.¹ Although that method is often thought to be a product of the new "scientific anthropology" of the 19th century, Nisbet says its roots are as old as Greece and Rome. In essence, the Comparative Method involves

¹ My capitalization of "Comparative Method" follows Nisbet.

a comparison of societies "within a *presumed* order of growth and development" (1969, pp. 192-93, italics added). In other words, it forces data into a preconceived mold, leading to a basic circularity in reasoning.

All this might be regarded as an exercise for antiquarians were it not for the recent disturbing revival of interest in evolutionary theory. For, despite the best efforts of the new evolutionists to differentiate between 19th- and 20th-century work, Nisbet says that he cannot find any substantive difference (1969, p. 227). Thus the book becomes a plea to social scientists concerned with social change to avoid the siren song of evolutionism and to keep their feet firmly planted on the solid rock of history. The great virtue of history "lies in its concern with the concrete and the particular, and its strict observance of the limits of time" (1969, p. 268).

To help in this endeavor and to guide our thinking as we study change, Nisbet counters the six so-called basic premises listed above with six anti-evolutionary principles:

1. "Change is . . . not 'natural,' not normal, much less ubiquitous and constant. Fixity is" (1969, p. 270).

2. Change lacks directionality. ". . . [P]hilosophers of history and social evolutionists to the contrary, long-run directionality tends to be in the beholder's eye, not in the materials themselves" (1969, p. 284).

3. Change—especially the more significant instances of it—is primarily the result of the intrusion of external forces into the life of a society (1969, pp. 275-82).

4. Change is not cumulative. "There is no historical evidence that macro-changes in time are the cumulative results of small-scale, linear micro-changes" (1969, p. 288).

5. ". . . [T]here is no necessity in any pattern of change if our attention is fixed, not upon the wholes and universals of social evolutionary theoretical interest, but instead upon the social behavior of human beings in time and place" (1969, p. 294).

6. Although uniform processes exist wherever human beings interact with one another, uniformitarian concepts do not "aid appreciably in the study of social change." "Constants are ordinarily of little help in accounting for variables" (1969, pp. 296-98).

THE PLACE OF METAPHOR IN THE NEW EVOLUTIONARY THEORY

If Nisbet is correct, the current effort to build a new evolutionary theory is certainly, at best, misguided. But is he correct? Or has he failed to understand the new evolutionism and, perhaps also, the real world that it seeks to describe and explain? To answer these questions, let us examine his assertions one by one.

The first, and most serious, charge that Nisbet levels at evolutionism,

both old and new; is that it rests on the foundation of the metaphor of growth and the analogy to the life cycle of organisms. This may have been true for some of the early evolutionists, but it is certainly not true today. I think it is no accident that in his more than 300 pages of text, Nisbet makes almost no reference to recent work in archaeology, in which evolutionism is now the dominant paradigm.² Ever since the revolution brought about by the introduction of carbon-dating techniques, the onetime problem of synchronizing findings from scattered research sites has vanished, and it is now possible to relate chronologically social and cultural developments in East Asia with those in Africa or South America. Though problems of detail remain, the basic outlines of human history from the Lower Paleolithic through the Bronze Age are now clear. They can be described only in developmental terms: evidence of increasing numbers in the human population, evidence of human residence in more diverse habitats, evidence of increasingly complex technology, and evidence of increasing production of durable goods and accumulating capital (Renfrew 1973; Clark and Piggott 1965; Ucko, Tringham, and Dimbleby 1972; Braidwood and Willey 1962; Braidwood 1960; Adams 1960). The biological basis of the analogy to the life cycle of organisms could be proved false tomorrow, and these findings of the new archaeology would not be affected in the slightest.

Historians pick up the story where archaeologists leave off, and their findings both reinforce and extend the picture provided by those of the archaeologists: continuing growth in population; continuing advances in technology, in production, and in capital accumulation; and, in addition, social systems that generally become more complex, more differentiated, more urban, and more powerful as time goes on.

There are, of course, exceptions, and these have been a basic weapon in the arsenal of critics of evolutionism from Boas to Nisbet. Rome declined, as did Mayan civilization, and many groups remained hunters and gatherers down to the modern era. How can evolutionists possibly explain such deviations from the hypothesized pattern of growth and development?

The answer to this question is actually quite simple. The key is pro-

² One anonymous reviewer criticized this paper for failing to make sufficiently clear to sociologists how widely evolutionary theory has come to be accepted in sister disciplines. As the reviewer put it, "The author does not adequately convey to the sociological community the fact that for the past 10 years evolutionism has constituted the dominant paradigm in American anthropology. While this statement is not [entirely] applicable to social and cultural anthropology, it is certainly true of archaeology. In social and cultural anthropology the paradigms of cultural materialism, cultural ecology, and neo-Marxist variations occupy a central position and are related to and compatible with, if not subsumed by, the evolutionists' paradigm in anthropological archaeology. The degree of Nisbet's ignorance concerning the institutionalized strength of neo-evolutionist viewpoints is therefore far more appalling than the author indicates."

vided by Marshall Sahlins's (1960) distinction between specific and general evolution. Specific evolution pertains to some one society or group of related societies, general evolution to the totality of human societies. Specific evolution is concerned with case studies, general evolution with frequency distributions. One does not disprove a proposition about frequency distributions at the level of general evolution by citing a single case study, or even a number of case studies, concerning specific evolution. One disproves a proposition about general evolution only by showing that the properties of the frequency distribution fail to conform to the hypothesized pattern. Thus, to disprove the theory that there is long-run directionality in such phenomena as population size, level of technology, or complexity of social systems, critics must show that at the *global level* there is no trend with respect to central tendencies or the upper limit of the frequency distributions. This, of course, cannot be done because there are now mountains of evidence supporting these hypotheses. The most that can be done is to show that the lower limits of the frequency distributions have not changed much, if at all, since the end of the Paleolithic, but even this possibility will vanish presently, since true hunters and gatherers are likely to follow the dodo very shortly.

Let us call a halt to the practice of citing deviant cases as disproof of propositions about a population of societies. We would not say that the existence of 50 black millionaires disproves the thesis that the black population as a whole is less wealthy than the white. Deviant cases can be interesting and the analysis of them can often lead to the enrichment of theory, but the very notion that a case is deviant implies the existence of some more general pattern.

Nisbet's epistemology as it pertains to the concept of metaphor also leaves something to be desired. He claims that no one has ever seen the growth and development or death of a society, but that we see, instead, migrations, wars, dynasties toppled, and the like. This distinction, if indeed it exists, is much too subtle for me. Although I was a participant in World War II, I have never seen a war in its totality, just as I have never seen in its totality American society's economic growth during my lifetime.

Nisbet also makes a distinction between the empirical basis of the theory of biological evolution and that of the theory of sociocultural evolution, saying that no one has ever seen the growth and development of societies "as we see these things in the world of plants and animals." This sounds correct until one stops to realize that the theory of biological evolution does not rest on observations of individual plants and animals alone. On the contrary, it depends on inferences drawn from many such observations and applied to vast populations of thousands of species extending over aeons. Sir Julian Huxley, George Simpson, and Ernst Mayr have not "actually, empirically seen" these things in their totality any more than

social evolutionists have seen the complex trends and patterns they describe.

Finally, it might be well to make explicit a point that is implicit in what I have already said. Proponents of the new synthetic theory of evolution in biology now insist that populations of organisms, rather than the individual organism, must be the basic unit of analysis in evolutionary theory. As George Gaylord Simpson (1958, p. 14) put it once, "The thing that is actually evolving is a population." The same can be said of socio-cultural evolution.

IS FIXITY MORE NORMAL THAN CHANGE?

Turning now to the six propositions that Nisbet attributes to evolutionists and the six alternatives he offers in their stead, one is struck immediately by his apparent fondness for dichotomies. Once this is recognized, one cannot help asking whether they are not possibly *false* dichotomies—dichotomies that invite us to make "either-or" choices when "both-and" choices might be far wiser.

Is change normal and natural, or is fixity? Ironically, we can answer this question only by adopting the kind of uniformitarianism that Nisbet inveighs against elsewhere. Evolutionists would much prefer to answer that change has been much, much more evident at certain times in human history than in others. And this has not been a simple random matter. During the Lower Paleolithic, so far as archaeologists can judge today, change was incredibly slow. One writer (Cole 1965, p. 123) speaks of the astonishing slowness of cultural development" in this era, another (Hawkes 1965, p. 172) of the "almost unimaginable slowness of change." No one would use such phrases to describe human life today. Available evidence indicates that the rate of change for humanity as a whole has been steadily rising at an accelerating rate. Thus, Nisbet's assertion that fixity is more normal than change fits the facts rather well for the Lower and Middle Paleolithic. On the other hand, the assertion that change is normal and natural is probably closer to the truth for more recent times. But change and continuity alike have been elements in the fabric of human life from time immemorial, and it does not serve the cause of social theory to raise questions which, by their very formulation, obscure this fact.

IS LONG-RUN DIRECTIONALITY AN ILLUSION?

Probably none of Nisbet's assertions is more astonishing to the student of world history than his statement that "long-run directionality tends to be in the beholder's eye, not in the materials themselves." To support this contention, he quotes H. A. L. Fisher, a prominent student of European history of the last generation, as saying: "One intellectual excitement has

. . . been denied me. Men wiser and more learned than I have discovered in history a plot, a rhythm, a predetermined pattern. These harmonies are concealed from me. I can see only one emergency following upon another, as wave follows upon wave, only one great fact with respect to which, since it is unique, there can be no generalization . . . the play of the contingent and the unforeseen" (1969, p. 284).

This statement has, of course, a substantial element of truth in it. No serious scholar, evolutionist or nonevolutionist, would deny that much of human history is made up of unpatterned events that arise in response to unpredictable contingencies. But evolutionists go a step beyond and ask whether *all* of history is merely sound and fury—or mere "noise," as physicists describe it. Is not signal also present, involving useful information? I am reminded here of a statement Sir Fred Hoyle (1971, pp. 119–20), the noted British astronomer, once made: "The protagonists of studies in the humanities fail to appreciate the extent to which their problems are of the signal-to-noise kind. . . . Instead of separating the noise—throwing it away as the physicists do—they spend their energies chasing every detail of the darned stuff. Students of history do this with ferocious concentration, spending year after year studying their 'period' as they call it."

As Hoyle suggests, what we find in the study of history depends to a large degree on what we look for. This is not to say, however, as Nisbet does, that what we find is only "in the beholder's eye." Instead, it is to say that if you do not look for something you are very likely not to find it even when it is present, and this certainly seems to be true so far as historical trends are concerned.

Even if one limits one's study to European history, the presence of long-term trends is clearly evident—trends that have endured for 500 years or more: growth in numbers, growth especially of urban populations, increasing division of labor, increasing monetization of the economy, growth of trade and commerce, shift in employment from primary to secondary and tertiary industries, increased production of goods and services, growth in the power of the state, and increased bureaucratization, to name but a few of the more obvious. Perhaps it should be added that these trends are by no means trivial matters. On the contrary, they constitute the foundation of what can only be described as a revolution in the conditions of human life. By comparison, the unpatterned events on which earlier historians so often focused—the outcomes of specific battles, the lives of individual leaders—pale into insignificance, except, of course, for the individuals concerned and for the scholars who built their careers upon them.

The trends that can be observed in European history are, for the most part, components of larger ones in world history. All of those listed above and many more can be observed in the record provided today by archae-

ology and history. Speaking for myself, I am inclined to believe that underlying all, or most, of these trends is a single master trend that explains the others. This is the growth in the store of information available to humans—especially information relevant to the manipulation of the material world, that is, technology.

IS CHANGE IMMANENT OR IS IT DUE TO EXTERNAL FORCES?

Once again Nisbet confronts us with a false dichotomy. We are asked to choose between the belief that change is immanent and the belief that it is due to external forces. To make our decision easier, he presents this particular choice instead of one between internal and external forces: using immanence as the first alternative enables him to freight the belief he rejects with the burden of teleological determinism.

To clarify the meaning of immanence, Nisbet quotes Leibniz as saying, "I mean that each created being is pregnant with its future state, and that it naturally follows a certain course if nothing hinders it" (1969, pp. 170-71). He also cites Marx's belief that "in capitalism there is a natural course of development, one that can be decelerated temporarily by alien forces or accelerated by, for example, a revolutionary vanguard of the proletariat, but that cannot, in the long run, be abrogated by human decision" (1969, p. 172).

Nisbet's treatment of the first alternative seems to me a very mixed bag. It contains some ideas that are almost common sense; these are tied to others that are of very dubious merit. For example, it seems almost commonplace to suggest that the present state of a social system will have consequences for its future state. In fact, this relationship is so strong in most cases that one of the major methodological problems encountered recently in quantitative studies of trends and their causes is the problem of autocorrelation (i.e., the very strong correlations observed between measures of a variable at two or more points in time).

To say that the characteristics of a variable, or of a system, at one time have consequences for its characteristics at some later time is not, however, to deny the influence of external forces. It is essentially a case of "both-and" rather than "either-or." Moreover, I think it is clear from the statement of Leibniz that Nisbet quotes that Leibniz understood this even if Nisbet does not. Why else would he have worded his statement, ". . . it naturally follows a certain course *if nothing hinders it*" (italics added)? This surely indicates that he entertained the possibility that outside forces could deflect the development of an entity from the course indicated by its inner characteristics.

This view of things seems even more justified today in view of our new understanding of genetics. *Homo sapiens* is a species with a very distinct-

tive heritage that reflects its primate and mammalian origins. Above all, this heritage emphasizes the learning and sharing of information as a major adaptive mechanism. In man, these tendencies become intensified to the point where the learning and sharing of information becomes the chief adaptive mechanism. It seems to me to be more than an act of naive credulity to believe, therefore, that this immanent element in human life has played, and will continue to play, an important role in shaping the course of events.

It is, of course, dangerous to be deterministic about the future, and a number of evolutionists, including Marx, have been guilty of this. But it is equally dangerous to ignore the linkages between past, present, and future. In this connection, I much prefer the view of René Dubos (1968, p. 270), the noted microbiologist, who said, "The past is not dead history; it is the living material out of which man makes himself and builds the future." But the basic point that modern neoevolutionists would make is that we do not have to choose between pure immanence and pure environmentalism. There is a middle ground.

IS CHANGE REALLY NONCUMULATIVE?

Nisbet tells us that "there is no historical evidence that macro-changes in time are the cumulative results of small-scale, linear micro-changes." In reading this, one wonders whether, just possibly, he is putting us on. A thousand examples that contradict this statement come so quickly to mind: the growth of London or New York, the growth of General Motors or any other large corporation, the growth of the division of labor, the accumulation of capital, and the growth of population, to name but a few.

To draw attention to instances of incremental growth of this kind is not to deny the existence of more sudden and drastic changes. Some of the latter can be easily explained by the so-called threshold effect, which has an honored place in evolutionary theory: the slow and patient accumulation of many new bits of information may have no significant effect until some final bit is obtained, opening the way to a major breakthrough—as in the harnessing of atomic energy.

Beyond this, there are other instances of sudden and drastic changes that are not so easily explained. Sometimes we lack the relevant data. Sometimes we look for the causes in the wrong places. Sometimes the causes lie wholly, or partially, outside the scope of variables in our theory (e.g., the abrupt decline in European population in the 14th century that resulted from the Black Plague). But it is worth noting here that many of the allegedly noncumulative patterns of change that so fascinate anti-evolutionists have been either minor short-run deviations in long-term

trends in societal development or relatively trivial events. It is probably no coincidence that the one example of a noncumulative event that Nisbet provides in his critique of the evolutionist position on cumulation is the New York Giants' victory in the 1951 pennant race. Evolutionary theory was never intended to explain happenings of this kind.

IS CHANGE DEVOID OF NECESSITY?

Probably the least objectionable of Nisbet's proposals concerning the nature of change is his assertion that "... [T]here is no necessity in any pattern of change if our attention is fixed, not upon the wholes and universals of social evolutionary theoretical interest, but instead upon the social behavior of human beings in time and place." One might paraphrase this by saying that if you study problems other than those with which evolutionary theory was designed to cope, the principles of evolutionary theory will not necessarily apply. To which most evolutionists would probably respond, Amen.

In controversies over theory, we seem easily to forget that every worthwhile theory is an attempt to provide answers to certain specific questions. No theory attempts to explain everything. Evolutionary theory is designed to provide answers to questions about fundamental trends in history; it was never intended to provide explanations of the actions of all the individuals who, collectively, create the trend.

The situation here is not unlike that encountered in physics when one heats water in a sealed container. A physicist can predict with great accuracy the effect of increased heat on the level of pressure in the container, but he cannot predict at all the behavior of individual molecules in the container. One aspect of the problem has a completely deterministic outcome, the other a completely unpredictable one. Many situations in human life are similar. As Durkheim pointed out almost a century ago, we can predict national suicide rates with great accuracy even if we cannot predict which individuals will commit suicide. With respect to patterns of growth and development, we can usually predict the behavior of aggregates with far greater accuracy than that of individual units.

Actually, most contemporary evolutionists would be even more cautious than Nisbet in one respect. Most would not claim that there is necessity in most patterns of change—even when attention is fixed on "the wholes and universals of social evolutionary theoretical interest." Most of us would probably be content to say that there are simply varying degrees of probability. One of the important differences between the 19th- and 20th-century forms of evolutionism is the shift from deterministic to probabilistic formulations (Lenski and Lenski 1974, pp. 108–11).

IS UNIFORMITARIANISM PASSÉ?

Here, as elsewhere, Nisbet seems to encourage simple answers to complex questions. How we answer the question of whether uniformitarianism is a valid postulate depends entirely on the meaning we attribute to the term. Nisbet defines it at one point as "the uniformity of *fundamental causes of the change* involved in evolution" (1969, p. 182). He then equates this definition with the meaning given the term in the 18th and 19th centuries, a time during which uniformitarianism derived its meaning, in large measure, by contrast with an alternative theory of change, "catastrophism." Catastrophists saw the world as we know it today as the product of a number of catastrophic events, such as the Flood associated with Noah, which occurred once and were never repeated. Uniformitarians, in contrast, saw the contemporary world as the product of the same basic set of forces (e.g., natural selection) working slowly over vast periods of time.

Citing as his authority a single geologist, Nisbet (1969, p. 297) tells us that uniformitarianism is now regarded as passé in the natural sciences, and then proceeds to argue that the same should be true in the social sciences (using the analogical practice which he finds objectionable when evolutionists employ it). But if uniformitarianism is dead in the natural sciences, it is not catastrophism, or anything remotely resembling it, that has replaced it. Nisbet is rather vague on just what it is that has replaced uniformitarianism, mentioning briefly the phenomenon of mutation, which seems to suggest a more scientific version of catastrophism. Yet, if one stops to reflect, one quickly realizes that the process of mutation is not unlike the process of heating water in a sealed container: the individual cases may be quite unpredictable, quite irregular, but in the aggregate they often display striking regularities (e.g., in the frequency of mutations in specific species at specific sites). Furthermore, while mutations have come to be recognized as an important part of the process of organic evolution, they are not the critical element. To be of lasting importance, they must pass the test of natural selection, and this requirement increases the level of predictability even more.

If uniformitarianism is passé, it is not because of any swing back to catastrophism or anything resembling it. Instead, uniformitarianism has yielded to a perspective that might perhaps be labeled "emergentism." Emergentism shares with uniformitarianism the belief that uniform processes operate over vast expanses of time, some for the entire history of our planet and even longer. But the working out of these processes often leads to the emergence of new ones, which sometimes take precedence over earlier ones.

One of the best examples of this phenomenon is provided by the information-building process that is so fundamental to the whole of evolution, both

organic and cultural. In plants and in the simplest animals, information building is purely a genetic process, linked, of course, with natural selection. In certain animals (e.g., earthworms), however, the genetic process of information building is supplemented by individual learning. But it is important to note that learning does not replace the genetic process, it simply supplements it. In still more advanced animals (e.g., wolves), another mechanism of information building is added, signaling, which makes it possible for one individual to communicate learned information to another. Finally, at some point in hominid history, our own ancestors learned to communicate by means of symbols. But once again the newly emergent form of information building did not replace the earlier forms; it only supplemented them.

Looking back at this process, we see both continuity and innovation. It makes little sense to ask whether or not the label uniformitarianism applies. It does and it does not, both at the same time. The cause of scholarship is better served if we avoid ambiguous labels of this kind that were developed 200 years ago when the state of our knowledge of evolution was far less developed. It makes much more sense to say instead that the principles governing the evolutionary process are, themselves, subject to evolutionary change (Rensch 1960, pp. 112-13).

WHAT SHALL WE SAY OF THE COMPARATIVE METHOD?

Nisbet's challenge to evolutionism has not been limited to theory; he has extended it to methods as well. In fact, an entire chapter of *Social Change and History* is devoted to a critique of the Comparative Method.

The first question that should be raised by way of response to Nisbet is whether, in fact, the Comparative Method, as he defines it, is still the methodological foundation for evolutionary theory. My own answer to this would have to be an emphatic "No!" Although that method is still one of the tools employed by evolutionists, it is no longer the chief tool. The chief tools are those of archaeology, history, and sociology, which provide data on social and cultural systems identified as to time and place. We no longer depend (if, indeed, 19th-century evolutionists depended) on inferences from the study of modern hunters and gatherers to know that prior to the past 10,000 years, hunting and gathering, supplemented to some extent by fishing, were the sole means of human subsistence, and that horticulture, agriculture, herding, and industrialism were unknown. We know these things on the basis of thousands of carefully dated archaeological excavations that have provided a remarkably consistent and coherent picture of the Paleolithic world. These studies tell us much about the size and permanence of settlements, the kinds of goods produced, the relative material equality of the inhabitants, and similar matters. Admittedly, we do not

know much directly about the nonmaterial aspects of Paleolithic life—patterns of government, marriage customs, religious beliefs. But even in these areas, some beginnings of direct evidence are being developed. The important point is that today, thanks to the revolutionary advances in archaeology, our knowledge of the prehistoric past rests on much more than inferences drawn from observations of modern primitives, contrary to what Nisbet would have us believe.

The growing volume of carefully dated information from archaeology and history has not eliminated the use of ethnographic data. For one thing, as Grahame Clark, a leading British archaeologist, explained some years ago (1957, pp. 172–73), “. . . the great value of ethnography to the prehistorian is that it will often suggest to him what to look for. . . . By constant reference to the culture of living or recently living societies, the prehistorian should be able to enrich and fortify his interpretations of the past, as well as bring into the open problems calling for further research.” In a number of instances contemporary hunters and gatherers themselves have actually provided explanations for previously unexplained archaeological findings (Pfeiffer 1969, pp. 322–24).

Beyond this, studies of modern preliterate peoples provide invaluable opportunities to test hypotheses about the effects of variations in subsistence technology on other aspects of life. When we find, for example, that technological differences among contemporary preliterates are linked with predictable differences in community size, settlement patterns, extent of division of labor, size of political systems, and the like, and when these differences make sense theoretically and are consistent with findings from archaeology and history, evolutionists are inclined to view such evidence as relevant, even if it is not definitive. It is like one more piece of a puzzle that has fallen into place. In short, although we are not justified in saying that we can see in the Bushmen of the Kalahari or the aborigines of Australia how the ancestors of modern Europeans and Asians once lived, neither are the critics of evolutionism justified in writing off information about such peoples as irrelevant and immaterial to the question of societal development.

IS THE NEW EVOLUTIONISM REALLY NOT NEW?

The last of Nisbet's assertions that needs to be answered is the statement that there is nothing really new about the so-called new evolutionism (1969, pp. 226–27). There are at least four important differences between the older and the newer evolutionism that deserve comment, not to mention others of lesser importance.

To begin with, as we have just noted, the new evolutionism rests on a much firmer foundation of archaeological and historical fact than the older

evolutionism ever did. Moreover, this foundation is supplemented by Murdock's systematically coded data on contemporary preindustrial societies, data which tell us much about the limits which various kinds of subsistence technologies set on the development of other aspects of life, and the varying probabilities that operate within those limits (Naroll 1970).

A second major difference involves the clarification of the concept of progress and its place in evolutionary theory. In the older evolutionism there was a pervasive tendency to see progress on almost every front, from the technological to the moral. Human life was thought to be growing better in almost every possible way. Contemporary evolutionists do not share this optimism. When they use the term "progress," they use it only to call attention to specific directional trends (as in the expression, "the progress of a disease," or as Hogarth used the term in his famous series of prints, *The Rake's Progress*). I think there is a growing tendency among evolutionists to define sociocultural evolution as growth in the store of symbolically coded information and its consequences. Such a definition has no built-in assumptions about moral progress or any other form of human betterment: these remain empirical questions.

Third, the new evolutionism differs significantly from the old in its explanation of sociocultural evolution. As Marvin Harris has shown so well (1968) in his volume, *The Rise of Anthropological Theory*, the older evolutionism tended to be either eclectic or racist in its explanations. The newer evolutionism, by contrast, is increasingly materialist (the chief exceptions being Talcott Parsons [1964, 1966] and his students). The majority of the newer evolutionists explain the most basic patterns and trends in human life by reference to some combination of the following: (a) the human genetic heritage, (b) the technologies our species have fashioned to enhance this heritage, (c) the biophysical environment, and (d) intersocietal selection in the form of military and economic warfare (Lenski 1975, p. 147).

Finally, as noted previously, whereas 19th-century evolutionism tended to be deterministic in nature, the newer evolutionism is probabilistic. This change is, of course, a result of the tremendous impact statistical theory has had on our discipline. Even in situations where quantitative data are lacking, contemporary evolutionary theorists tend to develop their theoretical models in probabilistic terms.

One could easily add to this list of differences, but I hope these are sufficient to make clear why it is inaccurate to equate the newer and the older evolutionism, as Nisbet does.

CONCLUSION

Robert Nisbet concludes *Social Change and History* by saying, "Generalization is beyond question what we seek from the empirical and concrete. But

it is generalization from the empirical, the concrete, and the historical; not generalization achieved through their dismissal; not generalization drawn from metaphor and analogy" (1969, pp. 303-4). Against this standard, I would argue, the new evolutionism measures up rather well. Its claims of growth in human population, in the permanence of settlements, in the size and complexity of societies, in the degree of urbanization, in the extent of the division of labor, in social inequality, in the mobilization of energy, in the production of goods and services, in accumulated wealth, and, above all, in information are firmly grounded in the historical record. They do not depend on the logic of metaphor or analogy, and to imagine that they do is to misperceive the situation.

In trying to understand how a scholar of Robert Nisbet's obvious erudition and skill went so far astray in writing *Social Change and History*, I find myself coming back time and again to his failure to differentiate between general and specific evolution. Statements that are nonsensical when applied to some instance of the latter may be entirely valid on the level of general evolution. Not every society has grown in size and complexity, in urbanization, division of labor, and all the rest. Some have remained hunters and gatherers or horticulturalists down to the present day. Some have even regressed from levels they achieved at an earlier time.

Many of Nisbet's objections to the newer evolutionism seem to stem from his failure to appreciate the extent to which it has focused on general evolution. For understandable reasons, evolutionists today are taking more seriously than did their predecessors the essential unity of human evolution. As Wallerstein (1974), Heilbroner (1974), Moore (1966), Boulding (1964), and others have observed, the unitary character of human life is becoming ever more apparent. But it would be a mistake to imagine that this is a new development except in degree. One thing that has set socio-cultural evolution apart from organic evolution from the earliest times has been the ease with which information is transmitted across population boundaries (Lenski 1970, p. 61).

Some might object that the newer focus on general evolution is simply a device for evading a multitude of inconvenient exceptions to predominant trends and patterns. Perhaps. I think, however, that the newer focus on general evolution represents instead a serious attempt on the part of evolutionists to concentrate on that which is more basic and more relevant to the critical problems of the world today and in the future. It is an attempt to take the "signal-to-noise" problem seriously. In so doing, evolutionists must beware of oversimplifying, and, above all, they must beware of claiming, or even suggesting, that the principles of general evolution can be applied without modification on the level of specific evolution.

In conclusion I would argue that whatever faults the newer evolutionism has, the burden lies with its critics to provide a better explanation for the

observable patterns of human history. This Nisbet has failed to do in *Social Change and History*. Moreover, I seriously doubt that he or other critics of evolutionism can do this without themselves providing us with some new variant of general evolutionary theory—unless they are prepared to ignore the historical record.

History and evolutionary theory are not antithetical or even conflicting approaches to the study of the human record. They only appear to be so when we become confused over the level of generalization we are seeking and when the study of evolution is used to deny the existence of noise or the study of history to deny the existence of signal.

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A General Framework for Building Dynamic Macro Social Indicator Models: Including an Analysis of Changes in Crime Rates and Police Expenditures¹

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It is generally agreed that social indicators are measures of social conditions. Accordingly, recent interest in measuring social conditions and in building models of how various social forces determine changes in social conditions was predated by work of William F. Ogburn and his collaborators over 50 years ago. In this paper, we argue for a revival of analyses of social change based upon time series of indices of social conditions. To provide a general paradigm for this type of analysis, we describe (1) an opportunity-structures theoretical framework for generating specifications of equations of dynamic macro social indicator models, (2) a demographic accounting framework for grounding such equations in population stocks and flows, and (3) a structural equation strategy for estimating and evaluating the resulting models. To illustrate this paradigm, we present analyses of three equations determining changes in the national reported property crime rate, the reported violent crime rate, and the rate of public police expenditures. The equations fit annual 1947-72 time-series data well, yield theoretically meaningful coefficients, and lack demonstrable autocorrelation of disturbances. Moreover, the conditional forecasts of the 1973 values of the reported crime rates fall well within bounds set by the standard errors of the equations.

1. INTRODUCTION

During the past decade, sociologists and other social scientists have devoted a considerable amount of effort to the development of various indices of social life under the label "social indicators," a term which was first introduced into the literature as a result of the efforts of Bauer (1966) and his collaborators to assess the impact of the space program on American society. Within a short period, interest in social indicators grew so rapidly that Duncan (1969) dubbed it a "social movement."

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We shall not dwell here on the historical details of this movement, since they have been recounted elsewhere (see, e.g., Land 1975a; Sheldon and Parke 1975). Suffice it to say that most social indicators researchers would agree that social indicators are measures of social conditions.

There is less agreement, however, on how the conditions to be measured by social indicators should be identified. Two approaches to the identification problem have been discussed by Land (1975b, pp. 14-28). The first has its origin in social planning and the theory of social policy (see, e.g., Fox, Sengupta, and Throbecke 1973). It begins by identifying a social welfare function as dependent upon the utility or satisfaction levels of the members of a society. According to the theory of social policy, this is the function which social policy should aim to optimize. Since the utility for each individual depends upon his valuations of certain goods, services, and other aspects of his environment, it then follows that the social welfare function depends upon the same conditions and that these are the conditions which should be assessed by social indicators. In brief, this approach to the identification of social indicators leads to a class of indicators which are aptly termed "goal-output" or "welfare indicators." The second approach to the identification of social indicators requires that such indicators demonstrate a historical pattern of timing and covariation with social changes. This approach leads to the definition of the class of "indicators of social change" (see Sheldon and Moore 1968a; Duncan 1969). In brief, although an indicator of social change may be, or become, the object of social policy and hence a welfare indicator, this is not its defining criterion; rather, it must show some consistent relation to changes in social conditions.²

To illustrate, consider a classical example of a social indicator: the unemployment rate. Since an increase in this index measures a change in the employment status of individuals in a population, it meets the second criterion cited above and thus is reasonably considered an indicator of social change. On the other hand, the unemployment rate is usually considered an object of social policy, and hence it often enters into the standard social welfare functions of public policy-makers. As such, the unemployment rate is also a welfare indicator. The point of this illustration is that the two classes of social indicators are not mutually exclusive. Rather,

² Beyond this characterization of the two major approaches to the problem of identifying social indicators, we prefer not to get caught up in controversies over *intentional* definitions of social indicators. We believe that at this stage of development social indicators are best defined *extensionally*, that is, by pointing to the contents of major volumes of social indicators (e.g., Sheldon and Moore 1968a; U.S. Department of Health, Education, and Welfare 1969; Executive Office of the President, Office of Management and the Budget 1974) as examples of what is meant by the term. We shall argue below, however, that the operative meaning and interpretation of a social indicator depends upon its empirically verified membership in a social systems model as a parameter or variable (see Land 1971).

whether an indicator is considered to be a welfare indicator or an indicator of social change depends primarily on the point of view being taken toward the identification of social indicators. It is difficult, however, to focus social policy on more than a few indicators at a time. Moreover, the contents and relative weights of social welfare functions are subject to changes from time to time as the interests of members of a society and of policy-makers change. In general, therefore, as well as for the development of theories and models of social change, social scientists are probably better served by concentrating on the development of indicators of social change (see Sheldon and Moore 1968*b*, pp. 3-4).

Viewed from this perspective, the recent interest in social indicators has its sociological antecedent in work on the theory and measurement of social change by William F. Ogburn and his collaborators at the University of Chicago in the third and fourth decades of this century (see Duncan 1969, p. 7). Ogburn's theoretical position on social change was essentially that of a cultural evolutionist; he argued (Ogburn 1922) that social change must be traced back to cultural development or evolution. His empirical work on social change was characterized by an emphasis on uncovering present tendencies of change and estimating their probable consequences for the future. Thus, Ogburn and his co-workers stressed reliable measures of the changes that had taken place—quantitative descriptions in the form of statistical time series, where possible, or, in the absence of statistics, objective descriptions corroborated by the agreement of numerous careful observers (Duncan 1964, p. xvii). One product of this empirical effort was the two-volume *Recent Social Trends*, the report of the President's Research Committee on Social Trends (1933) under the direction of Ogburn, which was a comprehensive statistical report on trends in virtually every aspect of American life during that period. Indeed, its topical contents include most subjects found in the recent compendia on social indicators, as well as some additional topics pertaining to items of interest in the 1930s. Another product was an annual issue of the *American Journal of Sociology*, edited by Ogburn and devoted to social changes during the period 1928-34. Unfortunately, this work seems to have lost its continuity during the Great Depression of the 1930s; after the 1934 journal issue on social change, the series was discontinued. Although a special issue was published on "recent social changes" in May 1942, no further issues appeared after the Second World War.

In his empirical analyses of social and economic changes, Ogburn often employed what were, for his time, relatively sophisticated methods of statistical analysis in teasing out two components of change: long-term trends and short-term cyclical fluctuations. With improvements in statistical modeling and estimation techniques during recent decades, it might be expected that sociologists and economists would have improved upon

Ogburn's early efforts by constructing models for deciphering trends and fluctuations in social indicators. For this, however, this has not been the case. The one major exception during the past four decades, econometricians have developed empirically based models capable of simulating and forecasting fluctuations in economic (and some noneconomic social) development which has grown on the basis of three interrelated and empirical building blocks. First, many time series of indicators have been collected and made available to economists some on a monthly or quarterly as well as a yearly basis. Economists have the advantage, relative to sociologists, of an accounting which, despite its flaws and limitations, is suitable for communicating information in terms of a single monetary unit of value. More generally, economists have created an accounting system which facilitates quantitative analysis. Third, economics as a discipline is a set of macroeconomic theories which, even if oversimplified and incomplete and precisely specified, nonetheless can be operated out with great difficulty. For instance, Keynesian theory can be put in the form of a number of stochastic equations and identities.

In this paper, we suggest that the recent efforts to compile data on social indicators be brought into conjunction with modern demographic accounting and dynamic structural-equation models in order to reinstate Ogburn's emphasis on the analysis and prediction of social trends and fluctuations as a major objective of sociology. In particular, we discuss herein (1) a general opportunity-theoretical framework from which structural-equation models for predicting changes in social indicators can be specified; (2) a demographic accounting framework for keeping the equations of such a model consistent; and (3) a statistical methodology for specifying, estimating, evaluating, and forecasting with the equations of this theoretical and methodological framework, we present a model of social indicators in the area of public safety from an event analysis macro social indicator model of American society for the present.

2. A DEMOGRAPHIC ACCOUNTING FRAMEWORK FOR DYNAMIC MACRO SOCIAL INDICATOR MODELS

In practice, social indicator observations are sometimes in the form of individual-level data from samples of populations, and some are in the form of counts, averages, or rates computed from samples.

³ These three advantages are only relative and should not be overestimated. In point of fact, economic data are often poor and ill-suited to testing and estimating parameters, and economic theory varies enormously in its predictive guidance. These facts have led econometricians to note that "... econometrics is an art, and not a refined one at that" (Kuh and Schmalle).

or vital event registers. In the former case, the indicators may be termed "micro," whereas in the latter, they can be called "macro." Moreover, following Duncan's (1969) emphasis on replication of baseline surveys, many social indicators are increasingly available in replication form (i.e., in the form of two or three replicated cross-sectional surveys), while other social indicators, particularly those based on vital event registers and periodic samples, are available in time-series form. The type of model appropriate for the analysis of social indicators depends, of course, on the form of the observations. Our objective herein is the explication of a framework for the construction of dynamic models capable of accounting for changes, including both trends and cyclic fluctuations, in macro social indicators. Examples of such social indicators available in time-series form are the rates of marriage formation and dissolution, the birth rate, the infant mortality rate, and so forth.

Various social scientists (e.g., Gross 1966) have observed that one of the main obstacles to the construction of models of social conditions other than economic conditions has been the lack of a system of "social accounts" comparable to the "national income and product accounts" (for a description see, e.g., Ruggles and Ruggles [1970]). The latter accounts provide a systematic compilation of economic data into the basic variables used in the construction of macroeconomic models. Some analogous framework would be useful for setting out the social indicators to be determined in a model. Moreover, social accounts could provide social indicator models with a framework for building an internally consistent system of equations which determines many social indicators simultaneously, a function that the national economic accounts perform in the case of econometric models. Of course, macro models can be built without an accounting framework, and, indeed, this is just what happened in economics in the pre-Keynesian era. Thus, to explain variations in particular social indicators, one could proceed by utilizing various middle-range sociological theories. However, piecing these parts together into a consistent system would be difficult without some underlying social accounting framework.

Recently, several proposals for systems of social accounts have been advanced. Part of the reason for the existence of various proposals is the fact that an accounting system, of necessity, requires some common unit in which components can be expressed, and there exist several possible units for social accounting, namely, money (dollars), time, and people. Thus, for example, Ruggles and Ruggles (1973) propose to extend the national income accounts to "a system of social and economic accounts" in which the basic unit is the dollar, whereas Fox (1974) gives an approach to social accounting based on dollar equivalents of time allocation.

But the fact is that most social indicators are defined in terms of the numbers of people possessing certain attributes. Therefore, for our pur-



poses, it is more convenient to build an accounting system on the basis of "people" or "demographic" units, and thus of "demographic accounts." This type of framework for social accounting has been explored primarily by the British economist Richard Stone (1971, 1975). Appendix A illustrates how demographic accounts can be constructed for the United States from census and vital statistics sources.

The virtue of demographic accounts is that they provide a means of organizing the entire population of a society in terms of inflows and outflows on a year-to-year basis. Moreover, these flows can be classified into various states or activities, such as schooling, employment status, health status, etc., as is illustrated in Appendix A. To keep track of demographic flows, it is useful to employ an input-output matrix representation. Although the full power of the input-output demographic matrix representation is not employed in building the equations to be presented below, we give a discussion of this representation here because it provides the general context within which our approach can be developed and extended.

As Stone (1971, p. 36) notes, there are two possible input-output matrix formulations of demographic accounting, depending on how one records deaths. If one records deaths in the population at the beginning of the year and allows only those who survive to the end of the year to change state during the year, then one has what Stone calls a "closed" demographic matrix accounting system. On the other hand, if one allows the whole of the population alive at the beginning of the year (the "opening stock") to go through the process of redistribution among states during the year and records deaths at the next round, then one has an "open" matrix accounting system. In the first case, some people are shown as dying before they have reached their final state, whereas, in the second case, some are shown as surviving in their final state beyond the time of death.

TABLE 1
AN OPEN (YEAR-TO-YEAR) DEMOGRAPHIC MATRIX FOR ACTUAL RESIDENTS:
UNITED STATES, 1972-73 AND 1973-74

	OUTSIDE WORLD	UNITED STATES		TOTAL OUTFLOWS
		1973	1974	
Outside world	b'_t (3,602)	b'_{t+1} (3,630)	...
United States:				
1972	d_{t-1} (1,962)	$S_{t-1,t}$ (207,296)	...	$^o p_{t-1,t}$ (209,258)
1973	d_t (1,977)	...	$S_{t,t+1}$ (208,921)	$^o p_{t,t+1}$ (210,898)
Total				
inflows	$I'_{p'_{t-1,t}}$ (210,898)	$I'_{p'_{t,t+1}}$ (212,551)	...

SOURCE.—Approximated from *Current Population Reports*.

NOTE.—All numbers in thousands; index notation: $t-1 = 1972$, $t = 1973$, $t+1 = 1974$.

Building Dynamic Macro Social Indicator Models

Table 1 shows an open (year-to-year) demographic matrix for the United States for 1972-73 to 1973-74 constructed on the basis of accounting data such as those in Appendix A. Although the elements in this table are numbers (scalars), we give them a general vector or matrix notation, as they would become vectors or matrices in more refined demographic accounts containing classifications of the totals in each cell into age and social state (e.g., age-by-labor-force status as in table A2 of Appendix A). More generally, we define: $\mathbf{p}_{t,t+1}$ = the column vector of population flows from year t to year $t + 1$, where the element p_i represents the total number of people who flow through state i ; \mathbf{b}_t = the column vector of births and net migrations, where the element b_i represents the number of people who flow into state i from the outside world (by birth or migration) in the year t ; \mathbf{d}_t = the column vector of deaths, where the element d_i represents the number of people who flow out of state i into the outside world (by death) in the year t ; $\mathbf{S}_{t,t+1}$ = the matrix of survivors, where the element s_{ij} represents the number of people who leave year t in state i and flow into state j in year $t + 1$. We denote the transpose of each of the vectors and matrices by primes.

Given this notation, the elements of table 1 are related by the following definitions of the inflows and outflows of population from year t to $t + 1$: First, the flows of population *out of* year t consist of the deaths \mathbf{d}_t plus the survivors into year $t + 1$:

$$\text{Definition (outflows): } {}^0\mathbf{p}_{t,t+1} \equiv \mathbf{S}_{t,t+1}\mathbf{i} + \mathbf{d}_t, \quad (1)$$

where \mathbf{i} denotes the unit column vector, so that $\mathbf{S}_{t,t+1}\mathbf{i}$ denotes the row sums of $\mathbf{S}_{t,t+1}$, and the left superscript O denotes that this is a vector of outflows from year t . Thus, the vector of survivors into next year is defined as ${}^0\mathbf{p}_{t,t+1} - \mathbf{d}_t = \mathbf{S}_{t,t+1}\mathbf{i}$. Next, the flows of population *into* year t consist of the survivors from year $t - 1$ plus year t 's births and net migrations:

$$\begin{aligned} \text{Definition (inflows): } {}^I\mathbf{p}'_{t-1,t} &\equiv \mathbf{i}'\mathbf{S}_{t-1,t} + \mathbf{b}'_t, \\ \text{or, by transposing,} & \\ {}^I\mathbf{p}_{t-1,t} &\equiv \mathbf{S}'_{t-1,t}\mathbf{i} + \mathbf{b}_t, \end{aligned} \quad (2)$$

where the left superscript I denotes that this is a vector of inflows into year t . Thus, the vector of survivors from last year is defined as ${}^I\mathbf{p}_{t-1,t} - \mathbf{b}_t = \mathbf{S}'_{t-1,t}\mathbf{i}$. It should be clear that this model allows for differential fertility and mortality among states. As a specific illustration, from the numerical data of table 1, we see that the inflows into 1973 equal the outflows from 1973: ${}^I\mathbf{p}_{t-1,t} \equiv \mathbf{S}'_{t-1,t}\mathbf{i} + \mathbf{b}_t = 207,296 + 3,602 = 210,898 = {}^0\mathbf{p}_{t,t+1} \equiv \mathbf{S}_{t,t+1}\mathbf{i} + \mathbf{d}_t = 208,921 + 1,977$. Note, in particular, that ${}^I\mathbf{p}_{t-1,t} \neq {}^0\mathbf{p}_{t-1,t}$, but rather ${}^I\mathbf{p}_{t-1,t} = {}^0\mathbf{p}_{t,t+1}$.

A matrix of outflow transition coefficients, ${}^0\mathbf{C}$, can be formed by dividing

the elements in each row of $S_{t-1,t}$ by the corresponding element of the outflow vector of ${}^0p_{t-1,t}$:

$$\text{Definition (transition matrix): } {}^0C_{t-1,t} \equiv [\text{diag } {}^0p_{t-1,t}]^{-1} S_{t-1,t}, \quad (3)$$

where $[\text{diag } {}^0p_{t-1,t}]^{-1}$ denotes the inverse of a diagonal matrix formed from the outflow vector ${}^0p_{t-1,t}$. The matrix 0C gives the proportions in which the survivors from year $t-1$ distribute themselves among the various states in year t . For the simple scalar example of table 1, ${}^0C_{t-1,t}$ is just the scalar outflow transition coefficient 0.9906. By transposing equation (3) and postmultiplying by $[\text{diag } {}^0p_{t-1,t}]$, we can write:

$$S'_{t-1,t} = {}^0C'_{t-1,t} [\text{diag } {}^0p_{t-1,t}]. \quad (4)$$

Substituting (4) into (2) gives

$${}^1p_{t-1,t} = {}^0C'_{t-1,t} {}^0p_{t-1,t} + b_t. \quad (5)$$

This equation expresses a set of linear equations connecting the population inflows ${}^1p_{t-1,t}$ with the outflows ${}^0p_{t-1,t}$ and with the births and net migrations b_t .

Given the S or 0C matrices for each pair of years, equation (5) provides a recipe for making forward projections of the population from any given initial year. Furthermore, various equilibrium conditions can be specified under which the projections become very simple. For example, suppose we have the conditions:

$$\text{Equilibrium condition: } S_{t-1,t} = S_{t,t+1} = S, \quad (6)$$

and

$$\text{Equilibrium condition: } b_t = b_{t+1} = b. \quad (7)$$

The reader will recognize that these are the stationary equilibrium conditions that give the stationary population of the life table (see, e.g., Keyfitz 1968, pp. 3-19), in which each year a fixed number of births are recorded and survived in successive years through the ages by a fixed mortality regime. These conditions imply, by using equations (1)-(3), that:

$$d_t = d_{t+1} = d, \quad (8)$$

$${}^1p_{t-1,t} = {}^1p_{t,t+1} = p, \quad (9)$$

and

$${}^0C_{t-1,t} = {}^0C_{t,t+1} = C. \quad (10)$$

Given these simplifications, we can rewrite equation (5) as:

$$p = C'p + b \quad (11)$$

or

$$p = (I - C')^{-1} b. \quad (12)$$

This last equation has the same form as the Leontief open input-output model in economics (see, e.g., Chiang 1967, pp. 120-28). In that case, a matrix multiplier, usually written as $(\mathbf{I} - \mathbf{A})^{-1}$, transforms levels of final demand into levels of output. In equation (12), the matrix multiplier $(\mathbf{I} - \mathbf{C}')^{-1}$ transforms the numbers of people flowing from the outside world (through birth and net migrations) into the different states. The matrix $(\mathbf{I} - \mathbf{C}')^{-1}$ is a lower triangular matrix with 1's in the principal diagonal. The numbers below the diagonal in each column of this matrix represent the proportions in which those persons found in year t in a given state are likely to distribute themselves among the various states open to them in year $t + 1$.

We shall not elaborate the matrix algebra of demographic accounting further in this context. Suffice it to say that definitions (1)-(3) are adapted from Stone (1971, pp. 90-91), but equilibrium conditions (6) and (7) are somewhat different from his. We have formulated them in this way in order to emphasize that various conditions can be applied. For example, if we impose a constant mortality and fertility rate regime but allow the actual numbers of deaths and births to fluctuate, then we get a model which is similar to the discrete-time stable population model of P. H. Leslie (see, e.g., Keyfitz 1968, pp. 27-94; Pollard 1973, pp. 37-59).

3. AN OPPORTUNITY-STRUCTURES THEORETICAL FRAMEWORK FOR SOCIAL INDICATOR MODELS

Consider next the relation of these inflow-outflow demographic accounting equations to the construction of dynamic models for a macro social indicator analysis. First, as we noted above, the population must be divided into the categories which are to be used in the analysis. Usually, this will involve a breakdown into age categories and, within age categories, into various activities or other characteristics. These categories define the rows and columns of the $S_{t-1,t}$ survivor matrix (and, hence, of the ${}^0C_{t-1,t}$ outflow transition matrix). They also define the rows of the ${}^1p_{t-1,t}$ inflow vector. Note that these two representations correspond to the two major forms taken by social indicators. That is, social indicators such as unemployment or school enrollment rates correspond to elements of an inflow vector ${}^1p_{t-1,t}$ and represent *stocks* of persons in given states at a certain point in time. On the other hand, social indicators such as school retention rates correspond to elements of an outflow transition matrix ${}^0C_{t-1,t}$ and represent *flows* of persons from one state to another in a given time period.

Given a standard set of age categories, we could define one ${}^0C_{t-1,t}$ matrix for one activity and another for another activity; alternatively, we could define one large ${}^0C_{t-1,t}$ matrix incorporating many social activities simultaneously. If these activities are institutionally organized, it follows

that *demographic accounts quantify certain aspects of the institutional structure of the society*; that is, they give a numerical definition to certain aspects of the pattern of society.⁴ Taken by themselves, however, demographic accounts do not suffice to simulate and predict the pattern of social change, except under very special societal conditions. For, as we have seen, equation (12) can be used to transform population inflows into different states in order to make projections forward through time only if the transition matrix is constant. But it is evident from the discussion above that this will be the case only under a constant mortality, fertility, and mobility regime, such as that given by conditions (6) and (7), which yield a stationary population and a stationary society. However, since most contemporary societies do not attain this type of stability, the main use of equation (12) is the standard demographic application of life table and stable population theory to reveal the underlying asymptotic implications of current conditions.

An alternative approach to modeling social change is to use equation (5). Given a constant outflow transition matrix, the representation in equation (5) is formally analogous to the discrete-time Markov process model that has been applied to many social processes such as occupational and geographical mobility (for a survey see, e.g., Bartholomew [1973]). Numerous authors have noted, however, that the assumption of a constant transition matrix often can be maintained only with respect to a short period of time, if at all.⁵ Thus, in order to model the actual changes that occur in a society, it is more realistic to allow the transition matrix to change from period to period (i.e., to be time inhomogeneous), as in equation (5). But this then raises the issue of how the changes are to be determined.

There are several possibilities to choose from. For instance, as in classical demography, one can introduce a hypothetical sequence of changes into an initial observed matrix and study the resulting social changes. Unless the sequence of transition matrices is to be rather arbitrary, however, some procedure must be utilized for its derivation. One possibility is to assume that the transition matrix will change to some final form after k periods and then to interpolate a sequence of matrices between the observed and

⁴ Many aspects of social life of interest to sociologists cannot be captured in demographic accounts simply because the requisite data do not exist. Nevertheless, we believe that more than enough such data are available to keep sociologists busy building social indicator models for some time.

⁵ E.g., in his study of mobility within a state police system over the years 1927-70, Stewman (1975) found that a stationary condition could be approximated only for the period 1949-70, and he referred to various "perturbations" which occurred during this latter period. In the present paper, we are interested in determining these perturbations of transition coefficients, insofar as possible, as a function of other variables.

final forms.⁶ Another possibility is to parameterize the elements of the transition matrix, that is, to represent the transition coefficients as themselves functions of other processes. Again, there are several possible ways of proceeding with this. First, in what may be called a "stochastic parameterization," the transition coefficients may be represented as deriving from a probability distribution which is itself a function of certain parameters, an approach explored in the context of mobility matrices by Spilerman (1972a). Second, if the transition coefficients are systematically related to properties of the individuals in a population, one can parameterize them as a function of the relevant individual-level properties, a procedure analyzed in the context of mobility matrices by Spilerman (1972b). This latter procedure, which may be termed a "micro parameterization," allows the breakdown of transition coefficients into (1) a portion due to changes in the composition of individuals in a population who possess certain properties and (2) a residual component which may be identified with structural change. A third possibility, which may be called, analogously, a "macro parameterization," consists of parameterizing the transition coefficients in terms of functions of other flow or stock coefficients (or determinants of the latter). In terms of the matrix framework of equation (5), this approach consists of writing the transition coefficients of one matrix for a given type of activity as a function of coefficients from matrices representing other activities, presuming that the latter are causally independent of the former.⁷ In brief, the macro approach parameterizes the changes in one macro stock or flow coefficient in terms of other macro variables. Thus, it provides for representing societal changes directly in terms of other societal changes rather than residually, as in the second approach. In spite of this, surprisingly perhaps, sociologists seem to have given it little attention. For the most part, our approach to the construction of social indicator models will take this latter alternative, although we shall also indicate how the first two options can be incorporated.⁸ We believe that this approach is capable of measuring the degree

⁶ E.g., assuming that the population of Mexico in 1960 is the midpoint of demographic transition, Keyfitz (1968, pp. 74-80) derived the implications of a logistic approach to the fertility and mortality conditions of the United States in 1940.

⁷ If this is not the case, then a set of nonrecursive simultaneous equations is necessary to account for the interdependence.

⁸ Note that the micro and macro parameterizations lead to complementary models. E.g., in determining the composition of changes in the unemployment rate, a macro parameterization would focus on other aggregate economic conditions, whereas an individual parameterization would break down the employment status of individuals into a function of various social characteristics such as age, sex, race, education, etc. Of course, for a complete understanding of most social phenomena, neither type of parameterization alone is sufficient. In other words, most social conditions have both individual and aggregate causes.

of institutional interdependence among those aspects of society captured by the demographic accounting matrices.

As a concrete example, Land (1975*b*, pp. 29–31) reported an analysis of a simple schooling indicator: the eleventh to twelfth grade continuation ratio, computed annually for 1910–66 by Ferriss (1969). On the basis of correlations reported by Ferriss, Land broke down this ratio using a regression analysis into a positive time trend component, a positive impact of the unemployment rate, a negative impact of the military expansion rate, and a stochastic disturbance. In brief, this model related a macro indicator of one aspect of the educational sector to other macro indicators, namely, those of the economic and military (political) sectors, respectively. Although this analysis was instructive, it was not based on any general theoretical principle pertaining to social change.

To proceed more generally with the construction of dynamic macro social indicator models, it is thus necessary to establish at least the rudiments of a general theory for the specification of variables which determine changes in social indicators. One key as to how to formulate such a theory can be found in Ogburn's efforts over 50 years ago to determine the influence of business cycles on certain social conditions (Ogburn and Thomas 1922; Ogburn 1923). After reporting on the correlations of various indices of business cycles with such social conditions as business failures, wages, unemployment, destitution, immigration, strikes, marriage, divorce, births, suicides, crime, liquor consumption, religious activities, and liberal labor policies, Ogburn (1923/1964, p. 246) made this comment:

One conclusion to be drawn is that the quantitative determination of the influence of the business cycle on social conditions is the measurement of a social force of magnitude and importance. Frequently failure, wages, poverty, marriage, crime, etc., are seen as purely individual problems, in terms of personality, will power, and individual behavior, as though the subjective free will of the individual were responsible in a purely personal way. But there is another side to the picture. Here we have shown the objective determinism of a social force that profoundly affects social conditions. Since such a social force exists, there is value in seeing it as a social force and not trying to treat the situation in terms of individuals and personalities.

In other words, Ogburn, in this quotation, emphasized the importance of what we have referred to above as a macro parameterization, in terms of business cycles, of changes in stock and flow indicators of social conditions.

More generally, Ogburn's analysis implicitly suggests a parameterization of changes in social indicators in terms of social forces that affect the opportunities of individuals to accomplish whatever objectives their "free wills" establish. In modern terminology, assuming that individuals seek to maximize their satisfactions or self-interests subject to whatever con-

straints are placed upon their behavior by an available opportunity structure, Ogburn emphasized parameterizations of changes in opportunity structures in terms of social forces such as those created by the business cycle.

In relation to the demographic accounting framework for social indicators introduced above, this suggests that an opportunity-structures interpretation would be fruitful. As a specific illustration, consider how the high school continuation model described above could be so interpreted. If we focus on only those students who flow out of the eleventh grade from year $t - 1$ to year t , then the ${}^0\mathbf{p}_{t-1,t}$ vector of equation (5) becomes a simple scalar ${}^0p_{t-1,t}$, and similarly the vector ${}^1\mathbf{p}_{t-1,t}$ of students who flow into the twelfth grade from year $t - 1$ to t becomes the scalar ${}^1p_{t-1,t}$. Moreover, the transition matrix ${}^0\mathbf{C}'_{t-1,t}$ becomes the scalar transition coefficient ${}^0c_{t-1,t}$. Assuming the birth vector, \mathbf{b}_t , can be ignored (i.e., assuming net migration can be ignored), we find that equation (5) reduces to:

$${}^1p_{t-1,t} = c_{t-1,t} {}^0p_{t-1,t}, \quad (13)$$

or

$$c_{t-1,t} = {}^1p_{t-1,t} / {}^0p_{t-1,t}. \quad (14)$$

If ${}^0p_{t-1,t}$ is taken as an index of the number of individuals demanding a twelfth year of schooling in the absence of competing alternative opportunities, then the transition coefficient determined in this last equation can be taken as a measure of the opportunity for a twelfth year of schooling. More generally, if we conceive of social benefits as those socially valued things that individuals attempt to attain or obtain in order to maximize their satisfactions, it follows that the transition coefficients of demographic accounting matrices can be taken, under conditions like those assumed here, to be measures of opportunities for social benefits.

In brief, this gives an interpretation of demographic accounting transition coefficients in terms of the structure of opportunities for social benefits. However, as noted above, equation (5) does not assume that the transition coefficients are constant, which is to say that the opportunity structure for social benefits is not assumed to remain constant. Rather, its degree of constancy over time is an empirical question. In the spirit of Ogburn, moreover, at least two components of change in opportunity structures should be distinguished, namely, trends and cyclical fluctuations, and these components should be parameterized in terms of social forces that affect the availability of social benefits. Specifically, for a theoretical guideline to the parameterization of changes in opportunity structures, we offer the following principle as a generalization of Ogburn's approach to the explanation of changes in social conditions. Because equation (14) shows that such a parameterization can be made either in terms of the demographic transition coefficients or in terms of the inflow

and outflow components of these coefficients, we state the postulate in two forms.

Opportunity-structures postulate (OSP1).—Suppose that the population is classified into n states (competing alternatives) with respect to a social activity. For states $i, j = 1, \dots, n$, we can then compute outflow transition coefficients ${}^o c_{i-1,t}^{ij}$ according to definition (3) for flowing from state i to state j between each pair of years $t-1$ and t . If we denote the amount of change in these transition coefficients between adjacent pairs of years by $\Delta {}^o c^{ij}$, that is, $\Delta {}^o c^{ij} = {}^o c_{t-1,t}^{ij} - {}^o c_{t-2,t-1}^{ij}$, then the changes in the opportunity-structure outflow transition coefficients, $\Delta {}^o c^{ij}$, are determined by those social forces that affect elements of the set of competing alternative opportunities (i.e., the set of states $j = 1, \dots, n$ to which transitions are possible from year $t-1$ to t). OSP2: Similarly, if we denote changes in the opportunity structure inflows into state j , $j = 1, \dots, n$, for pairs of adjacent years by $\Delta {}^i p^j$, then these changes are determined by those social forces that affect elements of the set of competing alternating opportunities.

In brief, to determine changes in the transition rate at which people flow from state i to state j , postulate OSP1 says that one simply needs information on those social forces that affect opportunities for transition from state i to state j for all possible states j . In functional equation form, postulate OSP1 can be written:

$$\Delta {}^o c^{ij} = f(x_{1,t-k}, x_{2,t-k}, \dots, x_{m,t-k}), \quad (15)$$

where x_1, x_2, \dots, x_m represent social forces affecting the available opportunities, with arbitrary lags $t-k$.⁹ Similarly, postulate OSP2 can be written

$$\Delta {}^i p^j = f(x_{1,t-k}, x_{2,t-k}, \dots, x_{m,t-k}). \quad (16)$$

Ideally, a complete set of demographic accounts gives sufficient information to determine equations of the form of (15). But, in practice, information sufficient to compute transition coefficients and changes therein simply is not available for many social conditions. In these cases, however, social indicators are often available in the form of inflow stocks or rates of occupancy of social states for each year, in which case postulate OSP2 and equation (16) are applicable. The basic similarity of the two forms of the postulate follows from the relation of the inflows and the transition coefficients given in equations (13) and (14).

As a specific application of the postulate, consider again the high school continuation rate example cited above. Since the social indicator to be determined in this case is in the form of a transition rate, postulate OSP1

⁹ In general, the specific forms of the functions in equations (15) and (16) for particular social indicators and the exact lags of the x -variables are not completely known a priori. In any specific case, they must be determined on the basis of both theoretical argument and empirical evidence.

says that to determine its changes it is sufficient to measure changes in those social forces that influence the opportunities for whatever activities are available to high school students, namely, employment or military service. In brief, the postulate fits well with the empirical analysis actually followed in this example.

The postulate also accords well with other sociological models and analyses of social change. To begin with, it subsumes the explanatory assumptions used by Ogburn in the analysis cited above of the impacts on social conditions of the social forces created by the business cycle (as it should, since we began by generalizing Ogburn). In addition, the opportunity-structures postulate is consistent also with recent studies of changes in occupational mobility by Hauser et al. (1975), in which the shifting occupational structure has been shown to be the driving force in intergenerational mobility patterns, suggesting that stratification research should be reoriented toward a study of those social forces that determine changes in the occupational structure.

The opportunity-structures postulate has no pretensions to being a profound theory of social change. It does not incorporate a grand image of the form to which modern societies are evolving, and around which all social trends must cohere in some fashion, as have some theories (e.g., Bell 1973). While we are willing to grant that the postulation of some master social trends (e.g., bureaucratization) may aid in the interpretation of changes in specific social conditions, we prefer to build such hypotheses into a theoretical model very slowly and carefully in conjunction with specific empirical analyses. Ogburn's attitude towards "theory" was one of parsimony (Duncan 1964, p. xv), an attitude which we feel is most appropriate in the explanation of changes in social conditions.

Our postulate could not be much simpler without becoming a truism. Certainly, for any particular social indicator, it does not give a completely specified sociological theory of change. Rather, it says that substantive hypotheses incorporating standard sociological notions of norms, roles, stratification, conflict, etc., are relevant to the extent that they explain variations in opportunity structures. As such, the postulate is useful if it furnishes a guideline for the specification of equations that perform well empirically. Some evidence on this will be offered below.

4. SOME CONSIDERATIONS IN BUILDING STRUCTURAL-EQUATION MODELS FOR CHANGES IN MACRO SOCIAL INDICATORS

A. The Specification, Interpretation, and Estimation of Dynamic Equations

In his pioneering analyses of the effects of business cycles on social conditions, Ogburn (Ogburn and Thomas 1922; Ogburn 1923/1964) made extensive use of simple bivariate correlation analysis of time series as an

analytical tool. Other methods for the statistical analysis of time series simply had not been developed very fully by 1920. Moreover, the constraints on computation placed by hand methods were indeed real. In the intervening years, great strides have been made both in the development of statistical methodology for time series and in computational devices and aids. In particular, at present there exist three major types of statistical methods suitable for building models for the analysis of time series. First, under the label of Box-Jenkins or Box-Tiao methods (Box and Jenkins 1970; Box and Tiao 1965, 1973), a set of statistical methods has recently been developed based upon combinations of autoregressive and moving average models of stochastic disturbances or "noise." Second, in the post-World War II period, techniques of spectral analysis for time series have been developed largely on the basis of the initial efforts of Wiener (1949), Bartlett (1966), and Blackman and Tukey (1958); Mayer and Arney (1974) provide a survey of spectral concepts for sociologists with applications to social change. Third, stimulated largely by the development of Keynesian theory, econometricians, during the past four decades, have modified the general linear model of statistical theory and have adapted it to the analysis of economic time series in the form of structural-equation models; a survey of these methods can be found in any recent econometrics text, such as Johnston (1972) or Theil (1971).

Each of these methods has its advantages as well as its weaknesses, and we expect that each will make some contribution to the development of dynamic social indicator models. For our purposes, however, Box-Jenkins methods have the deficiency of not allowing the incorporation of detailed structural information about the determination of social indicators (i.e., hypotheses about the way social conditions are determined). In other words, Box-Jenkins models are essentially sophisticated models of noise, whereas we want to construct models embodying structural or causal hypotheses. Similarly, spectral analysis has limited uses for our purposes because it requires on the order of 100 time points for application (Mayer and Arney 1974, p. 346). For social indicator models built on annual data from the post-World War II era, time series usually contain less than 30 time points. Because they allow the explicit incorporation of structural or causal hypotheses, and because they are relatively robust for small samples, we believe that structural-equation model-building procedures provide the most appropriate formalism for the development of dynamic macro social indicator models.¹⁰ Of course, where possible, it is useful to corroborate structural-equation results with information derived on the basis of other procedures such as those mentioned above.

In this section, we outline some characteristics of the use of structural-

¹⁰ The structural-equation modeling procedures also are, incidentally, the closest of these three to the correlation analyses made by Ogburn.

Building Dynamic Macro Social Indicator Models

equation modeling procedures in a time-series context. Although sociologists are familiar with the use of structural-equation modeling procedures in a cross-sectional survey context (see, e.g., Blalock 1971; Goldberger and Duncan 1973; Duncan 1975), the construction of dynamic structural-equation models presents a number of problems of interpretation not normally encountered in a cross-sectional analysis. We shall elucidate some of these problems and illustrate them with some concrete empirical examples from our ongoing research.

To begin with, it is useful to state the sense in which the equations of macro social indicator models—of the form of (15) and (16)—are “dynamic.” The mere fact that the equations are to be fitted to a time series of observations does not make them dynamic. Rather, dynamics are introduced into the equations by the fact that the values of the social indicators may change over time and take at least one time period to adjust to changes in other variables. To clarify this characterization, it is necessary to introduce an expression for the rate of change of a variable with respect to time. For a continuous analysis, this rate is provided by the time derivative of the variable (see Coleman [1968] for an exposition of continuous-time procedures for modeling social change), whereas, in a discrete-time analysis, the rate of change is given by the finite first difference $\Delta y = y_t - y_{t-1}$ between the values of the variable, y , at times t and $t - 1$. Since social indicator time series are given in discrete-time form, it is natural to use the latter rate, as we have in our postulate on opportunity structures, although the former rate can, of course, be taken as a limit of the latter. One of the simplest possible parameterizations of the first difference of a variable is as a linear function of the time $t - 1$ value of y with coefficient b and a set of exogenous variables, which, for the present, we shall denote by a constant a .¹¹

$$\Delta y = y_t - y_{t-1} = a + by_{t-1}. \quad (17)$$

This equation says that the amount of change in y -values from time $t - 1$ to time t is a function of the value of y at time $t - 1$ and the values of the exogenous variables as summarized in the constant a . Equivalently, by adding y_{t-1} to both sides of (17), we have

$$\begin{aligned} y_t &= a + by_{t-1} + y_{t-1} \\ &= a + (1 + b)y_{t-1} \\ &= a + b^* y_{t-1}, \end{aligned} \quad (18)$$

where $b^* = 1 + b$. Because of its convenience for statistical estimation and substantive interpretation, we shall hereafter primarily use the form (18) rather than (17).

¹¹ More complicated nonlinear forms of difference equations can, of course, be used if there are theoretical reasons for specifying a nonlinear form or if a linear equation provides an inadequate fit to a set of data.

Consider next the interpretation of the behavior of first-difference equations. Given an initial value of y , y_0 , a standard theorem on linear first-difference equations of the form (18) (see, e.g., Goldberg 1958, p. 86) states that the unique solution for the sequence of y -values is:

$$y_t = \begin{cases} b^{*t} (y_0 - y^e) + y^e, & \text{if } b^* \neq 1, \\ y_0 + at, & \text{if } b^* = 1, \end{cases}$$

where $y^e = a/(1 - b^*)$ is the equilibrium value toward which the y -values will move as t increases without bound for constant a and b^* coefficients. The behavior of the solution sequence $\{y_t\}$ is given in table 2 for all

TABLE 2
BEHAVIOR OF THE SOLUTION SEQUENCE $\{y_t\}$ TO THE DIFFERENCE EQUATION
 $y_t = a + b^*y_{t-1}$, $t = 0, 1, 2, \dots$

Case	CONDITIONS			INFERENCES	
	a	b^*	y_0	For $t=1, 2, 3, \dots$	The sequence $\{y_t\}$ is
1	$b^* \neq 1$	$y_0 = y^e$	$y_t = y^e$	constant ($=y^e$)
2	$b^* = 1$	$y_0 > y^e$	$y_t > y^e$	monotone increasing, diverges to $+\infty$
3	$b^* > 1$	$y_0 < y^e$	$y_t < y^e$	monotone decreasing, diverges to $-\infty$
4	$0 < b^* < 1$	$y_0 > y^e$	$y_t > y^e$	monotone decreasing, converges to limit y^e
5	$0 < b^* < 1$	$y_0 < y^e$	$y_t < y^e$	monotone increasing, converges to limit y^e
6	$-1 < b^* < 0$	$y_0 \neq y^e$...	damped oscillatory, converges to limit y^e
7	$b^* = -1$	$y_0 \neq y^e$...	divergent, oscillates finitely
8	$b^* < -1$	$y_0 \neq y^e$...	divergent, oscillates infinitely
9	$a = 0$	$b^* = 1$...	$y_k = y_0$	constant ($=y_0$)
10	$a > 0$	$b^* = 1$...	$y_k > y_0$	monotone increasing, diverges to $+\infty$
11	$a < 0$	$b^* = 1$...	$y_k < y_0$	monotone decreasing, diverges to $-\infty$

SOURCE.—Goldberg (1958, p. 84).

possible values of a , b^* , and y_0 . In particular, if $-1 < b^* < 1$, then $\{y_t\}$ converges with limiting value y^e , and if $|b^*| > 1$, then $\{y_t\}$ diverges unless $y_0 = y^e$. In the former case, the difference equation is "convergent," whereas in the latter it is "explosive."

Table 2 will be of use in interpreting the estimated equations to be presented below. Note, however, that it is derived under the assumption of constant a and b^* coefficients. The stability of the b^* coefficient is an empirical question, and we shall show how to obtain some evidence on sta-

bility below. However, given our interpretation of the a coefficient, it is unlikely to be constant. In brief, we assumed above that the a coefficient represents the numerical value of a function of all exogenous variables explicitly included in the model (usually an additive total of the variables, each multiplied by its regression coefficient). In any specific equation, this "forcing function" would, of course, be broken down into various specific exogenous variables. Moreover, these latter variables would likely change from year to year, which implies that the equilibrium value $y^e = a/(1 - b^*)$ would also change from year to year. In other words, in a difference equation with changing exogenous variables, the implied equilibrium value of the equation becomes a moving equilibrium, changing from year to year. Thus, for any given year t we can compute the coefficient a of an equation as the sum of the exogenous variables multiplied by their coefficients and obtain the implied equilibrium value for year t , $y_t^e = a_t/(1 - b^*)$. Insofar as the exogenous variables change significantly from year to year, this procedure may yield a changing sequence of implied equilibrium points. On the other hand, if the values of the exogenous variables are relatively constant or have counterbalancing effects, then the implied equilibria, y_t^e , will be relatively close together, and, given a constant b^* coefficient, the sequence of observed y_t values will tend toward those indicated above.

With this characterization of the behavior of linear first-difference equations in hand, we can give a more precise statement of the sense in which equations of the form (15) or (16) are dynamic. For this, suppose first that the constant a of equation (18) is disaggregated into the sum $a_1 + a_2x_{t-k}$ of a constant a_1 and a constant a_2 multiplied by an exogenous variable x lagged k time periods, with k being some definite positive integer. We can then rewrite equation (18) as

$$y_t = a_1 + a_2x_{t-k} + b^*y_{t-1}. \quad (19)$$

Suppose now that x_{t-k} is given some fixed value and that y_t adjusts to that value immediately so that $y_{t-k} = y_{t-1} = y_t$.¹² Then equation (19) can be written as

$$y_t - b^*y_{t-1} = (1 - b^*)y_{t-k} = -by_{t-k} = a_1 + a_2x_{t-k}, \quad (20)$$

where the b in the second equality is the same as that defined in equation (17). This last equation can also be written as

$$y_t = y_{t-k} = -(a_1/b) - (a_2/b)x_{t-k}. \quad (21)$$

In this form, we see that the assumption that the endogenous variable adjusts immediately to the exogenous variable x implies that equation (19) can be written without a lagged value of y . Mathematically, finite-time

¹² For finite difference equations, it is sufficient that "immediately" mean that the time period of adjustment is shorter than the time period of observation.

equations without lagged endogenous variables are termed algebraic rather than difference equations. Algebraic equations are not considered dynamic whereas difference equations (or their continuous-time counterparts) are. In brief, we conclude that equations describing the behavior of macro social indicators of the form of equations (15) or (16) are dynamic if their endogenous variables take one or more time periods to adjust to exogenous changes in social forces and thus such equations have nonzero coefficients for the lagged endogenous variables.

As a specific illustration in a social indicator context, consider the child adoption rate. If it is the case that the child adoption rate in year t (denoted by y_t) adjusts with a dominant lag of, say, two years to the illegitimate birth rate (denoted by x_{t-2}), then a simple algebraic equation of the form of equation (21),

$$y_t = a_1 + a_2x_{t-2}, \quad (22)$$

will serve as a model for determining changes in the child adoption rate. On the other hand, if the child adoption rate does not have a dominant lag or combination of lags for reacting to changes in the illegitimate birth rate, then a difference equation at least as complicated as the linear form (19) with a nonzero coefficient for the lagged endogenous variable is necessary to account for changes in the adoption rate. In other words, in the latter case, if the illegitimate birth rate increases in some period t , then the reaction time of the child adoption rate to this change is spread out over several subsequent time periods of observation rather than being concentrated in a particular subsequent time period. It is in this type of situation that the appropriate model is dynamic.

For statistical estimation of difference equations of the form of (18), the usual procedure is to add to the difference equation a stochastic disturbance, ϵ_t , representing the effects of all unmeasured variables left out of the equation at time t , to get the form:

$$y_t = a + b^*y_{t-1} + \epsilon_t. \quad (23)$$

Problems of statistical estimation for difference equations of this linear form have been studied quite extensively in the applied statistics literature (for a summary see, e.g., Johnston [1972], pp. 292-321). If the difference equation to be estimated is not part of an entire system of simultaneous difference equations, then the main barrier to the use of ordinary-least-squares (OLS) regression to estimate an equation such as (23) is the presence of autocorrelation of the disturbances over time. However, the seriousness of the problems created by autocorrelation depends on the nature of the model. For example, if the model contains only lagged exogenous variables or no lagged variables at all, then autocorrelated disturbances do not produce biased estimates of the regression coefficients, even

in small samples. In this case, however, the variances of the regression coefficients are likely to be underestimated, leading to inflated *t*- and *F*-ratios and hence to possibly erroneous inferences. On the other hand, lagged *y*-values with random disturbances will give OLS estimators which are statistically consistent, though biased in small samples. However, the combination of lagged *y*-values and autocorrelated disturbances produces inconsistent OLS estimators. Moreover, in the latter case, the conventional Durbin-Watson test for autocorrelation of the disturbances is biased towards the value for a random disturbance.

B. The Statistical Evaluation of Dynamic Macro Social Indicator Models: A Public Safety Example

The statistical procedures appropriate for getting a "final" form of each of the stochastic difference equations estimated in a dynamic macro social indicator model perhaps can be illustrated best in the context of a particular substantive example. For this purpose, we shall discuss the specification, estimation, and evaluation of equations for three social indicators pertaining to the topic of public safety, namely, the national reported property and violent crime rates, and the national rate of public police expenditures per capita. The analysis of the dynamic interrelationships of these three variables is relevant to the following types of sociological questions: (1) Which social forces tend to force up or down reports of property and violent crime? (2) Net of these social forces, do increasing allocations of fiscal resources to police activities increase or decrease reported crime rates? (3) Do increases in reported crime produce increases or decreases in police expenditures?

Before discussing the specification of equations for these social indicators, we emphasize that the importance of this analysis does not rest on the accuracy or veracity of the reported property and violent crime rates as measures of the "true" extent of acts violating the relevant criminal laws. Rather, our analysis pertains to the determination of changes in the incidences of reported crimes; these are important social indicators primarily because of the widespread public uses and significance attached to them. As such, sociologists should concentrate some efforts on developing models of the complex social processes that generate reported crime rates.¹⁸ Our equations can be viewed as a contribution to this effort.

Consider first the specification of an equation for determining changes in the national reported property crime rate. Viewed from the opportunity-structures perspective introduced in Section 3, this rate should be affected

¹⁸ This perspective on reported national crime rates as social indicators of the complex interactions of norm violators, victims, and bureaucratically organized law enforcement agencies is consistent, e.g., with the interpretations Kitsuse and Cicourel (1963) and Black (1970) give to crime statistics.

by those social forces that influence the opportunity for persons to engage in a property crime *and* by the opportunity for victims and others to report property crimes. Several of these forces were cited in Biderman's (1966) analysis of the composition of changes in national crime rates. First, Biderman underlined the importance of prosperity in creating more property and thus in increasing the opportunity for property crime. Second, Biderman emphasized that reported property crime could be driven up by general price inflation, which serves to increase the value of more pieces of property above the minimum levels for official reporting. Third, Biderman suggested the relative importance of cohorts of young men (who are assumed to be the most prone to commit property crimes that become reported) in the age structure of the population as a force driving up the reported property crime rate. Biderman points out that increases in social forces such as these are often associated with "social progress," and thus that a rising reported property crime rate may be an index of social development. Another progressive social force emphasized by Biderman is the increasing professionalization of police organizations, which increases the likelihood that property crimes will be reported to the police and become official crime statistics.

To apply the Biderman analysis to the specification of an equation suitable for determining trends and cyclical fluctuations in the reported national property crime rate during the post-World War II era, we begin with the following preliminary functional equation:

$$RPCR_t = f(RPCR_{t-1}, \%M_t^{15-24}, CPI_t, AGNP/POP_t, PE/POP_t), \quad (24)$$

where the variables in order of appearance are: $RPCR_t$, the reported national property crime rate, expressed as the total number of burglaries, larcenies (\$50 and over), and motor vehicle thefts reported in year t per 100,000 midyear population; $\%M_t^{15-24}$, the percentage of the total population that is male, ages 15-24, in year t ; CPI_t , the consumer price index for year t , expressed as a percentage of the 1967 (base) CPI; $AGNP/POP_t$, the actual dollar value of gross national product in billions of year t dollars per 100,000 midyear population; PE/POP_t , the federal, state, and local police expenditures in billions of year t dollars per 100,000 midyear population. Briefly, the $\%M^{15-24}$, CPI_t , and $AGNP/POP_t$ variables in equation (24) correspond, respectively, to the age-structure, inflation, and prosperity social forces hypothesized by Biderman to determine changes in the reported property crime rate.¹⁴ In addition, the lagged

¹⁴ Note that we have not chosen to use the usual demographic procedure of age standardization to control for the effect of the age structure on the reported crime rates. Although that may be a useful analytical technique for some purposes, we have chosen not to use it here for two reasons. First, it is substantively important to assess the causal importance of age-structure variables relative to that of other factors affecting

reported property crime, $RPCR_{t-1}$, has a specific interpretation in terms of the "institutional accretion" hypothesized by Biderman to contribute to reported crime rates through increasing police professionalization. Finally, equation (24) includes a police expenditures per capita variable as an index of the allocation of fiscal resources to police organizations in year t . This index is included in the equation to assess whether, net of the other social forces represented in the equation, the extent of societal resources allocated to police agencies has a positive, null, or negative effect on the reported property crime rate. There is no consensus in the sociological literature on this relationship, and thus we do not attach any specific algebraic sign as an a priori hypothesis.¹⁵

This specification of equation (24) is illustrative of the usual amount of a priori information available for the construction of social indicator models. In brief, sociological theory usually is strong enough to specify a set of exogenous or predetermined variables that likely determine a social indicator, as well as the algebraic signs corresponding to the directions of the hypothesized relationships. However, sociological theory typically is not sufficiently detailed enough to specify the exact form of the function determining the indicator nor the values of its coefficients. Sometimes the optimal lags of the exogenous or predetermined variables also are not specifiable on an a priori basis. To determine these unknowns, recourse must be had to empirical estimates.¹⁶

Since there is no theoretical basis in the Biderman specification of equation (24) for anticipating a nonlinear functional relationship, we obtain the following OLS linear regression estimates for annual observations 1947-72:

Reported property crime rate (1):

$$RPCR_t = -2192.54 + 1.15RPCR_{t-1} + 49.84\%M_t^{15-24} + 27.51CPI_t \\ (3.57) \quad (5.95) \quad (1.56) \quad (3.04) \\ + 1717.73AGNP/POP_t - 1020.90PE/POP_t + e_t, \quad (25) \\ (1.98) \quad (4.39)$$

$$\bar{R}^2 = .9985, \bar{R}_A^2 = .9936, SEE = 51.28, D-W = 1.95, df = 20,$$

reported crime rates. This is easily accomplished in a functional equation context in which all relevant factors are allowed to vary (see, e.g., the analysis below of the net effect of the unemployment rate on the reported property crime rate in equation [26]). Second, one purpose of constructing a dynamic social indicator model is to improve the ability of sociologists to forecast social conditions. Again, this is easily accomplished in the context of functional equations in which all relevant factors can vary (see, e.g., the forecasts made in Sec. 4C).

¹⁵ For a sampling of sociological opinions on this question, see Reiss's (1970, p. 39) argument that police "cause" crime rates to increase, and Chilton's (1968, p. 90) discussion of bureaucratic incentives for police organizations to keep crime rates down.

¹⁶ Of course, this situation is no worse than that in most empirical disciplines. For an illustration of the uses of empirical data to complete the specification of macroeconomic functions, see, e.g., Kuh and Schmalensee (1973).

where the regression coefficients are estimated in terms of the metrics of the variables stated above, the t -ratio for each coefficient is given in parentheses below the coefficient, and e_t represents the computed OLS residual.

Examining the coefficient estimates in order of appearance, we find that the constant term is significantly negative. This is substantively interpretable, for it says that the reported property crime rate could decrease to zero without all of the other variables in the equation having to go to zero levels. In other words, it would be possible to have zero reported property crime without zero GNP, zero young males, and so forth.

Next, we find that the coefficient for the lagged reported crime rate has a highly significant t -ratio, and the coefficient indicates that, net of the other regressors, the reported property-crime rate in year t should be larger than that in year $t - 1$. Moreover, interpreting the value of this coefficient as being greater than unity, one would infer from case (2) of table 2 that the reported property crime rate is diverging to positive infinity. However, this interpretation is flawed. For not only is it impossible for the reported property crime rate to continue to increase indefinitely, but, with a standard error of 0.19, the estimated coefficient of $RPCR_{t-1}$ in equation (25) is not significantly greater than 1.0. Thus, it is safer to conclude merely that the estimated value of this coefficient indicates that the reported property crime rate is adjusting strongly to changes in the exogenous variables in the equation.

Continuing to examine equation (25), we find that the age-structure, inflation, and prosperity indices all have statistically significant positive coefficients as expected in the Biderman specification, although the age-structure coefficient is the least significant of the three. On the other hand, the coefficient for the police expenditures variable is strongly negative. This relationship holds in spite of the fact that the zero-order correlation of PE/POP_t and $RPCR_t$ is highly positive (0.98), a correlation which we explicate below by an equation for determining the police expenditures variable. Thus, controlling for the social forces in equation (25) which drive up the reported property crime rate, we find that the police expenditures variable has a net negative effect. Of course, before giving the coefficient of this variable much significance, it must be emphasized that this is the only index of criminal justice activity in the equation. If other indices of the criminal justice system could be explicitly entered into the equation, it might be the case that the size and level of significance of the coefficient of this variable would fall.

Beneath equation (25), we list several additional statistics which help us evaluate the performance of a dynamic macro social indicator equation. First, we report two \bar{R}^2 s, with the bars indicating that both are corrected for degrees of freedom because the sample on which the equation is esti-

mated is small. The second \bar{R}^2 has an A subscript to indicate that it is adjusted for the contribution of the constant to the explained variance of the dependent variable. In other words, this is just the usual \bar{R}^2 defined in terms of deviation scores as found in standard statistics texts. The first \bar{R}^2 is unadjusted for the constant, and thus it allows the size of the constant to contribute to the explained variance. Because of this, $\bar{R}^2 > \bar{R}_A^2$; for a formal definition of the two \bar{R}^2 s and a derivation of their properties, see Theil (1971, pp. 164, 176).

Although the unadjusted \bar{R}^2 is unconventional in most regression analyses of cross-sectional sociological surveys, a comparison of the two coefficients is a useful interpretive device in a dynamic context. In brief, if the dependent variable is reacting strongly to exogenous forces and thus is not near its equilibrium value, the two \bar{R}^2 s will be very close in value. On the other hand, if the dependent variable has adjusted to whatever exogenous forces are influencing it, so that it is at or near its equilibrium value, then the contribution of the constant term of the equation to determining the value of the dependent variable will be larger, the contribution of the lagged dependent variable will be smaller, and the values of the two \bar{R}^2 s may be substantially different.¹⁷ Examining the \bar{R}^2 s for equation (25), we find that the two are identical to the third decimal place; this indicates that the reported property crime rate variable is not near an equilibrium over the time period on which the equation is estimated. This interpretation is consistent with that of the estimated regression coefficients given above.

At this point, it is useful to add a comment on the sizes of the \bar{R}^2 s

¹⁷ As an example of a dynamic macro social indicator equation that is near its equilibrium, consider

$$\%MIGIS_t = 2.84 + 0.20\%MIGIS_{t-1} - 0.06UN_t + e_t,$$

(4.00) (0.98) (1.55)

$$\bar{R}^2 = .995, \bar{R}_A^2 = .075, SEE = 0.222, D-W = 2.271, df = 20,$$

where the variables are: $\%MIGIS_t$ = the percentage of households in year t that report having moved from one state to another within the last year, and UN_t = the annual average unemployment rate in year t . Although the usual $R_A^2 = .159$ is of the magnitude familiar to sociologists in a cross-sectional survey context, when adjusted for the degrees of freedom in this small sample, it is only .075. Moreover, because the equation is near its equilibrium value, we see that the dependent variable of the equation does not exhibit a strong time trend, as evidenced in the statistically insignificant value for the lagged dependent variable. Because of this, and because the computed least-squares residual is random relative to the equation (the Durbin-Watson statistic is large), we find that there is a large discrepancy between \bar{R}^2 and \bar{R}_A^2 . In other words, for an equation—such as this one—near the vicinity of its equilibrium, the contribution of the constant term to determining the value of the dependent variable is relatively much larger than in an equation that is adjusting over time to exogenous forces.

usually obtained in a time-series context. Accustomed as they are to cross-sectional survey analysis, sociologists typically expect an upper bound of about 0.5 in \bar{R}^2 s, and they are understandably skeptical of \bar{R}^2 s that approach 1.0 in value. Again, the sizes of the \bar{R}^2 s for a dynamic equation often depend upon the nearness of the equation to its equilibrium value. If the dependent variable is not near its equilibrium, this will reveal itself in a strong trend over time in the variable, in a statistically significant coefficient for the lagged dependent variable, and in high values (usually 0.95 or higher) for both the adjusted and unadjusted \bar{R}^2 s. However, if the dependent variable is close to its equilibrium value, then the variable will not exhibit a strong time trend, the regression coefficient for the lagged dependent variable will be small and possibly insignificant statistically, and the \bar{R}^2 s may be quite different. In the latter situation, the unadjusted \bar{R}^2 will usually be large, since it includes the contribution of the constant to explaining the variance of the dependent variable. By contrast, the size of \bar{R}_A^2 will depend on the ability of the independent variables in the equation to explain the variation of the dependent variable about its equilibrium value. This explanatory ability may be no greater than that of independent variables in a cross-section survey context, which means that \bar{R}_A^2 may be 0.5 or smaller.¹⁸ In brief, the large \bar{R}^2 s obtained in the estimation of equations from a time-series context are often a consequence of the fact that the dependent variables are governed by a dynamic equation and therefore exhibit a strong time trend. Frequently, this accounts for the large sizes of \bar{R}^2 s in dynamic macroeconomic models relative to those usually obtained by sociologists in a cross-sectional context. It is not that sociological models are puny in explanatory power compared to those of economists; rather, sociologists rarely are in a position to take advantage of the inflation of \bar{R}^2 s due to time-dependent equilibrating processes.

Three additional statistics given under equation (25) complete the interpretive material for a dynamic equation. First, the standard error of estimate (SEE) is the usual estimate of the standard deviation of the stochastic disturbance or error term of the equation. It can be used in the standard way to estimate the amount of inaccuracy in the prediction of the dependent variable of the equation. In equation (25), for example, the observed values of the reported property crime rate are within 100.5 per 100,000 population of the predicted values 95% of the time, under the assumption that the disturbances are normally distributed. Second, the Durbin-Watson statistic (D-W) is a measure of the extent of autocorrelation of the computed residuals of the equation.¹⁹ Although usually not

¹⁸ See the equation in the preceding note.

¹⁹ For a discussion of the Durbin-Watson statistic, see, e.g., Johnston (1972) or Theil (1971).

of interest in cross-sectional survey analyses, autocorrelation can be a serious problem in a time-series context for reasons indicated at the end of Section 4A. Thus, the D-W statistic provides another useful diagnostic for the performance of the specification of an equation. In particular, if the equation is far from its equilibrium value so that its dependent variable exhibits a strong time trend, and if the specification of the equation fails to include the lagged dependent variable, then the D-W statistic will likely be too low to accept the null hypothesis of zero autocorrelation among the disturbances. In this case, the D-W diagnostic may indicate the need for a respecification of the equation to include the lagged dependent variable. For equation (25), however, the D-W value of 1.95 is above the 1.74 value necessary to accept the null hypothesis of zero autocorrelation at the 1% level of significance for an equation with six independent variables (counting the constant).²⁰ A final statistic given under equation (25) is simply the degrees of freedom of the equation. Although again this is usually not of crucial importance for a survey analysis based on thousands of observations, it is clear that a dynamic equation estimated over a relatively short time series must be interpreted in the context of its degrees of freedom. For equation (25), there are 26 annual observations, 1947-72, less six estimated coefficients, yielding 20 degrees of freedom. Although not large, 20 degrees of freedom are comparable to those found, for example, in the pioneering Klein-Goldberger (1955) econometric model of the U.S. economy.

Although all of the standard statistical diagnostics look good for equation (25), it would be useful to have additional evidence that the estimated parameters of the equation are robust and not simply spurious consequences of the peculiarities of the data and the specification of the equation. One method of evaluating an equation estimated over a time series, to be illustrated below, is the use of the equation to forecast additional time points beyond those over which the parameters are estimated. Another method is the standard practice of re-estimating the equation under alternative specifications to determine how the original parameter estimates are affected.

As an illustration of the latter procedure for the reported property crime rate function, consider the alternative specification which follows; in addition to the hypotheses of the Biderman specification, it incorporates the standard sociological explanation of cyclical variation of this rate in terms of the effect of the unemployment rate on the opportunity structure for employment in normatively acceptable economic activities.

Reported property crime rate (2):

²⁰ Tables for the 1% and 5% levels of the D-W statistic can be found in any econometrics text.

$$\begin{aligned}
 \text{RPCR}_t = & -2172.24 + 1.27\text{RPCR}_{t-1} + 39.19\%M_t^{15-24} + 25.81\text{CPI}_t \\
 & \quad (3.54) \quad (5.66) \quad (1.17) \quad (2.81) \\
 & + 2492.46\text{AGNP/POP}_t + 14.87\text{UN}_t \\
 & \quad (2.16) \quad (1.02) \\
 & - 1196.50\text{PE/POP}_t + e_t, \\
 & \quad (4.14)
 \end{aligned} \tag{26}$$

$\bar{R}^2 = .9985$, $\bar{R}_A^2 = .9936$, $\text{SEE} = 51.23$, $\text{D-W} = 2.0$, $\text{df} = 19$, where the only new variable is: UN_t , the annual average national civilian unemployment rate in year t .

Examining the estimated coefficients of this alternative reported property crime rate equation, we find that almost all of them have the same algebraic signs, and approximately the same magnitudes and t -ratios, as in equation (25). The one exception is the age-structure variable which retains the same algebraic sign but loses statistical significance to a probability level of about .15. Similarly, the unemployment rate exhibits a coefficient with a theoretically expected sign that is statistically insignificant by the usual criteria. On the other hand, taken together, these two results accord well with sociological theory, on the basis of which one would expect an increase in the unemployment rate to have the greatest impact on the job opportunity structure of young males. The other statistical indices provide no clear-cut case for preferring equation (26) to (25). In brief, there is no increase in \bar{R}_A^2 , and the Durbin-Watson statistics are very close. Thus, if a case is to be made for retaining equation (26), it must be made on the basis of the theoretical significance of the unemployment variable rather than its statistical performance. Alternatively, a decision could be based on forecasting performance, which we shall examine below.

Continuing to specify equations for public safety social indicators, we next examine the specification of an equation for determining trends and cyclical variations in the national reported violent crime rate. According to Biderman's (1966) analysis, many of the social forces which determine the reported violent crime rate influence the reported property crime rate as well. Thus, one specification of a reported violent crime rate function would include most of the variables in, say, equation (26). If, in addition, the reported property crime rate is allowed to have a direct effect on the reported violent crime rate on the hypothesis that some persons involved in violent crimes are also involved in property crimes, then the following equation is obtained for the 1947-72 period:

$$\begin{aligned}
 & \text{Reported violent crime rate (1):} \\
 \text{RVCR}_t = & 28.90 + 0.79\text{RVCR}_{t-1} + 0.09\text{RPCR}_t \\
 & \quad (0.36) \quad (11.61) \quad (6.64) \\
 & - 3.26\%M_t^{15-24} - 0.43\text{UN}_t \\
 & \quad (1.07) \quad (0.42) \\
 & - 0.08\text{CPI}_t - 41.84\text{PE/POP}_t + e_t, \\
 & \quad (0.08) \quad (1.73)
 \end{aligned} \tag{27}$$

$$\bar{R}^2 = .9995, \bar{R}_A^2 = .9971, \text{SEE} = 4.59, \text{D-W} = 2.30, \text{df} = 19,$$

where the only new variable is $RVCR_t$, the national reported violent crime rate, expressed as the total number of murders and nonnegligent manslaughters, forcible rapes, robberies, and aggravated assaults reported in year t per 100,000 midyear population. The theoretical reasoning for each of the regressors in this equation is much the same as that for the reported property crime rate. The only exception is that now the inflation rate indexes whatever ill effects inflation has on the tranquility of social life. Moreover, equation (27) does not include the GNP variable because of the lack of a theoretical interpretation in the violent crime context. On the basis of the estimated coefficients, we see that the net impacts of the unemployment and inflation rates and of the age-structure index are negligible. Thus, we conclude that if those social forces driving up the reported property crime rate are allowed to affect the reported violent crime rate indirectly through their effect on the former, then they do not have a net direct effect on the latter.

On the basis of this reasoning, we reformulate and re-estimate to obtain the following equation:

Reported violent crime rate (2):

$$RVCR_t = 3.90 + 0.77RVCR_{t-1} + 0.085RPCR_t - 39.17PE/POP_t + e_t \quad (28)$$

(1.28) (17.07) (10.78)
(5.50)

$$\bar{R}^2 = .9996, \bar{R}_A^2 = .9972, SEE = 4.52, D-W = 2.06, df = 22.$$

This version of the reported crime rate function has the virtue of performing well with only three regressors. The Durbin-Watson statistic indicates that the null hypothesis of zero autocorrelation of the disturbances need not be rejected. Substantively, the equation indicates that about 8.5% of the reported property crime rate translates into reported violent crime. With a lag coefficient of about 0.8 and recent reported property crime and police expenditure rates in the region of 3,000 and 3.0 per 100,000 population unit, respectively, we can use case 5 of table 2 to find that this equation implies an equilibrium value for the reported violent crime rate in the neighborhood of 575 per 100,000 population. This accords well with recent observations of about 500 reported violent crimes per 100,000. In contrast to the estimated reported property crime rate equations, we observe that equation (28) has a constant term that is positive and of marginal statistical significance. This means that the equation will generate a rate of reported violent crime approaching zero as the predetermined variables driving the rate decrease toward zero. Just as in the case of the reported property crime rate equation, however, we find that the rate of public police expenditures has a negative net effect on the reported violent crime rate. Although previous sociological research has found a null relationship between measures of police activities and such violent crimes as

murder and assault (see, e.g., Sharkansky 1968; Swimmer 1974), it should be noted that the reported "violent" crime rate of this equation includes robberies (which constitute about a third of the total rate) and that previous research has found a negative impact of police activity indices on reported robberies. In brief, to the extent that public police expenditures have a net suppressive effect on the reported "violent" crime rate, this most likely is reflected in the reported robberies component of the rate.

As a third topic to be addressed in the public safety area, consider the issue of what effect, if any, the trends in reported crime rates have on the trend in public police expenditures. There is little guidance in sociological theory for the specification of an equation for this social indicator. However, since public appropriations for police agencies are usually made on an annual basis, it is reasonable to expect that in year t the allocating authorities would react, if at all, to the most recently available reported crime rates, namely, those for year $t - 1$. Moreover, the rate of inflation clearly must be controlled for, and the rate of increase in police expenditures may depend on the total level of wealth in the society. This reasoning leads to the following specification:

Police expenditures (1):

$$\begin{aligned} \text{PE/POP}_t = & -1.73 + 0.00034\text{RPCR}_{t-1} + 0.00078\text{RVCR}_{t-1} \\ & (6.37) \quad (1.47) \quad (0.78) \\ & + 0.027\text{CPI}_t + 0.097\text{AGNP/POP}_t + e_t, \\ & (5.60) \quad (0.11) \end{aligned} \quad (29)$$

$$\bar{R}^2 = .9989, \bar{R}_A^2 = .9951, \text{SEE} = 0.048, \text{D-W} = 0.99, \text{df} = 21,$$

where all variables have been defined previously. It is readily seen from this equation that the trend in police expenditures is dominated by the trend in the reported property crime rate and the rate of inflation. This leads to the following alternative version of the police expenditures function:

Police expenditures (2):

$$\begin{aligned} \text{PE/POP}_t = & -1.57 + 0.00049\text{RPCR}_{t-1} + 0.025\text{CPI}_t + e_t, \\ & (9.13) \quad (7.02) \quad (9.31) \end{aligned} \quad (30)$$

$$\bar{R}^2 = .9989, \bar{R}_A^2 = .9953, \text{SEE} = 0.047, \text{D-W} = 0.98, \text{df} = 23.$$

The t -ratios and \bar{R}^2 s indicate that this equation performs adequately. Although the Durbin-Watson statistic does not allow acceptance of the null hypothesis of zero autocorrelation of the disturbances, it is not small enough to force rejection of that hypothesis.

This completes our specification and estimation of a dynamic macro social indicator model of three standard social indicators in the area of public safety. Although these equations constitute an increment in socio-

logical knowledge of the interrelationships of trends and cyclical fluctuations in these social indicators, they are relatively crude at this stage and can probably be improved upon by disaggregation and respecification with additional variables. We are currently engaged in such modifications. However, even in their present crude state of development, they combine into an interesting negative feedback system, as illustrated in figure 1.

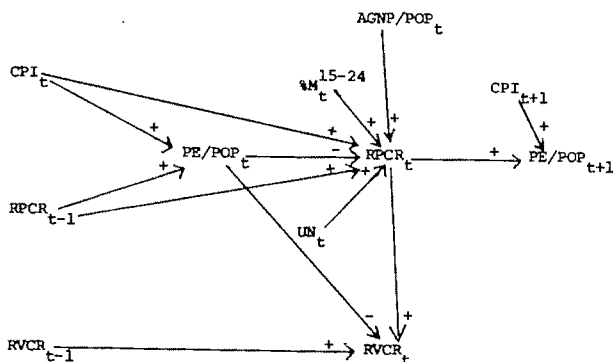


FIG. 1.—Flow diagram for a dynamic structural-equation model of reported crime rates and police expenditures (the notation is defined in equations [24]–[30] in the text).

Briefly, the diagram shows the positive reaction of police expenditures in year t to the reported violent crime rate in year $t - 1$, the negative net effects of police expenditures in year t on the reported property and violent crime rates in year t , the positive net direct and indirect effects of the exogenous economic and demographic forces on the reported property and violent crime rates in year t , the adjustment of the crime rates in year t relative to the exogenous forces and their previous values, the positive effect of the reported property crime rate in year t on the rate of police expenditures for year $t + 1$, and so forth through additional periods. Although a modest application of the sociological imagination could lead to the postulation of the reported property crime rate as a homeostatic variable and the consequent interpretation of these equations in terms of functional sociological theory, we shall refrain from such an interpretation at this point. Suffice it to say that, in spite of the popularity of functional theory in sociology (see, e.g., Parsons 1951, 1966; Merton 1957; Stinchcombe 1968), almost no examples exist of quantitatively estimated homeostatic social systems. If societies have any variables that qualify as homeostatic, acts considered as criminal may be prime candidates. If so, the public safety equations presented here are virtually unique quantitative contributions to the literature on homeostatic social systems.

C. Forecasting with a Dynamic Macro Social Indicator Model

As noted in the introduction to this paper, one of Ogburn's empirical interests in studying social change was to estimate the probable consequences of current changes for the future. In other words, Ogburn was concerned with the art of social forecasting. While social forecasting is not ordinarily a concern in cross-sectional survey research, it becomes a natural outgrowth of the time-series estimation of a macro social indicator model. But, not only are substantive forecasts feasible in the context of a dynamic model, they also provide a statistical means for further evaluating the model's adequacy.²¹ In brief, one can further determine the performance of a model by comparing observed values of its dependent variables with those predicted by the estimated equations for time points beyond those used for fitting the model. If the estimated coefficients are statistical approximations to stable structural parameters and not just randomly determined by the peculiar set of equation specifications and observations on which they are based, then forecasts of subsequent time points of the dependent variables, conditional on observed values of the exogenous and predetermined variables, should be relatively accurate. That is, in the absence of true structural change (which can never be completely discounted), a conditional forecast is another means of determining the impact of possible autocorrelation, misspecification, and simultaneity bias on the obtained parameter estimates.

Since we estimated the equations of our public safety model on the years 1947-72, and since the final observed values of the endogenous variables are now available for 1973, we can apply this comparison procedure to the model for 1973. Table 3 reports the observed values, forecasted values, and errors for the three endogenous variables. The forecast values were derived by giving the exogenous and predetermined variables in our model their values for 1973 and then computing the conditional expected values of the dependent variables. As can be seen from the table, the values of the reported property crime rate forecasted from both versions of the property crime rate equation are less than the observed value, although the second version does slightly better than the first. Similarly, the forecasted police expenditures value is less than the observed value. By contrast, the forecasted reported violent crime rate is almost equal to the observed value.

To make comparisons of observed values and conditional forecasts statistically more precise, the necessary formulas for standard errors of forecasts, *t*-ratios, and confidence intervals have been derived (see, e.g.,

²¹ For a discussion of the uses of forecasting to evaluate a dynamic structural-equation model, see, e.g., Christ (1966, pp. 543-78). Of course, many procedures for evaluating a model by forecasting and simulation exist besides that which we illustrate here.

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TABLE 3

CONDITIONAL FORECASTS OF THE 1973 VALUES OF THE REPORTED NATIONAL
CRIME RATES AND POLICE EXPENDITURES

VARIABLE	1973		
	Observed	Forecast*	Error
RPCR	2824.3	(1) 2739.4 (2) 2750.4	84.9 73.9
RVCR	414.0	(2) 414.1	-0.1
PE/POP	3.48	(2) 3.01	0.47

* The numbers in parentheses beside the forecasts indicated the version of the equations used to make the forecasts.

Goldberger 1964, pp. 168-71; Johnston 1972, pp. 152-55; Theil 1971, pp. 134-36). These statistics can be used to test the hypothesis that observations subsequent to those in the estimation period are generated by the same causal structure as that of the estimation period. Although such formulas could be applied in the present case, with respect to the reported crime rate equations, the computations are unnecessary for anything other than a textbook exercise because the standard errors of forecast are always greater than or equal to the standard errors of estimate of the equations, and the latter show that the forecasts are relatively accurate. Examining the forecast errors of table 3 relative to the standard errors of the equations, we find that the forecasted values of the property crime rate are within 1.66 and 1.44 standard errors of estimate of the observed rate. Thus, even though the second version of the equation includes the marginally significant unemployment rate, it performs slightly better in the 1973 forecast than does the first version. Moreover, the observed value of the reported violent crime rate is well within the standard error of estimate of the forecasted value. Thus, we conclude that the structures given in equations (25), (26), and (28) could have generated the 1973 reported property and violent crime rates. By contrast, the 1973 forecast error for the rate of police expenditures is 10 times the standard error of estimate of equation (30). This is consistent with the statistical results given above which show that this is the least well specified of the three equations. In brief, public police expenditures are regulated by public finance mechanisms (see, e.g., McMahon 1971; Musgrave 1969; Musgrave and Peacock 1958), and we have not incorporated any political or public finance considerations into the specification of our police expenditures equation. Apart from specification errors, it should be noted that the 1973 police expenditures rate seems subject to a larger than usual increment. In brief, in spite of the fact that the 1972 reported property crime rate shows a slight decline from the 1971 rate (which, in our equation, leads to a

smaller forecasted rate for 1973, other things being equal), the 1973 police expenditures rate is nearly 20% higher than in 1972. That this rate of increase is somewhat unique is confirmed by the fact that the 1974 forecasted rate is much closer to the 1974 preliminary observed rate.

5. CONCLUDING COMMENTS

In this paper, we have described: (1) an opportunity-structures theoretical perspective for generating specifications of dynamic macro social indicator models and for bringing many diverse sociological theories to bear on these specifications; (2) a demographic accounting framework for maintaining consistency among the equations of such models and for making clear the distinction between those social indicators corresponding to stocks and those corresponding to flows; and (3) a structural-equation strategy for building macro social indicator models. We believe that our initial results in the area of public safety show that this type of work is viable and potentially fruitful even in the present crude state of affairs with respect to the collection of crime statistics. Although some sociologists may rebel at the idea of determining such aggregate social indicators as we have dealt with here, we feel that this is important for at least three reasons. First, such indicators are indeed the subject of attention by observers of the society and one of the few systematic sources of information about social life. Second, as we have shown in this paper, these indicators are not subject only to random variation from year to year; rather, large portions of their variances are systematically related to other indices of social and economic conditions. Third, social indicators, such as those analyzed here, are important indices of real social changes. For example, an increase in the marriage rate indexes a change in the social relationships of thousands of men and women in the society. Of course, many sociological theories of social change are concerned with variables and phenomena for which time-series data are not likely to be immediately available, but this should not prevent sociologists from dealing with the data that are available.

In brief, we are advocating that the cross-sectional survey analyses dominant in contemporary empirical sociology be supplemented with time-series analysis of macro social indicators such as those we have illustrated here. Of course, we realize that our approach to construction of social indicator models can be dismissed as "merely" the importation to sociology of macroeconomic time-series modeling. To such criticisms, we respond simply that the spirit, if not the detail, of modern dynamic structural-equation model-building procedures was anticipated in the work of Ogburn over 50 years ago before such procedures were labeled "econometric." If he were studying social change today, we believe that Ogburn likely

would be utilizing some of the methods of analysis illustrated here, although probably with more sophistication than we have mustered.

APPENDIX A

How to Construct a Demographic Account

Stone (1971, pp. 17-22) notes that there are several ways of setting up a demographic accounting framework, depending on various contingencies. One contingency concerns whether the accounts are kept for the normal residents of a country (the national concept) or for the actual residents (the domestic concept). Normal residents are people who consider the country as their home country, whereas actual residents are people who happen to be living there at a particular time. A second contingency pertains to how migrants (persons permanently displaced from one country to another) and visitors (persons temporarily displaced from one country to another) are to be handled. Third, since demographic accounts link the opening stock of population at the beginning of a period (the survivors from the preceding period) with the closing stock at the end of the period (the survivors into the following period), the time interval of the period is a contingency. Usually, the time interval is a year. A fourth contingency is whether we consider the changes of social state taking place in a particular year and experienced by the people who flow into that year (a cross-sectional account) or the changes of social state occurring through a succession of years and experienced by the people born in one year (a cohort account).

TABLE A1
DEMOGRAPHIC ACCOUNTS FOR ACTUAL RESIDENTS WITH MIGRANTS AND VISITORS
SHOWN NET: UNITED STATES, 1973

Inflows		Outflows	
Survivors from 1972	207,296	Deaths in 1973	1,977
Births in 1973	3,141	Survivors to 1974	208,921
Net immigrants and visitors in 1973	461		
Total	210,898	Total	210,898

SOURCE.—Approximated from *Current Population Reports*.
NOTE.—All numbers in thousands.

Table A1 presents the cross-sectional demographic accounts for the actual residents of the United States in 1973, taking migrants and visitors as net. These accounts are aggregate in the sense that none of the residents are classified by social state. Note that the totals balance on both the right

and left sides of table A1, at 210,898,000 people. Inflows to the 1973 population include survivors from 1972 and births during 1973, as well as the net immigrants and visitors in 1973. Outflows include survivors to 1974 and deaths during 1973. The number of net immigrants and visitors can be negative or positive, depending on whether more flow out than in or vice versa.

These cross-sectional accounts can be further classified by age or any social state or activity for which data are available or estimable, including family, education, health, economic, or crime-related phenomena. An example of such classification is presented in table A2, where the population

TABLE A2

DEMOGRAPHIC ACCOUNTS FOR ACTUAL RESIDENTS CLASSIFIED BY AGE AND LABOR FORCE STATUS WITH MIGRANTS AND VISITORS SHOWN NET: UNITED STATES, 1973

INFLOWS		OUTFLOWS	
Age 0-14			
Survivors from 1972	56,207 ^a	Deaths in 1973	82 ^a
Births in 1973	3,141	Survivors to 1974	59,395
Net immigrants and visitors in 1973	129		
Total	59,477	Total	59,477
Age 15-64			
Survivors from 1972:		Deaths in 1973	636 ^a
In labor force	82,895 ^b	Survivors to 1974:	
Not in labor force	47,090	In labor force	86,807 ^c
Net immigrants and visitors in 1973	238	Not in labor force	42,780
Total	130,223	Total	130,223
Age 65+			
Survivors from 1972:		Deaths in 1973	1,259 ^a
In labor force	2,823 ^b	Survivors to 1974:	
Not in labor force	18,281	In labor force	2,288 ^c
Net immigrants and visitors in 1973	94	Not in labor force	17,651
Total	21,198	Total	21,198

SOURCES.—^a*Monthly Vital Statistics*, various issues in years mentioned; ^b*Employment and Earnings*, February 1974; ^cestimated from data in *Employment and Earnings*, February 1974, and from Census of Population, 1970; all other data are estimated from the *Current Population Report*, Series P-25, 1972, 1973, and 1974.

NOTE.—All numbers in thousands.

stocks and flows are broken down by age and labor-force participation. The selection of age intervals specified three groups of people: those too young to participate (ages 0-14), those in the ages where retirement rates are high (65+), and those in the prime years of economic activity (15-64). The age categories could be further broken down as appropriate for

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analytical purposes, even to single-year intervals or less, providing that data can be found. These classifications could be broken down much further if necessary, for instance, by sex or race or both, or by number of hours or weeks worked.

Even after the researcher has decided which type of demographic account is desirable, data problems are often found. In many cases, major social indicators are not classified by single year of age, so that estimation procedures are needed in order to arrive at detailed demographic accounts. In other cases, data must be matched from different government agencies using slightly different definitions of concepts or nonmatching age intervals. In some cases, classification systems change from year to year. Demographers are well aware of these problems and are accustomed to employing estimation procedures, such as indirect standardization, to solve them.

In general, these tables show that the demographic accounting is little different from any other procedure which classifies people in tabular or matrix form, except that inflows and outflows are clearly distinguished.

APPENDIX B

SOURCES FOR TIME-SERIES DATA: UNITED STATES, 1947-74

Variables	Sources
UN, AGNP	<i>Manpower Report of the President</i> , U.S. Department of Labor, 1975.
POP, M ¹⁵⁻²⁴	<i>Current Population Reports</i> , Series P-25, U.S. Bureau of the Census, annual.
RPCR, RVCR	<i>Social Indicators</i> , 1973, Executive Office of the President, Office of Management and the Budget.
CPI	<i>Handbook of Labor Statistics</i> , U.S. Department of Labor, 1974.
PE	<i>Historical Statistics of the United States</i> , U.S. Bureau of the Census, 1960; and <i>Expenditures and Employment for the Criminal Justice System</i> , U.S. Law Enforcement Assistance Administration, 1974; <i>Governmental Finances and Public Employment</i> , U.S. Bureau of the Census, annual.

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Crime Rates of American Cities in an Ecological Context

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Given a city that contains only a small proportion of the residents in the larger ecological community, the conventional crime rate for that city could be high merely because the denominator of the rate underestimates the potential number of victims or offenders. Accordingly, there is a basis for anticipating a direct relationship among cities between (1) community/city population size ratios and (2) rates for particular types of crimes. The relationship does hold for many American cities when Urbanized Areas (UAs) or Standard Metropolitan Statistical Areas (SMSAs) are taken as approximations of communities; but it holds only for singular cities, each of which is the only central city in a SMSA. The argument of this paper is that singular cities are much more homogeneous as regards dominance within the community than are other types of cities, and that dominance determines the extent to which a city will attract nonresident participants in crimes. In any case, the findings cast doubts on the use of conventional crime rates for cities in testing theories.

The reliability of official crime rates has been a major issue in criminology for decades, and the issue has become more acute since the emergence of the labeling perspective in studies of deviance. If the extreme version of that perspective is accepted, all official crime rates are reliable; the argument being that an act, event, or condition is a crime if and only if "so labeled" by officials. The counterargument is that some crimes go undetected or unreported, and all surveys of criminal incidence (self-reports or victim reported) are based on that counterargument.

There is no immediate prospect for resolution of the issue if only because the notion of a reliable rate is ambiguous, especially when considered in conjunction with the notion of validity.¹ In any case, there are problems pertaining to crime rates that transcend the issue of reliability, and those problems have been slighted, evidently because of the preoccupation with the labeling perspective. The reliability issue focuses on the numerator of the crime rate (the number of crimes); but there are some serious questions about the denominator, and those questions are not limited to the use of census data.

¹ A truly valid but absolutely unreliable crime rate would be logically impossible; and, in any case, there is no obvious procedure for demonstrating the extent to which a given crime rate (official or unofficial) is either reliable or valid.

THE CONVENTIONAL CRIME RATE

The formula for the conventional crime rate is C/P , where C is the number of crimes allegedly committed in some context (e.g., a city) during some specified period (e.g., one or more particular years) and P is the number of potential offenders and/or victims in that context during the period. When the context is a territorial unit, P is usually the number of residents at some point during the period. The conventional crime rate has been employed in research so often that only rarely do criminologists confront this question: Why is P the denominator? The conventional answer is: P represents the number of potential offenders or victims, which varies from one context to another; hence, the division of C by P ostensibly controls for variation in that number. Without such control, the incidence of crime is virtually certain to be greater for California than for, say, Wyoming.

The argument is defensible as far as it goes, but for virtually any territorial unit C is not limited to crimes committed by residents. Instead, C is the number of crimes (commonly those of some particular type) allegedly committed within a territorial unit (e.g., within the official city limits). Yet the number of potential offenders or victims does not necessarily equal the residents of the territorial unit; indeed, the number is unknowable and seemingly infinite. So there is a logical discrepancy between C and P in the conventional crime rate, meaning that P probably never represents what it purports to represent—the number of potential offenders or victims—and the problem has nothing to do with the reliability of the C figure.

One alternative is an unconventional crime rate: C_r/P , where C_r is the number of crimes committed by residents and P is the number of residents. However, not even that "residential rate" (Gibbs 1975) would solve the problem satisfactorily, because the number of residents as of some census date (e.g., April 1, 1970) does not represent the time spent in the unit by all residents (including recent immigrants and emigrants) who were physically present at some point during the period (e.g., throughout 1970) and who therefore had an opportunity to commit or be victimized by a crime. With that consideration in mind, the most defensible unconventional crime rate would be: C/T_t , where T_t is the total time (e.g., hours) during which all individuals were physically present in the territorial unit during the period.

Practical considerations alone preclude either of the foregoing unconventional rates. Official figures on the incidence of crime are compiled largely by time and place, the reason being that the police frequently cannot identify perpetrators, let alone their places of residence. Moreover, even if C_r could be determined, there are no official figures (e.g., census data) that even purportedly correspond to T_t .

IMPLICATIONS

Since the foregoing problem concerning the conventional official crime rate appears insoluble, investigators may elect to ignore it and assume that the "error" in P is, proportionately speaking, approximately the same for all territorial units. However, given contrasts in the "ecological position" of cities, the assumption is dubious when one compares crime rates of cities.

Virtually any city is surrounded by territory whose residents regularly work in or use the facilities of the city, and those residents are potential participants in crimes that occur within the city either as offenders or victims. Such surrounding territory is a part of a community which also includes the city, and the notion of a community introduces a crucial consideration. There is no basis whatever for assuming that the ratio of *community* residents (including those in the city) to *city* residents is even approximately constant from one city to another. Contrasts in the spatial position of cities are alone sufficient for inferring considerable variation in the "community/city" population ratio. Suppose there are 5,000 residents in each of two cities, A and B . Now suppose A is five miles from a city of 1 million, while B is located 60 miles from a city larger than itself. Other things being equal, B is likely to be the center of a much larger community than is the case for A . That is so because B is farther removed from the competition of a larger city, and hence its community boundary is likely to be more extensive (Haggett 1967).

The community/city population ratio is relevant because there is a rationale for anticipating that the conventional crime rate for a city varies directly with the ratio. The argument is simple: as the ratio increases, the number of potential participants in a "city crime" (that is, a crime in the city) increases proportionately. Thus, other things being equal, if the community/city population ratio is 3.0 for one city but only 1.5 for another, the crime rate of the former is expected to be greater than that of the latter. The rate of the former would not necessarily be twice that of the latter or even approximately so, which is to say that the exact *form* of the relationship between the community/city size ratio and the city crime rate must be arrived at inductively rather than deduced. Nonetheless, the argument can be stated as a theoretical proposition: the crime rate of a city varies directly with the ratio of the size of the larger community to the size of the city.

SOME MAJOR PROBLEMS

The foregoing proposition is designated as "theoretical" not only because it is untestable (as stated) but also because of some crucial qualifications. As for testability, the notion of a community is so vague that sociologists

do not agree in their definitions, let alone as to a defensible delimitation procedure (see Bell and Newby 1972). Moreover, even given an acceptable delimitation procedure, enormous resources would be required to apply it. Few if any census agencies delimit community boundaries; hence, no test of the proposition could be based on census data for communities, at least not for communities strictly construed. Nonetheless, some census agencies do delimit territorial units *around* large cities, and those units have some relation to conventional definitions of a community. In the United States the Bureau of the Census delimits two such types of territorial units: Urbanized Areas (UAs) and Standard Metropolitan Statistical Areas (SMSAs). Neither type corresponds exactly to a conventional definition of a community, but a SMSA or UA is surely a rough approximation of an ecological community. So there is justification for basing tests of the proposition on either UAs or SMSAs.² Divergent results (e.g., a closer relation in the case of UAs) could be instructive as to how communities should be defined and delimited with a view to crucial tests of the proposition.³ For the time being, however, the proposition is to be understood as applying to either urban areas or metropolitan areas as approximations of communities, whatever the census designation of such territorial units in a particular country.

The principle of limits.—Even if population figures on communities were readily available, the proposition is contrary to what should be taken as a principle for sociological theories—that the form and degree of association between any two variables are not uniform for an *infinite* range of values. To take extreme examples from among cities of 50,000 or more residents in the United States, the ratio of the UA population to the city population for Beaumont, Texas, is 1.00, while the corresponding ratio for Bloomfield, New Jersey (part of the New York–Northeastern New Jersey Urbanized Area), is 311.67. Surely it would tax credulity to interpret the proposition as asserting that Bloomfield's crime rate is some 300 times the rate for Beaumont. Even if the relationship asserted in the proposition is monotonic, it may not be linear for *all* values of the community/city population ratio; and it could be that beyond some value there is simply no relationship (linear or nonlinear).

Extreme values can be avoided by limiting a test of the proposition to "singular" cities, each of which is the only central city in the corresponding

² The idea is not that tests of the proposition must be limited to the United States. Census agencies in some other countries delimit urban areas or metropolitan areas (e.g., conurbations in Great Britain, agglomerations in France).

³ The point is especially important if one entertains the idea that some cities are so large that they comprise all or part of several communities. If a conceptualization of a community admits that possibility, some units considered in tests may actually be "composite" communities, meaning that each of them includes two or more communities that contain part of a particular large city.

UA and SMSA.⁴ Although the UA/city and SMSA/city ratios vary considerably among such central cities, the ratios for them do not remotely approach that for an extreme case like Bloomfield, New Jersey.

The principle of conditionality.—The principle of limits is not the only problem in contemplating tests of the proposition. Additionally, there is the “principle of conditionality,” which reduces to the assertion that, regardless of the range of values considered, the relationship between any two sociological variables is neither close nor invariant in all conditions (not even approximately so). Of the two principles, that of conditionality poses far more problems, at least as regards the proposition in question. That is so because there are reasons for anticipating that the relationship between the community/city population ratio and crime rates is contingent on several conditions.

One possibly relevant condition is the number of cities in a SMSA (or UA). In the case of multiple central cities, each one competes with the others in attracting participants in crimes. To illustrate, consider two SMSAs, each with a million residents; and suppose that in one SMSA there are three central cities, with a ratio of the SMSA population to the three central cities being 2.5 for the largest city (400,000 residents), 4.0 for the second largest, and 4.5 for the third largest. Now suppose that there is only one central city in the other SMSA, with the SMSA/central city ratio being 2.5 (i.e., the central city contains 400,000 residents). As such, in each SMSA there are 600,000 individuals who could be nonresident perpetrators or victims of a crime in the largest (or the only central) city; but in the SMSA with multiple central cities, the chances of a crime in the largest central city are less because there are two alternatives (i.e., two other central cities). The point takes on special significance with respect to robberies of or thefts from business establishments, for a disproportionate number of business establishments are found in cities. Viewed that way, given a SMSA with only one central city, that city is more likely to present opportunities for crimes (at least for certain types of crimes) than are any of the multiple cities in another SMSA. So matters are simplified if there is only one central city in a SMSA or UA, and hence the most positive results in tests of the proposition are expected for such cases.

To state the problem more analytically, there is no justifiable basis for assuming that the dominance of a city in a community is simply an inverse function of the community/city population ratio. If the ratio is unity (1.00), then the city is clearly dominant, for the ratio would indicate that the community and the city are the same. However, to the extent that the ratio is greater than 1.00, the dominance of the city is problematical. Thus,

⁴ A central city of a SMSA or UA is (1) the largest city, (2) a smaller city with at least 250,000 residents, or (3) a smaller city with at least 25,000 residents that is at least one-third the size of the largest city.

if the community/city population ratio is 2.0 for each of two cities in different communities, it could be that in one community there are several large cities, none of which are clearly dominant, while in the other there is only one city, and it is dominant by virtue of freedom from competition with other cities in the community.

Dominance is independent of (within broad limits) but no less important than the community/city population ratio. The ratio indicates the relative number of potential nonresident participants in a crime within the limits of a city; but whether the city will attract those potential participants is contingent on the dominance of the city, meaning the extent to which it offers unique services and employment opportunities in the community. Dominance cannot be defined more precisely, and there is no conventional or acceptable procedure for the numerical representation of it, let alone one that applies to census data. Indeed, virtually no progress has been made along that line since Bogue's well-known study (1949)', and it is doubtful that a direct measure of dominance can be formulated. Nonetheless, rough distinctions can be made (albeit inferentially) as to the probable dominance of certain types of central cities in SMSAs. Specifically, there is justification for assuming that dominance is more constant among singular cities than for any other type of central city, because they are much more free of competition with other cities in the SMSA. So singular cities are especially strategic for tests of the proposition, not only because their population ratios (community/city) have a fairly limited range but also because there is reason to believe that such cities do not differ a great deal as to relative dominance.

Additional contingencies.—Even if tests of the proposition were restricted to singular cities, there are several reasons for not anticipating a truly close relationship between the community/city population ratio and crime rates. Obviously, the notion of "opportunities" for crime cannot be justifiably limited to population alone, that is, to the sheer number of residents. For some crimes, theft in particular, the number of business establishments or automobiles is also relevant (see Boggs [1965] for an illustrative treatment of such opportunities).⁵ Most important of all, if any of several well-known theories about the "etiology" of crime are valid (e.g., Merton 1957; Sutherland and Cressey 1970), the crime rate reflects more than simply opportunities (however conceived or measured).

Just as there are reasons for anticipating that the relationship asserted

⁵ Similarly, the argument does not deny the importance of standardizing or adjusting crime rates (e.g., limiting the denominator of the rate for rape to females over, say, 10 years of age). However, no such standardization or adjustment takes into account nonresident participants in city crimes.

in the proposition is not truly close for any type of crime, so there are reasons for anticipating that the magnitude of the relation is contingent on type of crime. To illustrate, suppose that for a particular type of crime in 95% of all cases both the victim and the offender resided in the city where the crime occurred. If so, the expectation would be a negligible relationship among cities between the community/city population ratio and the rate for the type of crime, because the coresident figure (95%) indicates that potential participants who live outside the city are rarely actual participants *in* the city. That expectation cannot be assessed systematically, because research findings on the relationship between the place of crime and the residence of participants are limited to particular cities, to a few types of crimes, and largely to residence of offenders. Moreover, the findings pertain commonly to distance between place of residence (of offender and/or victim) and place of crime, a combination of factors which is at best indicative only of the proportion of cases in which all of those places are within the same city. Nonetheless, fragmentary though they are, the findings indicate that the proportion of cases in which the offender and/or the victim resided in the city where the crime occurred is greater for crimes against persons than for ones against property (see, e.g., President's Commission 1967, pp. 60-76; White 1932). So on that basis, a closer relation between the community/city population ratio and crime rates is expected for property crimes.

Up to this point, attention has focused on reasons for not anticipating a truly close relationship to be shown by tests of the proposition. However, even if a truly close relationship should be found in all tests (regardless of the type of crime or other conditions previously considered), the interpretation of the results would be disputable. In particular, the relationship could reflect the demographic characteristics of cities and not the relative number of potential participants in crime living outside the city. The most obvious argument can be made when UAs are treated as approximations of communities. If the central city of an UA is "underbounded" (i.e., much less extensive than the UA), that condition indicates that the city boundary ceased to expand with the horizontal growth of the UA; and, given known differentials between suburban and "core" populations, the central city is likely to be inhabited by a disproportionate number of nonwhites, low-income families, and/or unemployed persons. Since arrest rates and victimization rates for such populations tend to be relatively high, a high crime rate for underbounded cities would be consistent with the proposition, but only because of the residential correlates of stratification. The argument is surely plausible, and fortunately it can be assessed by controlling for income, unemployment, and racial composition when testing the proposition.

CITIES AND DATA

Tests of the proposition are based on data for 372 cities in the United States that had a population of at least 50,000 residents in 1970 (U.S. Bureau of the Census 1972a). Official figures on the annual incidence of eight types of crimes⁶ are reported by the FBI for cities of 25,000 or more inhabitants, but 50,000 was taken as the minimum limit because cities of that size are necessarily parts of a SMSA and an UA (though they may not be central cities). Unless a city is part of a SMSA and an UA, one cannot compute an approximation of the community/city size ratio, the approximation being either the UA/city ratio or the SMSA/city ratio.

The cities do not include all cities of 50,000 or more inhabitants as of 1970, because the requisite data for all cities in that size range are not reported. For some of them, the official incidence of crimes by type (both 1970 and 1971) is not tabulated in the *Uniform Crime Reports* (Federal Bureau of Investigation 1971, 1972); for others, the tabulations are incomplete. Nonetheless, the 372 cities constitute over 90% of all cities in the size range 50,000 or greater (as of 1970).

FINDINGS

Table 1 summarizes all findings that bear directly on the major proposition, which anticipates a direct relationship between community/city size ratios and city crime rates. The community/city size ratio is approximated by each of the two alternatives: (1) the UA/city ratio and (2) the SMSA/city ratio.

Two central sets of correlation coefficients.—Of the correlation coefficients (r 's) in table 1, only those in rows 1 and 6 are central for a test of the proposition in question. Those two sets are central because they pertain to singular cities; and, for reasons indicated earlier, only findings for that type of city are expected to be consistent with the proposition.

All of the 16 correlation coefficients (one for each type of crime in both row 1 and row 6) are in the direction anticipated by the proposition. Since the 180 singular cities cannot be construed as a random sample, the importance of the level of significance ($P < .01$ for each of the 16 coefficients) is debatable. Nonetheless, one would be hard pressed to identify any variable that is more highly correlated (among cities) with as many types of crime rates as are the community/city population ratios (see Ogburn 1935; Schuessler 1962).

⁶ The eight types are listed in table 1. Criminal homicide excludes "manslaughter by negligence," because a large number of cities did not report any cases for either 1970 or 1971. The rates are average annual incidence per 100,000 population for those two years. Figures for 1969 were not included because the minimum size limit for cities pertains to the population as of the 1970 census date.

TABLE 1
CORRELATIONS (r's) BETWEEN APPROXIMATIONS OF THE COMMUNITY/CITY SIZE RATIO AND CRIME RATES FOR
FOUR TYPES OF UNITED STATES CITIES, 1970-71^a

AVERAGE ANNUAL CRIME RATES PER 100,000 POPULATION, 1970-71										
APPROXIMATIONS OF THE COMMUNITY/CITY POPULATION SIZE RATIO ^b AND TYPE OF CITY ^c	N	Row	Criminal Homicide (Col. 1)	Forcible Rape (Col. 2)	Robbery (Col. 3)	Aggravated Assault (Col. 4)	Larceny		Auto Theft (Col. 8)	
							Burglary (Col. 5)	Over \$50 (Col. 6)		Under \$50 (Col. 7)
Ratio of UA population to city population:										
Singular	180	1	.46*	.36*	.59*	.35*	.47*	.30*	.22*	.56*
Primary	33	2	-.17	.29	.32	-.18	.21	.02	-.06	.61*
Secondary	85	3	.01	-.14	.09	-.03	-.05	-.13	-.25*	.03
Tertiary	74	4	-.14	.12	.13	.10	.12	.11	-.07	.23
Total	372	5	-.18*	-.07	-.01	-.10	-.07	-.02	-.16*	.07
Ratio of SMSA population to city population:										
Singular	180	6	.34*	.21*	.40*	.25*	.35*	.31*	.25*	.33*
Primary	33	7	-.21	.00	.04	-.12	-.05	.04	-.13	.22
Secondary	85	8	-.03	-.14	-.03	.05	-.21	-.00	-.12	-.09
Tertiary	74	9	-.10	.23	.15	.09	.17	.25	.02	.26
Total	372	10	-.20*	-.06	-.05	-.10	-.11	.03	-.12*	.03

^a See text for explication of variables and sources of data.

^b UA is an abbreviation of Urbanized Area and SMSA is an abbreviation of Standard Metropolitan Statistical Area.

^c A "singular" city is the largest city in a SMSA with only one central city; a "primary" city is the largest city in a SMSA where there is more than one central city; a "secondary" city is smaller than the one central city in a SMSA; and a "tertiary" city is any city smaller than the largest one in a SMSA where there are two or more central cities. All cities had 50,000 or more residents in 1970.

* $P < .01$.

The correlation coefficients in rows 1 and 6 are by no means of the same magnitude; some of the differences are instructive, while others are contrary to expectations. The differences between the coefficients in row 1 and those in row 6 are instructive, for they suggest that the "effective community" of potential nonresident participants in city crime is fairly small. That is so because UAs are much smaller in area than SMSAs, and with only two exceptions the correlations between community/city population ratios and crime rates are greater when UAs are taken as approximations of communities. Yet it should be recognized that the boundaries of some SMSAs may be grossly unrealistic, largely because the census delimitation procedure does not admit the possibility of only part of a county being within the boundary of a SMSA.

As for exceptions to anticipations, the correlations are not consistently greater for property crimes than for crimes against persons. When UAs are taken as approximations of communities, the highest correlations between community/city population ratios and crime rates are for robbery, auto theft, and burglary (in that order); but the two lowest correlations are for petty larceny (under \$50) and grand larceny (over \$50). Crime statistics pertaining to the value of items taken in a theft are questionable for all manner of reasons, and it may well be that larceny (other than auto theft) is especially likely to go undetected or not be reported to the police if detected. However, since observations on the differential reliability of crime rates by types of crime can be little more than conjecture, the only justifiable conclusion is that the findings are not consistent with anticipations as regards the contrast between property crimes and crimes against persons.

Other types of cities.—The correlation coefficients in rows 2–5 and 7–10 of table 1 bear on the argument that tests of the proposition will yield consistently positive results only for singular cities. Rather than limit the analysis to the relationship between the community/city population ratios and crime rates for all 372 cities as a group (including the 180 singular cities), an attempt was made to distinguish types of nonsingular cities in such a way that some types would be ostensibly more homogeneous with regard to intracommunity dominance.⁷ Thus, "primary" cities are presumed to be more homogeneous with regard to dominance than are "secondary" cities, because all of the former are central cities but some of the latter are not. "Secondary" cities are presumed to be more homogeneous with regard to dominance than are "tertiary" cities, but for a different reason. Some in-

⁷ See the notes to table 1 for definitions of each type and observe that the classification of particular cities would have been much the same had UAs rather than SMSAs been taken as the basis for the typology. No special significance should be attached to the terms chosen to designate types, as the meaning of those terms derives from U.S. census practices.

stances of both types are not central cities, but each secondary city competes with only one central city in the SMSA, whereas the number of "central city competitors" is not the same for all tertiary cities.

The argument that consistently positive results will obtain only for singular cities is clearly supported by the correlation coefficients in table 1. Whereas all of the coefficients pertaining to singular cities (rows 1 and 6) are in the direction anticipated and significant at the .01 level, this is true of only one of 48 coefficients pertaining to nonsingular cities (rows 2-4 and 7-9). Even more impressive evidence of a contingent relationship is found in the correlation coefficients for all cities (including the singular), with 13 of the 16 coefficients being negative (see rows 5 and 10).⁸ On the other hand, there are no consistent differences among the three types of nonsingular cities. That finding is not particularly surprising, since the inferences as to the homogeneity of each type with regard to dominance are crude at best. The point is not just that the census criterion of a central city is somewhat arbitrary but also that the dominance may be extremely variable within any set of cities if each of those cities does not represent the *only* central city in a SMSA.

Controls for other variables.—As suggested earlier, given a high community/city population ratio for a particular city and given known correlates of urban residential patterns in the United States, one would expect such a city to be inhabited by a disproportionate number of nonwhites and/or economically disadvantaged persons. Hence, one could argue that the present findings merely reflect the population characteristics of "under-bounded" cities, and thereby deny the relevance of potential offenders or victims who reside outside the city but in the community.

Table 2 provides data for assessing that argument. Each correlation in table 2 is between the UA/city population ratio and a type of crime rate, with one of four race-economic variables partialled. Only UA/city population ratios are considered, because they lend the most support to the proposition (i.e., a direct relation between community/city population ratios and crime rates), but distinctions as to types of cities are maintained.

As for the singular cities, the argument in question anticipates that correlation between the UA/city size ratio and crime rates approaches zero when race-economic variables are partialled. The findings show that this is clearly not the case. Only when the percentage of nonwhites is partialled

⁸ The fact that the relationship does not hold for all types of cities may generate doubt about the validity of the proposition. Yet if one accepts the "principle of conditionality," a theory or isolated proposition is simply incomplete without stipulation of the conditions (at least one of them) under which the relationship or relationships in question will not hold. In any case, the fact that the relationship does not hold for nonsingular cities precludes dismissing it among singular cities as a "statistical artifact" stemming from the correlation between two ratios in which the city population is the denominator of both.

TABLE 2

CORRELATIONS FOR FOUR TYPES OF UNITED STATES CITIES BETWEEN CRIME RATES AND THE RATIO OF THE URBANIZED AREA
POPULATION TO THE CITY POPULATION, WITH FOUR RACE-ECONOMIC VARIABLES PARTIALED, 1970-71^a

RACIAL OR ECONOMIC VARIABLES PARTIALED ^b AND TYPES OF CITIES ^c	Row	TYPES OF CRIMES							
		Criminal Homicide (Col. 1)	Forcible Rape (Col. 2)	Robbery (Col. 3)	Aggravated Assault (Col. 4)	Burglary (Col. 5)	Larceny Over \$50 (Col. 6)	Under \$50 (Col. 7)	Auto Theft (Col. 8)
Proportion of city population nonwhite, 1970:									
Singular	1	.29*	.22*	.49*	.21*	.36*	.28*	.24*	.49*
Primary	2	-.12	.42*	.51*	-.12	.26	.08	-.04	.63*
Secondary	3	-.30*	-.45*	-.13	-.25	-.23	-.14	-.25	-.10
Tertiary	4	-.05	.27	.35*	.30*	.27	.10	-.04	.40*
Total	5	-.17*	-.01	.08	-.05	-.02	-.01	-.15*	.13*
Median family income, 1970:									
Singular	6	.46*	.35*	.59*	.34*	.47*	.31*	.23*	.56*
Primary	7	-.14	.22	.29	-.22	.17	-.01	-.05	.61*
Secondary	8	.03	-.14	.12	-.02	-.04	-.13	-.25	.05
Tertiary	9	-.01	.17	.22	.21	.17	.05	-.10	.30*
Total	10	-.03	.02	.08	.01	.00	-.04	-.15*	.14*
Percentage of labor force unemployed, 1970:									
Singular	11	.48*	.36*	.58*	.34*	.45*	.27*	.18*	.55*
Primary	12	-.18	.30	.34	-.20	.23	.02	-.05	.62*
Secondary	13	.07	-.11	.15	.00	-.00	-.12	-.24	.07
Tertiary	14	-.21	-.03	-.02	-.02	-.07	.05	-.21	.11
Total	15	-.18*	-.06	.00	-.10	-.06	-.02	-.16*	.08

TABLE 2 (Continued)

RACIAL OR ECONOMIC VARIABLES PARTIALED ^b AND TYPES OF CITIES ^c	Row	TYPES OF CRIMES							
		Criminal Homicide (Col. 1)	Forcible Rape (Col. 2)	Robbery (Col. 3)	Aggravated Assault (Col. 4)	Burglary (Col. 5)	Larceny Over \$50 (Col. 6)	Under \$50 (Col. 7)	Auto Theft (Col. 8)
Percentage of families below poverty level, 1970:									
Singular	16	.47*	.35*	.58*	.33*	.46*	.31*	.23*	.55*
Primary	17	-.11	.26	.32	-.17	.22	0.2	-.01	.62*
Secondary	18	.01	-.21	.14	-.04	-.08	-.13	-.25	.04
Tertiary	19	-.00	.24	.27	.28*	.23	.09	-.06	.34*
Total	20	-.03	.02	.08	.01	.01	-.02	-.15*	.14*

^a See text for explanation of variables and sources of data.^b Data from U.S. Bureau of Census (1972b, tables 16 and 44).^c See table 1 for a definition of each type of city and the *N* for each type.* $P < .01$.

(compare row 1 in table 2 and row 1 in table 1) is there more than a negligible decline in a correlation, and even in those cases all of the partial correlations for singular cities are significant at the .01 level.

Despite the partialing of race-economic variables for each *nonsingular* type of city, the correlations between crime rates and the UA/city size ratio remain either negative or positive but negligible (see rows 2-4, 7-9, 12-14, and 17-19 in table 2). The exceptions are largely those correlations in which the percentage of nonwhites is partialled. The partials in that case make the primary and tertiary cities somewhat more consistent with the proposition, but even for them several types of crimes remain glaring exceptions. Finally, as shown in rows 5, 10, 15, and 20, the partialing of race-economic variables in no way alters the conclusion that a direct relationship between community/city population ratios and crime rates simply does not hold for all types of cities considered together. If anything, the relation is even more negligible when race-economic variables are partialled.

SUMMARY AND CONCLUSIONS

The findings indicate that the crime rates of a city are to some extent a direct function of the community/city population size ratio; but the relationship is contingent on: (1) the manner in which the ratio is estimated, with the closest association obtaining when UAs are taken as approximations of communities; (2) the type of city, with the relationship being the most direct among "singular" cities (each of which is the only central city in a SMSA); and (3) the type of crime (generally, the closest relationship holds for crimes against property, but larceny is an exception). Like virtually all findings of sociological research, it remains to be seen whether the relationship holds in all countries;⁹ but even if it is universal, what one makes of it in the light of criminological theory is another matter.

The maximum amount of variance in crime rates explained by the community/city population ratio (about 35% in the case of robbery) is not substantial by an absolute standard, though it is substantial as such things go in research on crime rates. Moreover, should it ever become feasible to employ a direct measure of dominance as a control variable, the association between the population ratios and the crime rates may prove to be much closer (even among singular cities) than the present findings indicate. So the next step in research should be the development of a direct measure of city dominance (intracommunity);¹⁰ but the ultimate goal is

⁹ In tests of the proposition, all of the previously noted qualifications should be recognized, but they reduce to one stipulation—that tests be limited to urban areas or metropolitan areas in which there is only one central city, whatever the census designation of those types of territorial units in a particular country may be.

¹⁰ Figures on the proportion of community retail establishments located in a city may

not the formulation of a purely "ecological" theory of crime rates, with the community/city population ratio as one independent variable. Such a theory would be all too narrow, as it would exclude several theoretical and empirical variables (e.g., anomie, unemployment) that are central in well-known etiological theories of crime.¹¹ Nonetheless, the present findings should be recognized in attempting to test etiological theories. If the tests are based on conventional crime rates for cities, a substantial proportion of the variance in those rates will not be explained by an etiological theory unless the test procedure takes into account differences among cities as regards ecological position. Since crime rates cannot be computed for UAs, the only way ecological position can be taken into account is by (1) comparing the crime rates of SMSAs or (2) controlling for differences among cities as regards the community/city population ratio and dominance.

Finally, the findings have immediate policy implications that touch on something that is currently of great concern with regard to the "crime problem," especially the great alarm over high rates for particular cities. Policymakers and the public are simply ill informed if they do not recognize the possibility that high crime rates for some cities may reflect largely the ecological position of those cities and not social, economic, or cultural conditions.

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prove to be especially relevant. However, it should be noted that retail figures are compiled for SMSAs and cities but not for UAs (U.S. Bureau of the Census 1970). Moreover, retail stores are not the only type of facility relevant to the dominance of cities within SMSAs.

¹¹ Similarly, the present findings cannot be construed as directly relevant in assessing the labeling perspective in criminology. However, as regards the extreme version of that perspective (according to which the crime rate is generated by the police without regard to the actual frequency of particular types of acts perpetrated by members of the public at large), one must surely wonder why the police would tend to generate high rates in cities where a large number of potential offenders or victims reside outside the city but in the community.

Changes in the Socioeconomic Stratification of the Races, 1962-73¹

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The occupational socioeconomic status of men in the experienced civilian labor force (ECLF) rose between 1962 and 1973, and black men gained more than whites. Although the racial gap narrowed over the decade, black men in 1973 had not matched the occupational standing of white men in 1962. Intercohort changes in schooling and, to a lesser degree, changes in family background account for some of the shifts. Changes in the effects of schooling and of social background have occurred, and these differ between the races. Among white men in the ECLF, family factors are less important in occupational allocation than they were a decade ago; schooling has increased in importance relative to family background; both the effect of schooling on occupational standing and the educational level of occupational incumbents have increased. In contrast, among black men in the ECLF, family factors were more tightly linked with occupational achievement in 1973 than in 1962, and black men with the same socioeconomic background and schooling held higher-status jobs in 1973 than in 1962. Among young workers, patterns of socioeconomic stratification do not differ by race as much as they did in previous years. Still, socioeconomic discrimination by race is prevalent, especially among workers with established work histories. We suggest that race and class have become less important in the processes which allocate men from families to schooling and subsequently to positions in the occupational hierarchy.

Classical sociological theories about advanced industrial societies commonly propose that particularistic standards, such as those based on kinship, race, and ethnicity, eventually give way to universalistic, meritocratic practices and institutions (e.g., Levy 1966). Such transitions follow ineluctably from the functional "requirements" of industrial societies—those of factory pro-

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duction, the formal prerequisites of schooling for employment and jobs, and the character of technical knowledge. Consequently, proponents of theories of industrialization argue that modern societies tend toward common institutional forms and styles (e.g., Feldman and Moore 1962). Critics of these ideas about convergence note the importance of indigenous culture and political history for understanding the alterations of social structure under industrialization (e.g., Goldthorpe 1964; Bell 1973, chap. 1). Others question the verifiability of the theses of industrialization and convergence, arguing that they are definitions rather than testable theories (Garnsey 1975).

The predictions of the industrialism thesis notwithstanding, racial inequalities persist throughout modern American society, and they invite explanation. During the 1960s the civil rights movement, the war on poverty, and ghetto riots were among the more visible signs of the salience of race as a social attribute, but these events in themselves tell us little about changes in the socioeconomic life chances of blacks and whites. Neo-Marxist analysts (e.g., Hechter 1971, 1974) point to the resurgence of ethnic and racial consciousness in the United States and other advanced nations. They argue that racial inequalities and cleavages play an important part in maintaining the social relations of capitalist industrial economies (Bonacich 1976). Indeed, some view racial conflicts in this country as arising from the same "colonization" of the black minority as putatively took place among "peripheral" ethnics by "core" elites in Latin America (Blauner 1969).

Thus, theoretical as well as political or ameliorative interests point to the importance of monitoring racial inequalities and their sources. In this paper we present new data on trends in occupational inequality and stratification among black and white men in the United States, and we explore briefly the implications of our findings for theories of societal development and ethnic relations.

Recent studies of school and residential segregation in major cities find little amelioration since the mid-1950s (Farley and Taeuber 1974), despite the enactment of civil rights legislation in the 1960s, the general rise in the socioeconomic circumstances of blacks in the past 20 years (Farley and Hermalin 1972), and the substantial potential for residential integration that follows from these economic trends (Hermalin and Farley 1973). These concrete realities take on greater significance when seen against apparent shifts in white attitudes toward racial integration (Greeley and Sheatsley 1971; Hermalin and Farley 1973). Of course, disjunctures between public opinion and behavior are nothing new, but they underscore the concern expressed by social commentators (U.S. National Advisory Commission on Civil Disorders 1968) and social scientists alike about the potential volatility of racial relations in this country and about the tendency of

the races to remain apart, residentially, if not also in terms of public attitudes and sentiments (Campbell 1971; Schuman and Hatchett 1974).

Whether we speak of the quality of housing, of employment status, of educational attainment, of occupational level, or of earnings, the importance of socioeconomic standing for the assessment of race relations must not be minimized. Life styles, public attitudes, and political activities reflect socioeconomic circumstances, and civil disorder (at least its severity) seems to covary with racial inequalities in strategic socioeconomic conditions (Morgan and Clark 1973).

More generally, the ethnic dimension is fundamental to social structure as well as to the political climate of the society. Among major axes of social differentiation (e.g., age, sex, class), ethnicity (of which race is an instance) is unique in its potential for political mobilization, with movements to create and maintain separate nation-states serving as clear illustrations (Liebersohn 1970). In addition, ethnic inequality and stratification affect other elements of social structure. For example, they can alter relations among economic classes (Barth 1969; Bonacich 1976; Hechter 1971, 1974) or they can provide for differential patterns and rates of industrial or occupational growth (Hodge and Hodge 1965). Consideration of the racial dimension in studies of inequality and stratification in the United States is essential for understanding and interpreting changes in allocative social processes.

In this paper, we examine changes in the occupational standing of black and white men between 1962 and 1973. For each race separately, and then for both in comparison, we describe shifts in the occupational socioeconomic status of men in the experienced civilian labor force. We attempt to account for occupational changes and differentials in terms of changes and differentials in family background and educational attainment. We also examine the allocative life-cycle processes which relate family background and schooling to current occupational positions. These allocative processes are an important source of social differentiation and inequality, and we refer to them as processes of socioeconomic stratification (Duncan 1968*c*). In the early 1960s processes of stratification were different for black and white men, defining a situation of unequal opportunity for socioeconomic achievement for blacks and whites (Duncan 1967, 1968*a*). Whether these allocative processes have converged toward a uniform pattern is as important as whether the racial gap in occupational status has narrowed over the decade. Each datum refers to a different feature of status inequality in American society; changes in status levels need not imply change in stratification processes, and vice versa.

Recent assessments of socioeconomic trends for the races have noted selective improvements for blacks, both in absolute and relative terms (U.S. Bureau of the Census 1975; Farley and Hermalin 1972). A few studies

have analyzed change in terms of compositional shifts in socioeconomic background or schooling (Hauser and Featherman 1974*a*, 1974*b*). In the past decade blacks gained ground on whites in schooling, occupational status, and income, but the improvements were relatively greater for the young and in some instances occurred only among women. With respect to occupations, both black and white men experienced net upward status shifts in both the manual and nonmanual categories of the experienced civilian labor force, a decline in farming and self-employment, and an increase in salaried professional and managerial roles. These shifts were less a response to changes in the socioeconomic background or schooling of recent cohorts than to changes in labor markets and patterns of career mobility. For example, white men, but not blacks, need more formal education to hold the same jobs that same-aged cohorts held a decade ago. However, black men are still consigned to occupations of lower status than white men of similar social background and educational attainment.

These generalizations about the sources of changing socioeconomic distributions for the races could only be tentative, for they rested upon inferences or projections from baseline studies. For example, Hauser and Featherman (1974*b*) used the 1962 Occupational Changes in a Generation (OCG) survey (Blau and Duncan 1967) to estimate the occupational destinations of black and white cohorts in 1972 as if they had experienced the same allocative processes as men of the same age in 1962. From discrepancies between projected destination distributions and observed distributions in the March 1972 Current Population Survey (CPS), Hauser and Featherman inferred that change in racial stratification had occurred. Such indirect techniques of measuring change and of attempting to explain it are no longer necessary for the 1960s. New data about the socioeconomic origins and destinations of black and white men are available from the 1973 replicate of the 1962 OCG survey. These data provide direct evidence about change in stratification processes and outcomes among black and white men. They also permit some intriguing speculations about the course of racial inequality and about the evolving roles of families and schools in a maturing, postindustrial economy.

DATA

The 1962 OCG survey and its 1973 replicate were carried out in conjunction with the March demographic supplement to the Current Population Survey (CPS) (Blau and Duncan 1967; Featherman and Hauser 1975). The 1962 survey had a response rate of 83% to a four-page questionnaire which was left behind by the CPS interviewer. More than 20,000 men aged 20-64 in the civilian noninstitutional population responded. In 1973, the eight-page OCG questionnaire was mailed out six months after the March

CPS and was followed by mail, telephone, and personal callbacks. The respondents, 88% of the target sample, included more than 33,500 men aged 20-65 in the civilian noninstitutional population. Also, in the 1973 sample, blacks and persons of Spanish origin were sampled at about twice the rate of whites, and almost half the black men were interviewed personally. In this paper we shall make age-constant, intercohort comparisons among men in the postschooling, economically active years. We limit our analysis to men aged 25-64 in the experienced civilian labor force in March 1962 or March 1973, that is, men who were employed and those who were unemployed but had previously held a job. In both OCG samples, women are represented only through husbands. That is, socioeconomic background characteristics of women were elicited only if they were married and living with husbands in the OCG samples. Elsewhere, we have compared the educational and occupational attainments and earnings of male and female married, spouse-present populations (Featherman and Hauser 1976).

We have made extensive efforts to establish the comparability of measurements in the 1962 and 1973 OCG surveys (Featherman and Hauser 1975). Both surveys were carried out by the U.S. Bureau of the Census using its standard field methods and editing, coding, and weighting procedures. For the items on which this report is based, the CPS and OCG questions and questionnaire layouts were almost identical in 1962 and in 1973. Although 1970 census occupation coding materials were in use at the time of the 1973 survey, we arranged for occupation and industry reports to be coded using 1960 census coding materials.

Yet some failures of replication were inevitable, and we have attempted to explore their effects and draw attention to them where they may affect our conclusions. For example, we thought that the quality of the survey data for blacks might be better in the supplementary sample of men interviewed personally than in the basic sample. This might have affected comparisons of blacks and whites in 1973 and comparisons of blacks between 1962 and 1973. However, our examination of black men in the basic sample in 1973 and of those in the supplementary sample revealed no substantial differences between the two. Another unavoidable methodological change was a modification of the wording of the class-of-worker item, intended to improve the measurement of self-employment.

INTERCOHORT SHIFTS IN OCCUPATIONAL SOCIOECONOMIC STATUS

Reports of current (March or last) occupation, industry, and class of worker and corresponding reports for the OCG respondent's father (or other family head) when the respondent was about 16 years old were coded into seven-digit 1960 census codes, and these were mechanically

transformed into values of Duncan's (1961) socioeconomic index (SEI) for occupations. Thus our measure of occupational standing does not refer to the prestige of the jobholder or of incumbents in his occupation, but to the average educational and income level of occupational incumbents. We believe that occupational socioeconomic status is the major dimension along which occupational positions persist from generation to generation (Featherman, Jones, and Hauser 1975); for further discussion of the measurement and scaling of occupations, see Featherman and Hauser (1973). As noted above, if the respondent's father was absent, "father's" occupation refers to that of the mother or other household head; about one-third of black men and one-sixth of nonblack men said they did not live with both parents most of the time up to age 16.

Following a well-established pattern, the net intercohort shifts in current occupational socioeconomic status have been upward for both whites (table 1) and blacks (table 2) at all ages. For whites aged 25-64 in the

TABLE 1

MEANS AND STANDARD DEVIATIONS OF OCCUPATIONAL STATUS VARIABLES: NONBLACK MEN AGED 25-64 IN THE EXPERIENCED CIVILIAN LABOR FORCE, MARCH 1962 AND MARCH 1973

Age	1962		1973		ARITHMETIC CHANGE	
	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
Total, 25-64:						
Father's occupation ...	28.09	21.27	30.15	22.57	2.06	1.30
Current occupation ...	39.25	24.44	42.58	25.22	3.33	0.78
25-34:						
Father's occupation ...	30.36	21.75	33.96	23.93	3.60	2.18
Current occupation ...	40.37	24.96	42.74	24.95	2.37	-0.01
35-44:						
Father's occupation ...	28.74	21.78	30.13	22.42	1.39	0.64
Current occupation ...	40.66	24.71	44.59	25.45	3.93	0.74
45-54:						
Father's occupation ...	26.56	20.45	28.01	21.41	1.45	0.96
Current occupation ...	38.11	23.57	43.13	25.27	4.02	1.70
55-64:						
Father's occupation ...	25.86	20.44	26.52	20.92	0.66	0.48
Current occupation ...	36.89	24.23	38.63	24.88	1.74	0.65

experienced civilian labor force (ECLF), the increase of 3.33 points on the Duncan scale between 1962 and 1973 represented a shift of about 14% of the 1962 standard deviation. Larger than average intercohort improvements in current status were experienced by white men in the middle years, ages 35-54, while the youngest and oldest age groups had smaller gains. Among white men there were small upward intercohort shifts in the aver-

Socioeconomic Stratification of the Races, 1962-73

TABLE 2

MEANS AND STANDARD DEVIATIONS OF OCCUPATIONAL STATUS VARIABLES: BLACK MEN AGED 25-64 IN THE EXPERIENCED CIVILIAN LABOR FORCE, MARCH 1962 AND MARCH 1973

Age	1962		1973		ARITHMETIC CHANGE	
	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
Total, 25-64:						
Father's occupation ...	16.15	12.88	15.95	13.72	-0.20	0.84
Current occupation ...	17.77	15.16	25.76	20.44	7.99	5.28
25-34:						
Father's occupation ...	17.36	15.34	17.66	15.61	0.30	0.27
Current occupation ...	18.30	16.34	29.10	21.74	10.80	5.40
35-44:						
Father's occupation ...	14.79	11.26	16.32	14.20	1.53	2.90
Current occupation ...	19.24	16.05	27.66	21.34	8.42	5.29
45-54:						
Father's occupation ...	16.24	11.58	14.39	11.90	-1.85	0.32
Current occupation ...	17.19	13.85	23.43	18.66	6.24	4.81
55-64:						
Father's occupation ...	16.36	12.55	14.06	10.35	-2.30	-2.20
Current occupation ...	14.94	12.70	18.72	16.06	3.78	3.36

age status of father's occupation. These may be observed in comparisons across cohorts within either survey (read down the columns of the table) or across cohorts between surveys. Over the decade, inequality of occupational socioeconomic status remained virtually constant among whites, as shown by standard deviations in table 1.

At every age, black men enjoyed larger absolute and relative upward shifts in current occupational status than did whites (table 2). For example, the 8-point rise in average status among black men aged 25-64 was almost two and one-half times the gain for whites, and it represented an improvement equal to 53% of the black standard deviation in 1962. Absolute and relative gains varied inversely with age among blacks; the greatest gains occurred among young workers.² These changes seem un-

² In assessing shifts between the OCG surveys, it is important to remember that the civilian noninstitutional population of 1973 included a larger percentage of (especially younger) cohorts between the ages of 25 and 64 than in 1962. Better coverage in 1973 stems, in the main, from the fact that the armed forces had decreased in size. For example, coverage of men aged 25-34 in the 1962 OCG was 91.5%; in 1973, 94.5% of the men aged 25-34 were covered in the OCG sample under analysis. The bearing of more extensive coverage via a less extensive armed forces on our comparisons is difficult to assess, as the effects are apt to differ for the races. Moreover, our focus on the ECLF compounds the issue, inasmuch as young black men aged 25-34 were less likely to be in the labor force in 1973 than same-aged blacks in 1962. This is not merely of methodological interest: our study excludes the substantial minority of young black men who are not in the labor force as conventionally measured.

likely to have followed from shifts in the socioeconomic circumstances of the families in which black men were reared, as net shifts in paternal (head's) status were very small and irregular (both between and within surveys). The variation in current occupational status increased for blacks at every age over the decade, and the size of this shift, too, varied inversely with age. Thus greater status inequality within the black male labor force came along with higher levels of occupational standing.

Of course, gains for blacks must be viewed in the context of their historically subordinate status relative to whites. At every age, and both with respect to paternal and current occupational statuses, blacks in 1973 occupied a lower socioeconomic level than whites at any age 11 years earlier. Still, racial gaps in current occupational status have narrowed (table 3). In 1962 the black average of current occupational status was

TABLE 3
RACIAL DIFFERENCES IN AVERAGE OCCUPATIONAL STATUS AND IN SOCIOECONOMIC
VARIATION: MEN AGED 25-64 IN THE EXPERIENCED CIVILIAN LABOR FORCE,
MARCH 1962 AND MARCH 1973

Age	1962		1973		ARITHMETIC CHANGE	
	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
Total, 25-64:						
Father's occupation ...	11.94*	8.39*	14.20	8.85	2.26	0.46
Current occupation ...	21.48	9.28	16.82	4.78	-4.66	-4.50
25-34:						
Father's occupation ...	13.00	6.41	16.30	8.32	3.30	1.91
Current occupation ...	22.07	8.62	13.64	3.21	-8.43	-5.41
35-44:						
Father's occupation ...	13.94	10.52	13.81	8.22	-0.13	-2.30
Current occupation ...	21.42	8.66	16.93	4.11	-4.49	-4.55
45-54:						
Father's occupation ...	10.32	8.87	13.62	9.51	3.30	0.64
Current occupation ...	20.92	9.72	19.70	6.61	-1.22	-3.11
55-64:						
Father's occupation ...	9.50	7.89	12.46	10.57	2.96	2.68
Current occupation ...	21.95	11.53	19.91	8.82	-2.04	-2.71

* Positive difference indicates higher value for whites; negative difference indicates higher value for blacks.

more than 20 points below the white average at every age. By 1973, while the racial gap was still close to 20 points among men over 45 years old, it had narrowed to less than 14 points in the youngest cohort. The racial gap had closed by nearly $8\frac{1}{2}$ points on the Duncan scale at ages 25-34 and by $4\frac{1}{2}$ points for all men aged 25-64. These declines are 38% and 22% of the respective racial gaps in 1962. At the same time, blacks appear

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to have lost ground relative to whites in socioeconomic background, as all but the cohort aged 35-44 in 1973 were reared in relatively less beneficial socioeconomic circumstances than blacks in 1962. Thus, racial differences in occupational status origins may remain an important source of current occupational status differentials between the races.

Historically, variation in current socioeconomic status has been greater for whites than blacks, reflecting greater differentiation in the white occupational distribution. Shifts in differential status inequality have occurred since 1962; the ratio of the black to white standard deviations (tables 1 and 2) has risen from 0.62 to 0.81 for men aged 25-64 in the ECLF. Again, the convergence between the races is most apparent at younger ages.

Another way of describing the changes in occupational status differentials between the races is to note that black men in the ECLF of 1973 are more likely than those in the 1962 ECLF to have experienced intergenerational status mobility like that among whites. Table 4 reorganizes

TABLE 4

AVERAGE INTERGENERATIONAL OCCUPATIONAL STATUS MOBILITY: MEN AGED 25-64 IN THE EXPERIENCED CIVILIAN LABOR FORCE, BY COLOR, MARCH 1962 AND MARCH 1973

Age	1962		1973	
	Nonblack	Black	Nonblack	Black
Total, 25-64	11.16	1.62	12.43	9.81
25-34	10.01	0.94	8.78	11.44
35-44	11.92	4.45	14.46	11.34
45-54	11.55	0.95	15.12	9.04
55-64	11.03	-1.42	12.11	4.66

tables 1 and 2 by comparing the status of a man's current occupation with that of his father's occupation as an index of status mobility in the life cycle (between age 16 and the survey date). In 1962, white men in every cohort held occupations which averaged 10 or more points higher in status than their fathers' occupations. Black men of all ages except 35-44 had not been able to advance in the status hierarchy much beyond the positions of their family heads. This is not to say that black men tended to "inherit" the occupations of their fathers by going into the same general line of work; if anything, the facts are to the contrary (see Duncan 1968*b*; Hauser, Featherman, and Hogan, forthcoming). In 1973, the upward intergenerational mobility of whites continued in roughly the same amounts, but black men were far more likely to be upwardly mobile than their counterparts a decade earlier. In fact, at ages 25-34, the absolute amount of intergenerational mobility in the black population is greater

than in the white population (11.44 vs. 8.78 points on the Duncan scale). Thus, black men have begun recently to experience status mobility in their life cycles which resembles more closely that among whites. The importance of cohort replacement in social change is suggested by the inverse variation of intergenerational status gains with age among blacks in 1973. At the same time some of the observed status gains relative to fathers may have occurred since 1962. We can make a crude comparison of earlier and later measurements on the same cohort by reading table 4 diagonally (from upper left to lower right). For example, the status gain was 4.45 points among blacks aged 35-44 in 1962, but 9.04 points among black men aged 45-54 in 1973. If we assume that these figures refer to successive measurements of the same cohort, we must conclude that there was an intragenerational upward status shift of about 4.5 points between 1962 and 1973.

INTERCOHORT CHANGES IN SOCIOECONOMIC BACKGROUND AND EDUCATION

We have already commented on the rising occupational status origins of successive cohorts of white men and the absence of parallel shifts among black cohorts. In most other respects the social background of successive cohorts of both races has become more conducive to high levels of occupational achievement.⁸ Between the cohorts aged 55-64 and those aged 25-34 in the 1973 survey, the average educational attainment of fathers rose from 7.31 years to 9.89 years among white men and from 4.94 years to 7.64 years among black men. Between these same cohorts the average number of siblings declined from 4.30 to 3.18 among white men and from 5.32 to 5.07 among blacks. The proportion of nonblack men whose fathers were farmers fell from 34% to 14% among nonblacks and from 59% to 26% among blacks. However, between these two cohorts there was virtually no change in the proportions of blacks or whites raised in broken families; among whites the shares were 15% in the oldest cohort and 13% in the youngest. Thirty-two percent of both black cohorts were raised in broken families. Obviously, these trends have not eliminated racial differ-

⁸ Paternal education is scaled in years completed according to the following recode of class intervals: no school, 0.0 years; elementary (1-4), 3.3 years; elementary (5-7), 6.3 years; elementary (8), 8.0 years; high school (1-3), 9.9 years; high school (4), 12.0 years; college (1-3), 13.8 years; college (4), 16.0 years; college (5 or more), 18.0 years. Number of siblings is the number of respondent's brothers and sisters. Farm origins is a dummy variable, with a score of one indicating that respondent's father was a farmer, farm manager, farm laborer, or farm foreman. Broken family is a dummy variable, with a score of one indicating that the respondent was not living with both parents (however, respondent defined the situation) most of the time up to age 16. Respondent's education is in single years, as reported to the CPS.

ences in socioeconomic origins. These and other trends and differentials in social background are treated in detail by Hauser and Featherman (1976).

Trends in socioeconomic background are dwarfed by the large and regular increases in educational attainment among men of both races (not tabulated here). In 1962 the oldest cohort of black workers averaged 5.4 years of schooling, but by 1973 the youngest cohort of black workers had completed an average of 11.6 years of school. Among whites also the educational change was impressive: from an average of 9.6 years among the oldest men in 1962 to an average of 12.7 years in the youngest 1973 cohort. Overall, the schooling of the average black worker rose from 8 to 10 years between 1962 and 1973, while that of the average white rose from 11 to 12 years. Growth in educational attainment was accompanied by a narrowing of the educational distribution. Among white male workers the standard deviation of educational attainment fell from 3.6 years in the oldest 1962 cohort to 2.8 years in the youngest 1973 cohort. Among black men there was a parallel decline in educational inequality; the standard deviation of schooling was 3.8 years among the oldest black men in 1962 and 2.6 years among the youngest black men in 1973. Elsewhere, we have shown that greater educational equality was produced by a combination of reduced effects of social background and increased equality among men with the same social background (Hauser and Featherman 1976).

Racial differentials in educational attainment seem to be disappearing. Whereas the black-white difference in mean education was 3.0 years in 1962 for men aged 25-64 in the ECLF, the gap was 2.0 years in 1973. At ages 55-64 the racial gap in average educational attainment narrowed from 4.2 years in 1962 to 3.2 years in 1973, and among young men (aged 25-34) it closed by half, from 2.3 years in 1962 to 1.2 years in 1973. Indeed, in 1973 there was no difference in the average length of schooling between young black and white men with the same socioeconomic background (Hauser and Featherman 1976). Thus, declining racial differentials in schooling, especially at the youngest ages, parallel observed declines in occupational socioeconomic differentials and may help to explain them. We defer a discussion of the contributions of intercohort shifts in family socioeconomic factors and education to racial differentials in occupational status until we have looked at change over the decade in the process of occupational stratification.

PROCESSES OF SOCIOECONOMIC ALLOCATION IN 1962 AND 1973

In tables 5 and 6 we have elaborated a "basic" model of occupational stratification (Blau and Duncan 1967, chap. 5) to include a somewhat broader array of family background factors. Table 5 gives estimates of the

TABLE 5

REGRESSION ANALYSIS OF CURRENT OCCUPATIONAL STATUS ON FAMILY BACKGROUND FACTORS: MEN AGED 25-64 IN THE ECILF,
BY COLOR, MARCH 1962 AND MARCH 1973

PREDETERMINED VARIABLES								
POPULATION	Father's Occupational Status	Father's Education	Siblings	Farm Origin	Broken Family	R ²	CONSTANT	SEE
1962								
Total, 25-64:								
Nonblack	0.286 (0.016)	0.873 (0.080)	-1.097 (0.105)	-5.949 (0.662)	-3.245 (0.743)	.209	31.00	21.75
Black	0.067 (0.052)	0.563 (0.175)	-0.221 (0.261)	-4.978 (1.318)	-0.506 (1.354)	.080	17.06	14.61
25-34:								
Nonblack	0.265 (0.029)	1.173 (0.162)	-1.306 (0.207)	-5.502 (1.388)	-4.011 (1.446)	.216	28.54	22.13
Black	0.051 (0.086)	0.837 (0.369)	0.046 (0.417)	-6.822 (2.720)	-0.556 (2.781)	.110	13.89	15.64
35-44:								
Nonblack	0.277 (0.028)	0.985 (0.146)	-1.167 (0.195)	-6.456 (1.225)	-3.372 (1.370)	.224	31.79	21.79
Black	0.124 (0.115)	0.569 (0.362)	-0.506 (0.415)	-7.464 (2.522)	-1.367 (2.667)	.117	20.43	15.33
45-54:								
Nonblack	0.331 (0.031)	0.586 (0.149)	-0.921 (0.203)	-5.113 (1.247)	-1.225 (1.408)	.195	30.77	21.18
Black	0.039 (0.114)	0.264 (0.346)	-0.313 (0.462)	-2.765 (2.718)	0.112 (2.631)	.033	18.31	13.90
55-64:								
Nonblack	0.254 (0.041)	0.786 (0.195)	-1.006 (0.255)	-7.316 (1.559)	-4.438 (1.850)	.191	32.79	21.85
Black	0.134 (0.119)	0.348 (0.376)	0.138 (0.466)	-1.024 (2.915)	-1.717 (2.911)	.047	11.78	12.75

TABLE 5 (Continued)

PREDETERMINED VARIABLES							R ²	CONSTANT	SEE
POPULATION	Father's Occupational Status	Father's Education	Siblings	Farm Origin	Broken Family				
1973									
Total, 25-64:									
Nonblack	0.249 (0.010)	0.866 (0.056)	-1.266 (0.077)	-4.789 (0.494)	-2.472 (0.533)	.181	33.76	22.83	
Black	0.200 (0.043)	1.062 (0.160)	-0.513 (0.188)	-5.009 (1.198)	-1.946 (1.148)	.138	20.87	19.01	
25-34:									
Nonblack	0.232 (0.018)	1.020 (0.108)	-1.454 (0.148)	-1.616 (1.043)	-2.711 (1.001)	.175	29.98	22.68	
Black	0.246 (0.070)	1.180 (0.340)	-0.930 (0.344)	-3.861 (2.395)	-1.303 (2.095)	.167	21.87	19.97	
35-44:									
Nonblack	0.232 (0.020)	1.030 (0.112)	-1.379 (0.149)	-5.882 (0.988)	-3.078 (1.048)	.196	35.51	22.83	
Black	0.177 (0.088)	0.918 (0.361)	-0.526 (0.389)	-4.764 (2.522)	-2.621 (2.388)	.099	23.84	20.40	
45-54:									
Nonblack	0.260 (0.021)	0.965 (0.111)	-1.162 (0.147)	-4.924 (0.918)	-3.172 (1.017)	.182	34.58	22.87	
Black	0.162 (0.092)	0.732 (0.288)	-0.197 (0.367)	-4.034 (2.227)	-3.113 (2.210)	.081	20.98	18.03	
55-64:									
Nonblack	0.270 (0.026)	0.883 (0.130)	-1.053 (0.172)	-7.691 (1.054)	-0.631 (1.236)	.199	32.26	22.29	
Black	0.092 (0.111)	0.979 (0.332)	-0.144 (0.386)	-6.349 (2.429)	0.396 (2.409)	.132	16.95	15.16	

NOTE.—Approximate standard error in parentheses; SEE = standard error of estimate.

TABLE 6

REGRESSION ANALYSIS OF CURRENT OCCUPATIONAL STATUS ON FAMILY BACKGROUND FACTORS AND EDUCATION: MEN AGED 25-64 IN THE ECLF,
BY COLOR, MARCH 1962 AND MARCH 1973

POPULATION	PREDETERMINED VARIABLES						
	Father's Occupational Status	Father's Education	Siblings	Farm Origin	Broken Family	Education	SEE
1962							
Total, 25-64:							
Nonblack	0.167 (0.014)	0.072 (0.072)	-0.242 (0.095)	-3.000 (0.587)	0.576 (0.657)	3.597 (0.587)	19.14
Black	0.046 (0.050)	0.196 (0.175)	-0.112 (0.207)	-1.424 (1.351)	0.418 (1.301)	1.272 (0.175)	13.97
25-34:							
Nonblack	0.122 (0.025)	0.271 (0.141)	-0.332 (0.180)	-3.606 (1.179)	-1.762 (1.229)	4.435 (0.165)	18.76
Black	0.025 (0.082)	0.347 (0.366)	-0.066 (0.396)	-3.212 (2.697)	0.287 (2.640)	1.830 (0.405)	14.81
35-44:							
Nonblack	0.151 (0.025)	0.135 (0.131)	-0.253 (0.173)	-3.344 (1.066)	-0.379 (1.191)	3.978 (0.154)	18.85
Black	0.063 (0.113)	0.329 (0.356)	-0.318 (0.405)	-4.111 (2.619)	-0.284 (2.595)	1.153 (0.330)	14.80
45-54:							
Nonblack	0.189 (0.028)	-0.103 (0.135)	-0.181 (0.182)	-2.247 (1.109)	1.039 (1.247)	3.494 (0.160)	18.70
Black	0.078 (0.110)	-0.070 (0.344)	-0.108 (0.446)	1.421 (2.853)	0.917 (2.525)	1.271 (0.358)	13.29
55-64:							
Nonblack	0.157 (0.038)	0.058 (0.182)	-0.381 (0.234)	-5.283 (1.413)	-1.405 (1.632)	2.998 (0.191)	19.73
Black	0.022 (0.115)	0.140 (0.357)	0.158 (0.436)	2.054 (2.856)	-0.677 (2.739)	1.418 (0.390)	11.93

TABLE 6 (Continued)

POPULATION	PREDETERMINED VARIABLES						R ²	CONSTANT	SEE
	Father's Occupational Status	Father's Education	Siblings	Farm Origin	Broken Family	Education			
1973									
Total, 25-64:									
Nonblack	0.153 (0.009)	-0.112 (0.051)	-0.284 (0.068)	-1.399 (0.433)	0.848 (0.467)	4.258 (0.062)	.377	-10.98	19.91
Black	0.164 (0.039)	0.293 (0.151)	-0.322 (0.170)	-0.286 (1.118)	-0.382 (1.042)	2.666 (0.156)	.297	-3.62	17.18
25-34:									
Nonblack	0.135 (0.015)	-0.052 (0.097)	-0.318 (0.131)	-1.566 (0.901)	0.184 (0.868)	4.897 (0.124)	.384	-22.53	19.60
Black	0.151 (0.063)	0.623 (0.278)	-0.620 (0.310)	0.213 (2.182)	-0.756 (1.878)	3.827 (0.372)	.332	-19.34	17.90
35-44:									
Nonblack	0.122 (0.018)	0.055 (0.100)	-0.277 (0.131)	-2.877 (0.849)	0.204 (0.118)	4.300 (0.900)	.412	-12.18	19.52
Black	0.144 (0.076)	0.100 (0.323)	-0.149 (0.339)	-0.089 (2.234)	0.328 (2.092)	3.487 (0.332)	.323	-10.97	17.71
45-54:									
Nonblack	0.161 (0.019)	0.028 (0.100)	-0.359 (0.130)	-0.903 (0.808)	0.664 (0.893)	4.183 (0.119)	.378	-9.04	19.94
Black	0.116 (0.084)	0.001 (0.274)	0.090 (0.333)	-0.294 (2.060)	-1.527 (2.005)	2.406 (0.282)	.252	0.38	16.29
55-64:									
Nonblack	0.170 (0.024)	0.067 (0.120)	-0.261 (0.156)	-3.832 (0.951)	2.109 (1.105)	3.601 (0.140)	.365	-3.34	19.84
Black	0.076 (0.105)	0.535 (0.325)	-0.060 (0.364)	-3.774 (2.342)	1.367 (2.275)	1.506 (0.299)	.235	5.62	14.26

NOTE.—Approximate standard error in parentheses; SEE = standard error of estimate.

reduced-form equation relating five exogenous, predetermined family factors to occupational socioeconomic status. Table 6 reports estimates of our final equation, which includes education as a regressor. We do not include first job in our model, as this item was not comparable between the 1962 and 1973 surveys (see Featherman and Hauser 1975; the education equation in our model is analyzed in Hauser and Featherman 1976).

In the reduced-form equation for current occupational status (table 5) we find the now rather familiar pattern of relationships between family background and occupational status among white men in the 1962 ECLF. Both father's occupation and education made positive contributions to occupational standing, even if these were small in metric terms. Large numbers of siblings, farm origins, and rearing in a broken family all had negative effects. About 21% of the variance in occupational status was explained by these five family factors. Among blacks in 1962, only farm origins and paternal education had statistically significant effects on occupational status. Overall, the five family factors accounted for a mere 8% of the variance, but among blacks aged 25-44 the larger handicap of farm origins increases R^2 to about .11.⁴ While occupational status was more closely linked to family background among white than among black men in 1962, there was also greater variability in occupational standing among white than among black men with the same social background. (These two findings are not inconsistent because the total variability in occupational standing was much greater among white than black men.) The standard errors of estimate (standard deviations about the regression line) of occupational status were about 22 points on the Duncan SEI scale for white men at every age, but for black men they varied inversely with age from 12.8 points for the cohort aged 55-64 to 15.6 points for the cohort aged 25-34. Thus, net of social background, white men had more diverse status opportunities than blacks, but the variation by age among blacks suggested a movement toward the white pattern.

By 1973, the articulation of family background and occupational status had decreased slightly at all ages among whites, while it had increased among blacks except at ages 35-44 (compare R^2 values in 1962 and 1973 by age within race in table 5). Conditional inequality of occupational socioeconomic status (see the standard errors of estimate) increased within both races, but more for blacks than for whites. Still, the diversity of status opportunities remained greater among whites than blacks at every age in

⁴ Analysis of remeasurement data from the 1973 OCG survey shows that the depressing effects of random measurement error on these regression coefficients are larger among black than among white respondents (Bielby, Hauser, and Featherman 1976). Consequently, our comparisons of black and white regression equations for 1962 and for 1973 probably overstate racial differences in the occupational achievement process. Obversely, the convergence in achievement processes between black and white men may be closer than that reported below.

1973. Because the conditional inequality of occupational status increased substantially among black men, the changes in R^2 between 1962 and 1973 understate the increasing effects of socioeconomic background on occupational status among black men. That is, "class" factors are becoming more important in explaining the distribution of occupational status in the black population.

Among whites there were small, and in some cases irregular, changes in the effects of specific background variables on occupational status; among all men 25-64 years old the effect of every variable except number of siblings declined absolutely between 1962 and 1973. With the exception of farm origin, the bearing of each family factor on black occupational status increased (absolutely) over the decade. Thus, the several regression coefficients, as well as variance components, suggest a convergence between the races in patterns of occupational stratification. This is most apparent among men aged 25-34 in 1973. In that cohort the effects of father's occupational status and educational attainment were virtually identical for white and black men, and only the racial differences in the effects of number of siblings and farm origin were as large as one standard error of the black regression coefficient. Again, these similarities in regression slopes do not imply equality of occupational status levels between blacks and whites, but they do indicate growing similarity in the process of occupational achievement. Even outside the group aged 25-34 the regression coefficients for black men are more comparable to the white values than in 1962. One other noteworthy change is the declining influence of farm background for both whites and blacks in the two youngest cohorts.

In table 6 educational attainment of the respondent is added to the occupational status equation. Readers familiar with the 1962 OCG findings will remember that education was a major factor in the basic model of socioeconomic achievement (Blau and Duncan 1967, chap. 5). For virtually all cohorts in both races, the addition of education to the set of family background regressors nearly doubled the explained variance in occupational status. About 39% of that variance for whites and 16% for blacks was explained by family factors and schooling.

Of course, the introduction of education into the model of stratification altered the reduced-form coefficients for family effects on achievement in 1962. In brief, the effect of each family factor was reduced, signaling the importance of schooling as an intervening mechanism (as well as a direct causal agent) of status transmission (Alwin and Hauser 1975). That is, family socioeconomic resources and related factors were converted into occupational status by means of differential educational attainment of offspring. An illustration of this role of education is the reduction by about 50% of the handicap of farm origin for whites when education is entered into the equation (compare 1962 panels by age and race in tables 5 and 6).

Larger reductions occurred for older white men. Among blacks most (70%) of the negative effect of farm origin was associated with the lower educational attainments of black farmers' sons in 1962. Excepting the persistent influence of father's occupational status and farm origin, virtually all of the effects of socioeconomic origins on occupational status were explained by differential schooling in 1962. These same explanatory relationships reappear in the 1973 data. Schooling mediates much of the influence of socioeconomic background on occupational status among men of both races throughout the economically active years (compare 1973 panels by age and race in tables 5 and 6).

Perhaps the most important intercohort change in the occupational achievement process is the increasing effect of educational attainment. For white men aged 25-64 in the 1962 ECLF, each additional year of schooling was worth 3.6 points of occupational status on the Duncan scale; for black men, each additional year was worth only one-third as much, 1.3 points. Similar racial differentials in occupational effects of schooling occurred at every age in 1962. By 1973, the effect of an additional year of schooling on the occupational statuses of whites aged 25-64 in the ECLF had increased 18% (to 4.3 points); among blacks it had more than doubled (to 2.7 points). In relative terms the return to schooling increased for blacks to 63% of its value for whites. The effect of schooling on occupational standing increased between successive cohorts among black and white men at every age. Among white men, but not among blacks, the greater returns to schooling appear primarily to reflect cohort replacement. That is, returns to schooling do not appear to have changed within cohorts over the decade; the effect of years of schooling is about the same for the cohorts aged 35-64 in 1973 as for the respective cohorts aged 25-54 in 1962. On the other hand, among black men the effect of years of schooling was roughly twice as large at ages 35-44 and 45-54 in 1973 as it was at ages 25-34 and 35-44, respectively, in 1962. Thus, among black men career mobility as well as cohort replacement appear to be factors in the increasing effect of schooling on occupational status.

As a consequence of these changes, younger blacks and whites experienced much more similar occupational returns to each year of schooling in 1973 than in 1962. In fact, for young workers, especially those aged 25-34, the stratification process was far less differentiated by race than a decade earlier. Apart from the remaining difference in the education coefficient (about 1 point on the SEI scale per year of schooling), the net effects of family factors are rather similar, if not in absolute size, at least in that they are uniformly small and not significantly different from one another (compare the racial differences with the standard errors of the coefficients among blacks). At least at these younger ages, there is evidence of convergence

in the allocative mechanisms, though not of complete equality of occupational opportunity.

As returns to schooling have increased, so has the proportion of explained variance in occupational status attributable to education net of family background. About 38% of the variance in white occupational status and 30% of that in black attainment was explained in 1973. Note that R^2 *decreased* trivially over the decade for whites ($R^2 = .387$ vs. $.377$) and *increased* substantially for blacks ($R^2 = .160$ vs. $.297$). Of the explained variance, 52% and 54% are assignable to the effect of schooling net of social background for whites and blacks, respectively. (The figure for both races in 1962 was 48%.)

Despite the increased effects of schooling, the conditional inequality in occupational status increased between 1962 and 1973. That is, the variation in occupational status net of family background and schooling increased among black as well as white men and at every age (compare standard errors of estimate between years in table 6). While critics of educational credentialism argue that reliance on schooling as a criterion for employment creates a needless and rigid link between schooling and jobs, the data show that variations in occupational standing have increased along with the effects of schooling.

The possible convergence of blacks and whites in processes of stratification appears to result from two opposite trends. First, among whites there has been a modest weakening of stratification—the linking of status in one generation to that in the next. This has occurred without reducing occupational inequality and in conjunction with a greater role of education (relative to family background) in the generation of socioeconomic differences. Mechanisms which allocate whites in the occupational hierarchy became more egalitarian and meritocratic and less deterministic by the early 1970s than they had been in the early 1960s.

For blacks, there has been a second, more noticeable, and perhaps more socially significant change. The capacity of both families and schools to provide resources which black men can convert into occupational achievements has grown. The tighter articulation between family background and achievement has fashioned a pattern of intergenerational stratification for younger blacks which resembles that among younger white men. It is ironic that parity between black and white men in the achievement process may occur as a consequence of greater socioeconomic ascription within the black population. At the same time, the effect of education on occupational status has increased absolutely and relative to that of the family since 1962, and there is growing inequality in the statuses of black men of similar social origins and schooling. Thus, as intergenerational stratification has increased

for blacks, the process has also become more meritocratic, for educational credentials meant more to a black man in 1973 than in 1962.

SOURCES OF CHANGE IN SOCIOECONOMIC STATUS

For both black and white men the average socioeconomic status of occupations rose between 1962 and 1973. To what can we attribute this pattern of change? In particular, can intercohort shifts in status be explained by changes in family factors and educational attainment? In seeking answers to these questions we have standardized our data on the 1973 regression equations for each race separately. (For an alternative standardization see Hauser and Featherman 1974b.) For example, among blacks aged 25-34, the intercohort shift in mean socioeconomic status was 10.80 SEI points (table 2). To decompose this difference, we insert the means of the family background variables for 25- to 34-year-old black men in 1962 into the reduced-form regression for blacks aged 25-34 in 1973 (from table 5). The estimated socioeconomic score is 0.78 points lower than the observed mean in 1973, indicating that about 7% of the intercohort change is associated with shifts in family factors for this age between 1962 and 1973 (see table 7). We then insert the 1962 means of the family factors and educational attainment into the full 1973 regression equation (from table 6) for blacks aged 25-34. The estimated socioeconomic score is an additional 7.12 points below the observed 1973 mean; thus, shifts in educational attainment over the decade, net of family factors, account for some 66% of the intercohort change in occupational achievement among black men aged 25-34. The remaining 27%, or 2.90 points on the Duncan scale, represents change in occupational opportunities between 1962 and 1973. Of course, these components of change may vary if other variables are added to the regression model or if a different regression equation, for example the 1962 one, is taken as the standard. For this reason our calculation might better be regarded as a mental experiment than as a statistical estimate in the usual sense. We have carried out this indirect regression standardization for each age group in the white and black samples.

For blacks, shifts in family background account for only a small portion of total intercohort changes in occupational status—about 13% for men aged 25-64 (see table 7). A larger percentage of change comes from rising levels of schooling—between 60 and 75%—and there is less age variation in this percentage than for the family background components taken as a block. Except at ages 45-54, roughly three-quarters of the rise in occupational status of black men results from increased levels of schooling and small net improvements in family background circumstances. The remaining quarter represents change in the occupational opportunities of black

Socioeconomic Stratification of the Races, 1962-73

TABLE 7

COMPONENTS OF INTERCOHORT CHANGE IN OCCUPATIONAL SOCIOECONOMIC STATUS:
MEN AGED 25-64 IN THE EXPERIENCED CIVILIAN LABOR FORCE, BY
COLOR, MARCH 1962 AND MARCH 1973

Age and Component	Black	Nonblack
Total, 25-64:		
Family factors*	1.06 (13)	1.86 (56)
Education	4.68 (59)	3.07 (92)
Residual	2.25 (28)	-1.60 (-48)
Intercohort change	7.99 (100)	3.33 (100)
25-34:		
Family factors	0.78 (7)	2.78 (117)
Education	7.12 (66)	2.01 (85)
Residual	2.90 (27)	-2.42 (-102)
Intercohort change	10.80 (100)	2.37 (100)
35-44:		
Family factors	1.32 (16)	1.75 (45)
Education	6.48 (77)	4.23 (108)
Residual	0.62 (7)	-2.05 (-52)
Intercohort change	8.42 (100)	3.93 (100)
45-54:		
Family factors	0.15 (2)	1.58 (31)
Education	3.74 (60)	3.93 (78)
Residual	2.35 (38)	-0.49 (-10)
Intercohort change	6.24 (100)	5.02 (100)
55-64:		
Family factors	0.88 (23)	0.92 (53)
Education	2.85 (75)	3.96 (228)
Residual	0.05 (1)	-3.14 (-180)
Intercohort change	3.78 (100)	1.74 (100)

NOTE.—Numbers in parentheses are percentages.

* Includes paternal (head's) occupational status and education, number of siblings, farm origins, and broken family.

men with given socioeconomic origins and levels of schooling.⁵ Compositional shifts in family factors and education come closest to explaining intercohort change in occupational status at ages 35-44 and 55-64.

For whites, the compositional changes in family factors and education are more than large enough to account for the small intercohort rises in average occupational status. This fact is apparent from the negative signs of the residual components in table 7. For example, among men aged 25-64, intercohort increases in average schooling account for nearly all (92%)

⁵ Since effects of family background and schooling on occupational status increased among black men between 1962 and 1973, choice of 1973 regressions as the standard yields smaller residual components of change in status opportunities than would standardizations based on the 1962 regressions. Compare Hauser and Featherman (1974b, table 2).

of the total intercohort gain in occupational status. Coupled with rising socioeconomic levels of parents, this change could account for an upward status shift 148% as large as the observed intercohort shift. Thus, like blacks, whites have experienced changes in occupational opportunities which reflect more than compositional shifts in background and education. However, the net change for whites is a decline in mean occupational socioeconomic status. That is, white men of all ages in 1973 could expect to hold lower average socioeconomic statuses at each level of schooling and social background than their counterparts in 1962 (compare Hauser and Featherman 1974*b*). Unlike whites, blacks in 1973 did not have to acquire more education just to stay at the same occupational levels as same-aged men in 1962. Obversely, the schooling of white, but not of black, occupational incumbents was upgraded at each age between 1962 and 1973. We shall comment later on the significance of these differentials, especially since these shifts in occupational opportunities are coupled with intercohort increases in the occupational returns to each year of additional schooling for both whites and blacks.

Compositional differences in family background and schooling account for most of the racial differentials in mean occupational status in both 1962 and 1973, as shown in table 8. Here we standardize on the age-specific regression equations for whites from tables 5 and 6 for each year, inserting the black age-specific means into the white equations. The logic of these interracial comparisons is the same as in the decompositions of intercohort changes within races. As in previous use of this procedure (Duncan 1968*a*, 1968*c*; Hauser and Featherman 1974*b*), we interpret residual racial differences as conservative estimates of racial discrimination in the labor market.⁶ Obviously, "discrimination" measured in this way may be confounded with effects of omitted variables, such as differences between the races in culture or ability (to the extent that they are not represented by family background and educational attainment).

Racial differences in family background accounted for about an 8-point occupational status differential between black men in 1973 as in 1962 (see table 8). At no age in either year was this component of the racial gap larger than 9.5 SEI points or smaller than 7.0 SEI points. Differences in years of schooling accounted for smaller status differentials between the races in 1973 than in 1962. This is not a trivial finding, for the narrowed racial gap in schooling is partly compensated for in these calculations by the increased status returns to schooling in 1973 relative to 1962. The size

⁶ Use of regression equations for white men as the standard is conservative because the generally steeper slopes in white than in black equations lead to smaller estimates of racial "discrimination" than would standardization on the equations for blacks. Also, use of the white equations as the standard is more nearly correct, for response error leads to larger downward biases in regression coefficients estimated for black than for white men (Bielby et al. 1976).

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TABLE 8

COMPONENTS OF RACIAL SOCIOECONOMIC DIFFERENCES: MEN AGED 25-64 IN
THE EXPERIENCED CIVILIAN LABOR FORCE, BY AGE,
MARCH 1962 AND MARCH 1973

Age and Component	1962	1973
Total, 25-64:		
Family factors*	8.04 (37)	8.37 (50)
Education	7.90 (37)	2.55 (15)
Residual	5.54 (26)	5.90 (35)
Racial difference	21.48 (100)	16.82 (100)
25-34:		
Family factors	8.44 (38)	9.53 (70)
Education	5.06 (23)	-1.04 (-8)
Residual	8.57 (39)	5.15 (38)
Racial difference	22.07 (100)	13.64 (100)
35-44:		
Family factors	8.70 (41)	8.39 (50)
Education	6.85 (32)	4.39 (26)
Residual	5.87 (27)	4.15 (24)
Racial difference	21.42 (100)	16.93 (100)
45-54:		
Family factors	7.03 (34)	8.72 (44)
Education	6.58 (31)	5.88 (30)
Residual	7.31 (35)	5.10 (26)
Racial difference	20.92 (100)	19.70 (100)
55-64:		
Family factors	8.14 (37)	8.55 (43)
Education	7.74 (35)	6.90 (35)
Residual	6.07 (28)	4.46 (22)
Racial difference	21.95 (100)	19.91 (100)

NOTE.—Numbers in parentheses are percentages.

* Includes paternal (head's) occupational status and education, number of siblings, farm origins, and broken family.

of the racial gap in occupational status attributable to schooling varied directly with age in 1962. This age pattern was even stronger in 1973 because the effect of schooling on the occupational status gap declined most at the younger ages. Residual or discriminatory differentials in status between the races also declined inversely with age. Taken together, these two changes produced a direct variation of the total racial gap with age which was absent in 1962. These changes are obscured when the components are obtained for men throughout the working ages, for in the aggregate the effect of schooling differences on the racial gap has declined, but not the effects of discrimination and family background. These changes in the size of specific components of racial differences in occupational status have increased the relative importance of family background as a factor in the racial gap. In the aggregate, family background explained 37% of the

racial gap in 1962 and 50% in 1973. Similar shifts occurred at ages 35-64, and the change was even more marked at ages 25-34.

Last, what portion of the declining racial gap in occupational status (seen most clearly among men in their early work careers) is associated with changing differentials in family socioeconomic statuses and schooling? What portion represents "true" change in the occupational positions accessible to black and white men? Table 9 shows our analyses of these

TABLE 9
COMPONENTS OF CHANGE IN RACIAL DIFFERENCES IN OCCUPATIONAL STATUS:
MEN AGED 25-64 IN THE EXPERIENCED CIVILIAN LABOR FORCE,
MARCH 1962 AND MARCH 1973

Age and Component	SEI Points
Total, 25-64:	
Family factors*	0.95
Education	-3.32
Residual	-2.29
Net change	-4.66
25-34:	
Family factors	2.31
Education	-5.15
Residual	-5.59
Net change	-8.43
35-44:	
Family factors	0.15
Education	-5.39
Residual	0.75
Net change	-4.49
45-54:	
Family factors	1.08
Education	-1.46
Residual	-0.84
Net change	-1.22
55-64:	
Family factors	0.46
Education	-2.79
Residual	0.29
Net change	-2.04

* Includes paternal (head's) occupational status and education, number of siblings, farm origins, and broken family.

questions. We have used the age-specific regressions for whites in 1973 as the standard. Into these equations we have inserted the changes in the mean racial differences over the decade, following our earlier pattern. To interpret table 9, we observe that the racial gap in occupational status for ages 25-64 narrowed by 4.66 points between 1962 and 1973. We estimate

that the gap would have increased by about 1 unit on the SEI scale (0.95) if one took into account only the changes in racial differentials in family factors; but net of those changes, we note a decline of 3.32 points in the gap owing to shifts in schooling differentials. The difference between these net amounts and the observed total change of -4.66 SEI points is -2.29, or the decline in the gap attributable to changes in the differential occupational opportunities of black and white men. This aggregate decomposition is a bit misleading, for with the exception of men aged 25-34, change in the educational differences between the races is sufficient to account for the narrowing socioeconomic gap between black and white occupations. Among men aged 35-44, for example, a decline of 5.39 points is expected as a result of changing differentials in schooling, and this is larger than the observed decline of 4.49 SEI points. If changes in the educational differentials between the races serve to reduce occupational status differentials, the changing racial differences in family background serve to limit such improvements in the social standing of blacks. At every age the relative gains of whites in family contexts favorable to socioeconomic advancement partly offset the declines in racial occupational differences which stem from education.

For the youngest workers (aged 25-34), the shift toward greater educational equality also accounts for a large part of the declining occupational difference (5.15 SEI points), but it is smaller than the very large net decline in the occupational gap (8.43 points) at these ages. Only among the youngest workers did a decline in labor market discrimination appear to account for a substantial component (5.59 SEI points) of the reduction in black-white occupational status differentials. Interestingly, this is also the age at which the process of occupational stratification has shown the clearest signs of convergence between the races. Thus, changes both in levels of occupational standing and in processes of socioeconomic stratification have been most evident among young workers.

SUMMARY, INTERPRETATIONS, AND SPECULATIONS

In the decade between 1962 and 1973 both white and black males in the experienced civilian labor force enjoyed a general rise in the socioeconomic status of their occupations. Among whites, the gains were concentrated in the middle years of the work career; blacks in the early career experienced the largest improvements in average status. Relative to whites, black workers in 1973 had gained ground, closing the socioeconomic status gap by about 22%, with greater equality of attainments among men aged 25-34. Still, occupational statuses of blacks in 1973 fell below the average attainments of whites at every age in 1962.

Through the succession of cohorts, the socioeconomic circumstances of

black and white families of origin improved, as did levels of schooling, and this created more favorable environments for the social promotion of cohorts in the later period. These more favorable conditions for achievement do not account fully for intercohort shifts in occupational status for either race, indicating that occupational opportunities were changing.

Change in the process of stratification has followed different patterns for blacks than for whites. Having been reared in farm families represents less of an occupational handicap for both races in 1973 than in 1962, and the socioeconomic status returns to educational achievement for men of equivalent social backgrounds are greater in the 1970s than in the last decade. The increased occupational value of each additional year of schooling is more noticeable among blacks than whites and more evident among younger than older workers. Taken as a block, family factors play a somewhat less substantial role in the occupational attainments of whites than in 1962, and the relative importance of education (*vis-à-vis* the family) has increased. However, the occupational statuses of whites in 1973 vary more within levels of socioeconomic background and schooling than in 1962. Thus the process of occupational stratification has become more meritocratic and perhaps more random (with respect to the family and schooling) for whites. Schooling remains the single most important element of status allocation, and indeed the value of each additional year of education has increased for whites. At the same time, whites are unable to convert their educational attainments into occupational statuses at the same level as did men of equivalent schooling in 1962. Therefore, downward shifts in education-specific occupational attainments have occurred since 1962, even as the socioeconomic differentials between educational levels have risen.

If the process of stratification has become somewhat more random for whites over the decade, it has grown more deterministic for black men in the ECLF, as both socioeconomic background and, especially, schooling are more tightly linked to occupational statuses.⁷ Families and schools apparently have begun to function in the socioeconomic life cycles of blacks as they did for whites in earlier years. The greatest racial similarities in status allocation appear among workers in the early careers, the same group for whom the racial gap in occupational socioeconomic status has shrunk the most since 1962. Over the decade, increases in the value of each additional year of education have been large for blacks—nearly 50%. These substantial gains have not eliminated the racial difference in returns to schooling, but neither have blacks experienced the downward shift in education-specific occupational status which has occurred among whites.

⁷ Within these categories of family and schooling, however, occupational achievement was less determinate in 1973 than in 1962, even as in both years achievement was more determinate for blacks than whites.

The racial gap in mean socioeconomic status has declined, and similarities in the process of status allocation for young men of both races are greater, but blacks still experience occupational discrimination. There has been little change since 1962 in the percentage of the racial gap which we have designated as discrimination. Changes in educational differentials account for a significant portion of the declining gap at all ages. But it is among workers in their early careers that such compositional sources are least able to account for the narrowing (notable at ages 25-34) of the mean socioeconomic levels of the races. Among these young workers, changes in differential occupational opportunities, together with the near disappearance of racial differences in education, combine to reduce the occupational status differentials between whites and blacks.

What do these various trends and changes signify for racial or ethnic relations in the United States? Unfortunately, there is no simple answer. Even with respect to the limited issue of the "structural integration" (Hechter 1971) of blacks into the economy, the data are equivocal. On the one hand, the process of stratification appears to be moving toward a racially homogeneous pattern, as younger workers seem to be experiencing quite similar allocation from socioeconomic origins to schooling and then into the occupational hierarchy. Differentials in process and level of occupational attainment persist, even among the young, but gaps have declined and inequality of opportunity has diminished. Black families seem increasingly able to transfer their socioeconomic status to sons. Put another way, economic classes are more visible among the black population now than they were a decade ago. In addition, young black men have achieved near equality of schooling with whites, and relative to conditions for blacks over a decade ago, increments to regular or formal education provide better jobs at each level of schooling and for each additional year completed.

On the other hand, differentials in returns to education and family resources remain, as do gaps in average occupational status, especially among older men. Discrimination in the labor market, although perhaps smaller in absolute size, is not significantly less as a proportion of the total gap in occupational status than it was a decade ago. In addition, a more favorable socioeconomic position relative to whites has not led automatically to less discrimination against blacks in other components of the quality and style of life, as exemplified by the persistence of segregated housing. And, even though young blacks in the civilian labor force have gained ground on their white contemporaries, the likelihood of a young black being in the labor force was less in 1973 than in 1962. In sum, the evidence for a consistent trend in structural integration of the races is mixed. It confounds the always problematic associations among cultural, structural, and political integration (Hechter 1971) and makes predictions about change in racial relations impossible.

Surely racial stratification and inequality persist, even in these "post-industrial" United States. Even so, trends in stratification of the races reveal evidence for increased economic rationality which places constraints on the effective ability of the white majority to control the socioeconomic well-being of the black minority or to institutionalize the existing stratification system. Proponents of the thesis of industrialism (see Treiman 1970 for an overview)—the view that social change in the United States occurs primarily through industrial transformation and evolution—might be heartened by the diminished role of family factors as education becomes more effective in allocating men to occupational positions in the socioeconomic hierarchy. In that sense, stratification has grown more universalistic. The process is more rational in that, for example, it responds to larger cohorts of highly educated whites by raising the educational prerequisites for each occupation (see Smelser and Lipset 1966; Thurow and Lucas 1972). Educational upgrading of the occupational hierarchy in the last decade is consistent both with the view that credentialism is rampant (e.g., Berg 1970) and with the contention that economic change since 1962 has increased the premium for higher productivity.⁸ This premium takes the form of greater occupational and earnings differences among persons at each educational level than a decade earlier, as, for example, those with higher education are recruited into growth industries, especially in the tertiary sector (see Bell 1973, chap. 3).

Blacks have shared in these putative transformations of the economy and in the process of socioeconomic stratification. Because educational achievements of blacks became more important in status allocation over the decade, proportionately less of the variance we can explain in occupational attainment in 1973 reflects ascribed (family) factors. Increasing mean levels of schooling have not raised the educational prerequisites for

⁸ Competing explanations of these trends in terms of productivity versus credentialism effects are difficult if not impossible to adjudicate. We do note that for whites the predictive power (in R^2) of the family-plus-education equation is less for 1973 than for 1962. In addition, occupational inequality within categories of family and education has hardly changed for whites. Had credentialism grown as a tendency over the period, we might have expected (1) between-education variation to increase (it did) and (2) within-education variation to decrease (it did not). Were productivity relationships at work, we might have expected both within and between variation to rise, as both education and other skill-related characteristics (not indexed by formal schooling) have become more closely associated with occupational differences. The same line of argument leads to an expectation that on-the-job training and other skills have become more central in earnings differentials within jobs. We report on these analyses elsewhere (Featherman and Hauser 1976); here we merely note that male earnings were less determined by family, schooling, and occupational level in 1973 than in 1962 (controlling also for weeks worked), even as the (constant) dollar returns to each year of schooling have increased in the period. While somewhat equivocal in meaning, these data are not inconsistent with the view that productivity relationships, not credentialism, were the major force behind the rising returns to schooling between 1962 and 1973.

occupations for blacks as they have for whites: black men at each educational level were able to obtain higher-status jobs in 1973 than they could in 1962. At the same time, each increment of schooling brought greater returns than a decade ago. Blacks have become more internally differentiated by occupation, creating more distinctive economic strata within the race, with education serving as an effective mechanism allocating persons to jobs. The converging educational achievements of the races have provided a major impetus to the decline in occupational inequality between black and white men; and at the younger ages declining labor market discrimination has also narrowed the racial gap in occupational status.

In all of these contemporary shifts and changes in socioeconomic stratification we find little support for applying to ethnic relations in modern America a theory which casts the differential achievements of majority and minority groups in terms of "core" and "peripheral" classes or one which emphasizes racist tendencies, whether de jure or de facto, to the neglect of other tendencies in the allocation of statuses (cf. Hechter 1971, p. 42). Indeed, such theories are frequently designed to account for the stability of positions of super- and subordination rather than for change in the process of stratification. Our analysis of change in racial stratification shows that whites in 1973 faced less favorable circumstances than in 1962, especially as they attempted to convert their schooling into jobs and occupational statuses. In that respect, the relative improvement of the occupational standing of blacks has occurred at the expense of whites.

Whatever the source of ascendancy of whites over blacks, whatever the basis of current inequities in economic power, whites have not been able to monopolize the advantages of socioeconomic change since 1962. Stratification of the black population between generations is beginning to follow a pattern of relationships which tends to characterize majority populations in many industrialized nations (see Featherman et al. 1975 for a treatment of some of these commonalities). Meanwhile, white families have not effectively insulated their offspring from the occupational consequences of a burgeoning supply of highly educated workers. As parents of higher socioeconomic means become less able to guarantee the educational attainments of their offspring (Hauser and Featherman 1976) and as family factors grow less powerful in the occupational allocation of whites, the efficacy of both race and class as sources of status inequality declines.

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Economic Strains and the Coping Functions of Alcohol¹

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Some of the social and psychological antecedents of the use of alcohol to control emotional distress are examined. Interconnections are shown to exist among economic hardship, anxiety, and drinking for the relief of distress. These relationships are especially close among intensely anxious people having little sense of mastery and possessing low self-esteem. The results indicate that some functions of drinking stem from experiences and feelings rooted in basic features of social and economic organization.

Drinking is a multidimensional behavior, varying in such respects as frequency, quantity, the setting in which it is typically done, the occasions that give rise to it, and its many social and psychological consequences. In this paper our interest is confined to the use of alcohol as a mechanism for controlling anxiety. Of the innumerable conditions that potentially contribute to anxiety, we shall be concerned with but one: insufficient financial resources. Because so many life chances depend on the possession of economic resources, the scarcity of the latter can be an especially potent antecedent of anxiety.

The view that consumption of alcohol may serve as a mechanism for coping with anxiety aroused by economic conditions has a variety of conceptual and empirical underpinnings in the vast literature on drinking. Pivotal among them is the repeated finding from both the laboratory (Greenberg 1963) and the clinic (Alexander 1963) that alcohol in moderate amounts can alleviate the stressful impact of environmental threats through the metabolic and perceptual effects it produces in the organism (Washburn 1955; Chavetz, Demone, and Solomon 1962, pp. 6-8). The fact that alcohol can act as a tranquilizer for individuals should be reflected in broader relationships between the intensity of strains existing in a society and the level of alcohol consumption in that society. Such relationships, indeed, have been observed (Horton 1943, pp. 261-81), although it has been argued (Bales 1945, pp. 480-99) that the volume of alcohol consumption depends not only on extant strains, but also on the number of alternatives a society provides for dealing with tensions.

Such broad overviews of the interrelationships between cultural strains,

¹ We would like to acknowledge the assistance of Leslie Edelhoach in analyzing these data.

anxiety, and drinking have been supplemented by more recent systematic large-scale sample surveys of drinking practices (Riley and Marden 1947; Cahalan, Cisin, and Crossley 1969). One of the most important contributions of these surveys has been to establish that variations in drinking patterns correspond to lines of social structural demarcation. Such findings strongly suggest that if social strains do, in fact, help to regulate the use of alcohol, they will not be found to exist equally among people located differentially in society.

These findings and perspectives converge on the present study and help to bring into focus its purpose: to determine empirically whether drinking does function as a means for controlling and alleviating anxiety, especially that which arises from economic problems. It should be emphasized that it is not our intention to account for as many sources of anxiety as possible or to assemble all the possible reasons for drinking. Instead we intend to illustrate how the meaning and functions of drinking may be bound up in experiences and feelings that have their roots in basic features of social and economic organization.

BACKGROUND AND METHODS

The data analyzed in this paper are part of a larger investigation into the social origins of personal stress. The information was gathered through scheduled interviews with a sample of 2,300 people representative of the census-defined urbanized area of Chicago (U.S. Bureau of the Census 1972), which includes sections of northwestern Indiana as well as some of the suburban areas of Chicago. The sample was drawn in clusters of four households per block and used a total of 575 blocks, one-fourth the total sample of 2,300. The 1970 census reports that there are 2,137,185 households in the Chicago urbanized area (U.S. Bureau of the Census 1972); when this total is divided by the total number of blocks in which households were to be chosen (575), the result, 3,716, is the skip factor for the selection of households. That is, every 3,716th household was selected; the three additional households of the block cluster were then chosen by dividing the total number of households on each block by four and using the result as the factor for counting from the initially selected address. Among those contacted, 30% refused to be interviewed. In anticipation of refusals and to make allowance for households where contact could not be established within three callbacks, substitute addresses in each block were also prelisted. The targeting of households was thus entirely separated from the interviewing.

Two criteria were used in choosing respondents within sampled households. First, anyone younger than 18 or older than 65 did not qualify, for it was desired to have a sample weighted in favor of those actively engaged

in occupational life. Where more than one person in a household qualified as a respondent under the age limitations, the older candidate was systematically chosen. A second restriction concerned respondents' sex. The sex of the person to be interviewed in each household was predesignated so that the final sample would have as equal a number of males and females as possible. This restriction could be ignored only in instances where all age-qualified respondents were of the same sex. Because females typically head such households, the final sample contained more women than men.

The interview schedule was designed to yield several distinct types of information. First, it asked people about potential strains—that is, conflicts, frustrations, and threats—that earlier exploratory interviews had revealed to be commonly experienced. It was particularly concerned with the strains that occur in the major social role areas of occupation, marriage, child rearing, and economic life. Second, the interview included a number of questions about the coping strategies and coping resources people employ in dealing with the strains they experience in these roles. And third, it inquired into the emotional stresses that they feel and the extent to which they experience symptoms of various states of psychological disturbance, such as depression and anxiety. The present report, although it brings together only a small portion of the data, cuts across several of these areas.

DISTRESS CONTROL AS A FUNCTION OF DRINKING

The fact that the inquiry into drinking practices is part of a more inclusive study of social stress had considerable influence on the questions asked about drinking. As we noted earlier, drinking is a complex behavior that can be approached from many directions and described at many levels. In surveys dealing exclusively with drinking, considerable time is required simply to gather information pertaining to its elementary aspects, such as what, when, where, with whom, and how much people drink. It was necessary to be more selective here. In keeping with the broader goals of the inquiry, our questions were limited to learning whether respondents were abstinent or not and, if not, the extent to which drinking was part of their social life-style, how often in the past month they had drunk to inebriety and, finally, some of the manifest consequences that individuals attributed to their own drinking behavior. It is with the last mentioned data that this paper is primarily concerned, particularly those consequences indicative of the control of anxiety.

It is difficult to determine when drinking is used by people to control psychological distress, for there is nothing about alcohol consumption itself that indicates the reasons for or consequences of its use. In this respect

it is similar to a variety of behaviors, such as television viewing (Pearlin 1959), that may be employed as coping devices by some people but not by others. Since the behavior alone cannot provide a clear indication of how it is being used, it is necessary to ask the subject himself what drinking "does for him." A question adapted from the work of Cahalan et al. (1969, p. 242) was used for this purpose. Respondents, except those who had indicated previously that they were teetotalers (614, or 27% of the total sample), were presented with this item: "Here are different reasons people have for drinking. Thinking of yourself and your reasons, how true are [sic] each of these reasons for you personally?" Eight statements were then given, each answered by "very true," "true," or "not true." Only two statements will figure in this analysis: "A drink helps me to forget my worries" and "A drink helps cheer me up when I am in a bad mood."² A score of distress control was created simply by according a value from one to three to the response categories. Thus, people who responded "very true" to both statements have a score of six, and those answering "not true" to both have a score of two.

It needs to be recognized that the thrust of the questions is aimed at consequences of drinking about which the person himself has some consciousness. The measure, therefore, deals only with a single manifest function of drinking and encompasses neither latent functions outside the awareness of drinkers nor manifest functions other than the use of alcohol to assuage emotional distress. It is difficult to be certain, of course, whether the functions an individual recognizes are among those actually performed by his drinking. In this connection, however, it is relevant to note that there is a substantial rise in the frequency of reported inebriation as the disposition to use alcohol for the control of distress increases ($P < .001$).

Finally it should be emphasized that the index reflects the use of alcohol as a means for the temporary containment or diminution of an existing noxious emotional state. This coping behavior is thus an effort to control distress after it emerges and is very different from behaviors aimed at eliminating conditions underlying the distress. Clearly, the use of alcohol is at most a means of appeasing unpleasant emotions currently being experienced and probably leaves intact the situations out of which such emotions have grown.

²The remaining six statements are as follows: a drink helps me to relax; I drink to be sociable; a drink helps me when things go wrong; a drink helps me gain self-confidence; a drink helps me when I am lonesome; and I drink when I am bored. Drinking for relaxation and for sociability are so common that responses to these two items have very little variation. It should also be noted that the antecedent conditions for the statements above are for the most part different from those underlying responses to the two statements used in our index of distress control.

ECONOMIC STRAINS AND ANXIETY

In concentrating on economic antecedents of distress, we are being very selective, for there is a plethora of conditions with the potential for arousing emotional disturbance. However, there are strategic advantages in emphasizing economic circumstances. One is that problems in this area have an extraordinary capacity to provoke distress. Because so many fundamental activities and life chances depend on the economic resources people possess, deprivations and difficulties in this domain are not likely to be borne without emotional repercussions. Also, economic life circumstances typically resist efforts toward rapid improvement; problems that exist here, therefore, are often more persistent and durable than ones stemming from other sources. As a consequence of the persistent quality of economic problems, people tend to develop equally persistent ways of coping with them, such as habitual reliance on alcohol. If environmental strains, inner distress, and drinking behavior are interrelated, their connections should be especially stable and amenable to observation when the strains involve economic problems.

We assess a family's economic resources in two ways, one of them being total family income. Income information, however, is at best incomplete, for it does not take into account the fact that the same level of income reflects a different resource for families differing in size and age composition. The second indicator avoids problems of equivalence by focusing on the actual economic strain people experience, without regard to their incomes. It involves a three-part question: "How often does it happen that you do not have enough money to afford: 1) the kind of food you (your family) should have? 2) the kind of medical care you (your family) should have? 3) the kind of clothing you (your family) should have?" Each item was answered by "never," "once in a while," "fairly often," or "very often," and a total score was formed by adding the values of the three responses. This measure taps the intensity of the effort in which one has to engage simply to meet the fundamental economic requirements of life. There are families with fairly substantial incomes whose scores on the measure reveal severe economic strain, especially families with three or more children. Nevertheless, there is a close association between family income and our direct measure of strain, with strain intensifying as income declines.

How do these indicators of economic hardship relate to psychological distress, anxiety in particular? Anxiety is measured here by a set of items that is fairly standard in surveys. They ask respondents how frequently they experience 12 common psychophysiological symptoms of anxiety (Lipman et al. 1969; Derogatis et al. 1971). Table 1 is divided into two parts, showing separately the relationship of income and of economic strain to anxiety among nontotalers. The difference in the total number of

TABLE 1
ECONOMIC HARDSHIPS AND ANXIETY (%)

	ANXIETY			N	Total
	Intense	Moderate	Low		
Family income per year (\$):					
Less than 6,000	33	27	40	221	100
6,000- 9,999	18	29	53	265	100
10,000-13,999	19	29	52	427	100
14,000-17,999	17	29	54	274	100
Over 18,000	13	26	61	309	100
$\lambda^2 = 66.9; 8 \text{ df}; P < .001$					
Economic strain:					
Severe	41	27	32	99	100
Moderate	35	32	33	145	100
Little	24	34	42	234	100
None	16	26	58	1,191	100
$\lambda^2 = 84.8; 6 \text{ df}; P < .001$					

respondents reporting in each part results from over 300 respondents having been either unable or unwilling to provide income information. It is apparent from the results of the table that economic hardships, whether gauged by income or by felt strain, are significantly associated with anxiety. This element of psychological distress, therefore, does not arise simply from chance events impinging on the lives of people randomly located in the society, but is in part a consequence of distinctive circumstances experienced at different economic levels.

ANXIETY AND DISTRESS CONTROL

We have seen that limited incomes and economic strains contribute to anxiety. Once this psychological state is aroused, how do people deal with it? Drinking, we have been assuming, is one mechanism that may be employed to control anxiety, and the more intense the anxiety, the greater will be the tendency to use alcohol in this way.

In table 2 we see how anxiety is associated with the use of alcohol for distress control. The table reveals two findings of interest. First, there are many anxious people who do not place drinking in the service of distress management, and this is something we shall consider in depth shortly. The second finding is apparent in spite of the first: the more intensely one experiences anxiety, the more likely one is to use alcohol to control the anxiety.

These results, then, together with earlier findings, suggest the presence of a set of interlocking conditions converging on drinking behavior: because

TABLE 2

LEVEL OF ANXIETY AND USE OF ALCOHOL FOR CONTROL OF DISTRESS (%)

DISPOSITION TO USE ALCOHOL FOR DISTRESS CONTROL	ANXIETY		
	Intense (336)	Moderate (470)	Low (865)
Strong	23	14	12
Weak	18	16	16
Minimal	59	70	72

$$\lambda^2 = 24.4; 4 \text{ df}; P < .001$$

NOTE.—N's in parentheses.

of the crucial stakes that depend on economic resources, hardship in this area is likely to generate a high level of anxiety, and once they are host to intense anxiety, people become more disposed to use alcohol as a temporary escape from the unpleasant burdens of their distress. Thus drinking has meanings and functions traceable to the life circumstances of people and to the threats to their psychological well-being posed by these circumstances.

ANXIETY AND THE FUNCTIONS OF ALCOHOL: INTERVENING CONDITIONS

We noted concerning table 2 that in spite of the significant relationship between anxiety and drinking for distress control there is a substantial number of drinkers whose anxieties are at an intense level but who do not use alcohol to allay their tensions. Obviously, drinking has no coping functions for many imbibers who harbor intense anxiety. In order to understand more fully the use of alcohol for the control of distress, therefore, it is necessary to take into account conditions that serve to make drinking an attractive coping technique for some distressed people but not for others.

Some of the factors influencing the ways people attempt to cope with anxiety are rooted in personality. Especially crucial in this regard are personality characteristics that reflect self-attitudes. One of these concerns mastery, which here refers to the extent to which people see themselves, at one extreme, as being in control of the important circumstances of their lives or, at the other extreme, as having to submit fatalistically to external forces. One's standing on this dimension, we submit, can have a great deal to do with where he directs his coping efforts. When confronting distressing circumstances, a person in possession of a sense of mastery will attempt to deal with his distress by manipulating the circumstances. A person lacking this attribute is more likely to rely on devices that help him live with distress and to accept as inevitable the circumstances causing it. Let us see

whether drinking may be one such device whose use varies according to a sense of mastery.

Table 3 is arranged to show how the relationship between anxiety and the use of alcohol for distress control is regulated by different conditions of mastery.³ Although we know from table 2 that this use of alcohol is most apt to be found among people who are intensely anxious, table 3 shows that the level of anxiety is especially closely related to distress control under conditions of limited mastery. Indeed, the gamma coefficients, which are presented to facilitate the comparison of the three conditional relationships, indicate that anxiety has no significant or appreciable association with drinking for distress control among people possessing either a moderate or a great sense of mastery. When people are both intensely anxious and lacking in mastery, it is readily apparent that they are outstandingly more disposed to use drinking as a coping mechanism than are people with similarly intense anxiety who see their world as amenable to their control. Clearly, the relationship between anxiety and the coping functions of alcohol is mediated by sentiments of personal efficacy.

This kind of sentiment, furthermore, is connected to economic circumstances in a way that has crucial implications for the entire process of distress arousal and coping repertoires. We find, specifically, that the sense of mastery declines as income declines ($P < .001$) and as economic strain rises ($P < .001$). It should also be noted that mastery is even more substantially related to the level of educational attainment than to the level of income, suggesting that mastery probably does not result directly from the possession of money but indirectly from opportunities and achievements that are concomitants of economic resources. Whatever the linkages between economic factors and mastery, the central point of the findings is that the people who are most exposed to conditions provocative of strain and distress may also be those least in possession of an important psychological coping resource. Thus, it is the poor who are most likely to have to bear the burdens of anxiety that result from economic threat, and it is also the poor who are least likely to have the experiences and opportunities that contribute to a sense of mastery, a characteristic that serves to reduce potential reliance on alcohol as a distress management device. These inter-

³ The measure of mastery is derived from seven items, each responded to by "strongly agree," "somewhat agree," "somewhat disagree," or "strongly disagree." The scores ranged from 4 to 28, with people scoring from 4 to 17 classified as having limited mastery, from 18 to 23 as having moderate mastery, and from 24 to 28 as having great mastery. The seven items are: there is really no way I can solve some of the problems I have; sometimes I feel that I'm being pushed around in life; I have little control over the things that happen to me; I can do just about anything I really set my mind to do; I often feel helpless in dealing with the problems of life; what happens to me in the future mostly depends on me; and there is little I can do to change many of the important things in my life.

TABLE 3
MASTERY, ANXIETY, AND DISPOSITION TO USE ALCOHOL FOR DISTRESS CONTROL (%)

Disposition to Use Alcohol for Distress Control	LIMITED MASTERY			MODERATE MASTERY			GREAT MASTERY		
	Intense Anxiety (95)	Moderate Anxiety (67)	Low Anxiety (111)	Intense Anxiety (169)	Moderate Anxiety (229)	Low Anxiety (356)	Intense Anxiety (72)	Moderate Anxiety (174)	Low Anxiety (398)
Strong	39	22	19	17	16	12	14	8	10
Weak	10	13	14	23	18	21	18	16	12
Minimal	51	64	67	60	66	67	68	77	78
	$\lambda^2 = 11.5; 4 \text{ df}; P < .02$ Gamma = .24			$\lambda^2 = 4.6; 4 \text{ df}; N.S.$ Gamma = .09			$\lambda^2 = 5.8; 4 \text{ df}; N.S.$ Gamma = .09		

NOTE.—N's in parentheses.

locked circumstances impose a double penalty on those with restricted economic resources, first, by exposing them more fully to anxiety-producing conditions and, second, by providing them with fewer psychological resources that would help orient them directly toward the alteration of these conditions. The deprivations of the poor occur on many levels of experience.

Similar results appear with a second self-attitude, self-esteem. For our assessment of self-esteem we have adopted the Rosenberg scale (1965, pp. 305-7), a measure of the extent to which one holds a positive attitude toward himself. Table 4 shows that the largest proportion of those strongly disposed to put drinking in the service of distress control are those who have both intense anxiety and low self-esteem. Even the disposition of intensely anxious people to use drinking as a coping device diminishes as self-esteem rises. Thus, as in the case of mastery, the relationship between anxiety and drinking practices is greatest among those whose self-esteem is lowest. As the level of self-esteem rises, the magnitude of this relationship shrinks. It is apparent that self-esteem enables one to bear a burden of anxiety without turning to practices that blunt awareness of the burden. Self-esteem may not always succeed in allaying anxiety, but it does appear to help one to tolerate the distress without the same need to escape from an awareness of it that is suggested by the drinking practices of highly anxious people with low self-esteem.

Self-esteem also has a close relationship to the level of income, just as mastery does. Similarly, we find that self-esteem varies closely with educational attainment, as it did with mastery. Consequently, once more the group most lacking in economic resources and the achievement opportunities associated with such resources is also most lacking in a personal characteristic that helps to provide coping options. The very people most entrapped by the process leading to the use of alcohol to control distress are also least in possession of a personality attribute that could aid their discovery of alternative ways of dealing with distress.

Intense anxiety, in sum, is especially likely to result in the use of alcohol as a tranquilizer if a sense of personal efficacy is lacking and self-esteem is low. Each of these conditions, furthermore, is related to the economic circumstances that impinge on people. Indeed, because of the network of interrelationships among the conditions we have been examining, it is probable that they have an overlapping influence on ultimate drinking practices. Through regression analysis it is possible to evaluate more directly the separate contribution that each of the major conditions we have been examining makes to the dependent variable.

The original zero-order correlation between economic strain and drinking is 0.09. By itself it is rather modest, though significant. But as the mediating conditions are added sequentially to the regression equation,

TABLE 4
SELF-ESTEEM, ANXIETY, AND DISPOSITION TO USE ALCOHOL FOR DISTRESS CONTROL (%)

DISPOSITION TO USE ALCOHOL FOR DISTRESS CONTROL	LOW SELF-ESTEEM			MODERATE SELF-ESTEEM			HIGH SELF-ESTEEM		
	Intense Anxiety (114)	Moderate Anxiety (83)	Low Anxiety (98)	Intense Anxiety (81)	Moderate Anxiety (115)	Low Anxiety (145)	Intense Anxiety (141)	Moderate Anxiety (272)	Low Anxiety (622)
Strong	37	29	17	20	20	17	13	7	10
Weak	18	14	26	22	18	15	15	16	14
Minimal	45	57	57	58	62	68	72	77	76
	$\lambda^2 = 12.0; 4 \text{ df}; P < .02$ Gamma = .20			$\lambda^2 = 2.6; 4 \text{ df}; \text{N.S.}$ Gamma = .11			$\lambda^2 = 5.2; 4 \text{ df}; \text{N.S.}$ Gamma = .02		

NOTE.—N's in parentheses.

the multiple correlations increase. Thus, when economic strain and anxiety are found together, as we know is likely, their joint correlation with drinking for distress control rises to 0.15. By adding to the regression equation mastery and self-esteem, the two personality resources that have been shown to act as barriers to drinking for the control of anxiety, the multiple correlations are elevated to 0.21 and 0.25, respectively. Two interaction terms, mastery with anxiety and self-esteem with anxiety, provide an additional small increment, together raising the correlation to 0.27. When anxiety is aroused by economic hardship, then, the chances that escape drinking will result are substantially increased; when economic deprivations and anxiety are in the presence of limited mastery and low self-esteem, these chances are further advanced to three times the original correlation.

It is readily apparent from the magnitude of these correlations that conditions other than those examined here also have a major part in accounting for escape drinking. But from those that were considered, it is equally apparent that there is an interlocking set of economic, social, and psychological conditions that both contribute to the arousal of anxiety and channel behavior to drinking as a means of coping with it.

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Productivity, Favor, and Grants among Scholars¹

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Research grants are resources in academic stratification that may be distributed by performance criteria or by particularism or favor. This paper explores determinants of grant getting, using a subsample drawn from a 1972-73 general national survey of faculty in higher education. The findings show that grant getting depends primarily on productivity, secondarily on the inequalities in favor shown to specific fields, and very little on particularistic situational and personal status factors. The inevitable inequality of this meritocracy apparently is lessened by broad participation in grantsmanship.

Research on stratification in science has resulted in a cumulative body of findings focusing on "the recognition of excellence of achievement" (Merton 1973, p. 435) on the basis of intense competition (Hagstrom 1974) shaped by meritocratic universalism (Cole and Cole 1973). Central to the process leading to such recognition are authoritatively monitored media for evaluating scholarly work. Inequalities in the resources or attainments of scholars are professionally acceptable if they result from inequalities in knowledge produced. These principles explain aspects of academic stratification at several levels of generality, for example, recognition within specialized subfields (Gaston 1973), career role and status change within broad disciplinary fields (Allison and Stewart 1974), and local institutional reinforcement for productivity among all scholars (Blau 1973).

Surprisingly little of this type of research deals with the role of grants in academic stratification. While the "merit" theory has remained untested as it pertains to grantsmanship, a less developed "favor" theory has had some acceptance, especially in the defining of policy issues. Lay funders, for example, have recently (Walsh 1975) questioned the policies implied in the use of peer review in the distribution of grants and have rekindled suspicions that institutions rich in resources and entrepreneurs with special connections are favored. The extent and benefits of the favor shown to some fields are also at issue in policy circles (Culliton 1975). These issues raise empirical questions regarding (1) the effect of the priorities assigned to various fields by funding agencies on the overall production of knowledge and (2) the relative impact on individual grant getting of interfield or

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other types of favor compared with the impact of each scholar's demonstrated productivity.

This paper examines determinants of grant getting, using data from a recent national survey of faculty members. The findings are limited in two major respects. The available data pertain only to numbers of grants received, not to the size or substance of grants or to attempts to secure grants, and the focus in this paper is on all institutions and all fields rather than on intrafield patterns.

THE SAMPLE, DATA, AND VARIABLES

In 1972-73, the American Council on Education (ACE) conducted a stratified sample survey of faculty in 259 senior colleges and universities (on sampling and weighting, see Bayer [1973]). A 49% response rate yielded 40,421 teaching faculty respondents from these institutions. For the present study, I used a 15% subsample to reduce costs of data processing. The subsample was selected to approximate a simple random sample of the universe (see Liebert [1974] for procedures), thereby dispensing with weights. I focus solely on full-time university and four-year college faculty members with postsecondary degrees. Of the 5,687 such respondents in my subsample, 309 had held their highest degrees for only two years or less at the time of the survey; they were dropped to remove untested neophytes. I deleted another 431 cases because of missing data on productivity or grants, leaving 4,947 cases.

The ACE questionnaire (reprinted in Bayer [1973]) included a list of granting agencies on which respondents were to check each source of support for "scholarly work and research" in which they had engaged as principal investigators (PI) during the previous 12 months.² The list comprised 17 federal agencies and five nonfederal sources (table 1, note *). The number of sources checked provided the best available indicator of grant getting. Available indicators of productivity derive from two ACE items for which respondents checked precoded categories for (1) total number of articles ever published in professional media ("career articles") and (2) manuscripts of any type published or accepted for publication within the preceding two years. Approximations of underlying interval scales were developed by assigning respondents the mid-points of the categories they checked, with the exception of the open-ended highest category for each item. Respondents who checked the highest category for either item were assigned its lower bound.³ Other variables include such possible deter-

² Fewer than 5% of all respondents claimed to have grant support only as non-PIs. They are not counted as grant getters.

³ Relative to the underlying interval measures, the categorized and quasi-interval measures result in loss of information. The amount lost, or unreliability, is greater

minants of favor as personal and institutional status, location, and wealth.

Initial analysis showed that grants are not equally available from all agency sources or to all fields. Yet grant getting was sufficiently extensive to indicate that grants are not distributed merely to a few elite groups. Among all scholars in the subsample, 34.5% were grant-supported PIs in 1972-73 (21.5% in four-year colleges; 44.4% in universities).⁴ This surprisingly broad-based distribution deflates expectations of any findings showing severely selective elitist particularism or meritocracy.

Contributing to broad distribution is the considerable variability among agency sources with respect to the competitiveness of the grants they award. Table 1 shows this in two ways, first by an eight-row summary of

TABLE 1

ASSOCIATION (GAMMA) BETWEEN GRANT GETTING AND PRODUCTIVITY, AND PERCENTAGE OF GRANT GETTERS WHO ARE HIGHLY PRODUCTIVE, BY TYPE OF GRANTING AGENCY

GRANTING AGENCY*	GAMMA BETWEEN GRANT GETTING AND:		PERCENTAGE OF GRANT GETTERS HAVING:		N GRANT GETTERS
	Career Articles†	Recent Pubs.‡	11 or More Career Articles	5 or More Recent Pubs.	
NSF712	.713	73.3%	49.2%	257
HEW679	.713	70.4	46.7	262
DEFENSE730	.697	64.7	45.7	149
OTHFED449	.561	51.6	30.7	263
FNDTN523	.622	57.6	42.8	238
INDUSOTH383	.440	45.6	26.9	376
STATELOC410	.466	44.1	26.9	287
INTRAMUR395	.498	41.5	26.6	727
Major league655	.712	60.8	38.0	925
Minor league427	.518	41.1	24.3	1,197
All faculty580	.672	46.4	27.3	1,706
Total Faculty (N)	4,947	4,947			

* Agencies are defined as follows, with numbers in parentheses enumerating the 22 agency categories on the ACE questionnaire: NSF = (1) National Science Foundation; HEW = U.S. Dept. of Health, Education, and Welfare, including (2) FDA, (3) OE, (4) NIMH, (5) NIH, and (6) other HEW; DEFENSE = (7) U.S. Defense Dept., (8) Atomic Energy Commission, (9) National Aeronautics and Space Administration; OTHFED = U.S. Dept. of (10) Agriculture, (11) Commerce, (12) Interior, (13) Labor, and (14) Transportation, (15) National Endowment for the Arts and Humanities, (16) Office of Economic Opportunity, and (17) other federal agency; FNDTN = (18) private foundations; INDUSOTH = (19) private industry and (20) all other not elsewhere itemized; STATELOC = (21) state or local government; INTRAMUR = (22) institutional or departmental funds (intramural). The major league contains NSF, HEW, DEFENSE, OTHFED, FNDTN. The minor league contains INDUSOTH, STATELOC, INTRAMUR.

† Gammas were computed for the precoded seven-point ordinal scaled career articles variable with dichotomous grant-getting variables.

‡ Gammas were computed for the precoded five-point ordinal scaled recent publications variable with dichotomous grant-getting variables.

at the upper ends of the underlying variables than at the lower ends, for the precoded categories were broader at the upper ends.

⁴ Assuming random sampling and noting the large sample size, confidence intervals for these and subsequent percentages fall within very few percentage points of the reported estimates.

the agencies involved and second by a division of the agencies into two broad groups. All types of agencies selectively reward or compete for faculty with high demonstrated productivity, but some agencies award more grants to low producers than do others. By this standard the more provincial grants (intramural, state and local government, industrial, "other") appear to be the least competitive. In subsequent analysis, grants from these sources were summed in one variable called "the minor league." Grants from federal and foundation sources were also grouped, forming a variable called "the major league." In regression analyses discussed below, each grant-getting variable (all grants, major and minor) is the natural log of the number of grants received in the respective category minus one.

To determine the effect on grant getting of the priorities assigned by grantors to particular fields, a measure of the availability of resources to each field ("field resource availability") is needed. Twenty-seven fields, each containing at least 50 sampled scholars, were identified by major of highest degree. For each field, resource availability is the ratio of the number of grants received to the number of scholars sampled. Fields with the highest ratios on the variable representing all grants were the physical and biological sciences (0.8–1.6 grants per scholar). In the middle were the social sciences, while the low-ratio fields (under 0.3 grants per scholar) were the fine arts, the humanities, and lesser professions such as nursing and social work. Similar patterns of ratios obtained in both major and minor leagues. This interfield inequality of resources may affect the grant-getting prospects of individuals either directly, as a result of the extent of favor shown to their fields ("field favor"), or indirectly, by supporting higher productivity in grant-rich fields.

FIELD FAVOR, PRODUCTIVITY, AND GRANT GETTING

A two-stage causal path model is assumed for the two ways in which field resource availability might determine individual grant getting. The indirect route by way of individual productivity is in effect a path of resource-aided meritocratic competition: the resources traditionally invested in fields facilitate greater productivity, and the more productive get grants. The second causal path, direct and undecomposed, runs from field resources to grant getting without any intervening explanation other than that some fields are favored.

The two intervening variables on the indirect route, career articles and recent publications, were found to have nonlinear relationships with grant getting. Plots of grants received (all grants, major league grants ["majors"] and minor league grants ["minors"], both in raw score and in log N form) against the quasi-linear publications variables showed that increments in grants declined with each rise in the level of publications. In this study,

these relationships are specified in linear regression by the use of natural log transformations of the two publications variables.⁵ Of the two, I take recent publications to be a more direct determinant of grant getting because of the withdrawal from research associated with aging (Zuckerman and Merton 1973). Unfortunately, current grants could also cause recent productivity instead of always being caused by it, thereby misleadingly inflating the one-way causal effect of interest here. It was found, however, that deletion of the recent publications variable reduces explained variance by up to only five percentage points, transfers to career articles most of the effect of recent publications, and makes substantively minor and generally insignificant changes in the effects of other variables.

The analysis as developed in path model form is summarized in table 2.⁶ Direct effects are reported as standardized coefficients in regressions of grant getting, recent publications, and career articles on their predetermined (independent) variables. Using procedures advanced by Alwin and Hauser (1975), indirect effects were calculated and reported along with total effects.

Note initially the *total* effect of interfield resource inequality on individual grant getting in the model for all grants. It is greater than the total effect of publication productivity. The pattern is the same for the majors and the minors, even though the coefficients become smaller. However, a portion of the interfield priority effect is due to the higher productivity that comes from being in resource-rich fields. Let us take this step by step.

First, productivity of articles over the length of a career is substantially affected by the level of field resources. This effect is greater for the majors (.421) than for all grants (.400) or the minors (.319). To different degrees depending on type of grant, the outcomes of field resources involve stronger effects on supporting career productivity than on merely raising the prospects of individual grant getting. The career articles variable has, in turn, a huge impact on recent publications, with path coefficients of about .73. Nearly the entire total effect of resources on recent publications is indirect by way of career articles, suggesting that field resources affect productivity principally by encouraging more work by the most productive. Finally, as expected, recent productivity (and to a lesser extent productivity during the whole career) has positive effects on individual grant getting.

The total effect of interfield resource inequality on grant getting prospects is, then, partially mediated by higher productivity in the better supported fields. About one-third of this total effect is so mediated for the

⁵ Slopes for productivity with grant getting may (and do) differ by field. This is not at issue here where we are concerned with the aggregate pattern. Additional work in progress deals with differing *intrafield* patterns.

⁶ Means, standard deviations, and correlations are available from me. Other approaches to detecting field effects are possible; e.g., the analysis of covariance (see Hauser 1970).

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TABLE 2

EFFECTS IN A MODEL OF GRANT GETTING, FOR ALL GRANTS, MAJORS, AND MINORS
($N = 4,947$)

DEPENDENT VARIABLE (<i>R</i> ²), AND PREDETERMINED VARIABLE	TOTAL EFFECT	INDIRECT EFFECTS VIA:			DIRECT EFFECT
		Career Articles	Recent Pubs.		
Model for All Grants					
Career articles (.160):					
Field resources400400
Recent pubs. (.731):					
Field resources299	.284015*
Career articles727727
All grants (.328):					
Field resources416	.138	.003*		.275
Career articles349255		.094
Recent pubs.344344
Model for Major League					
Career articles (.177):					
Field resources421421
Recent pubs. (.732):					
Field resources308	.307001*
Career articles731731
Major grants (.278):					
Field resources387	.134	.002*		.251
Career articles312211		.101
Recent pubs.303303
Model for Minor League					
Career articles (.102):					
Field resources319319
Recent pubs. (.730):					
Field resources247	.217030*
Career articles726726
Minor grants (.160):					
Field resources310	.071	-.002*		.241
Career articles206169		.037*
Recent pubs.233233

NOTE.—See text for definitions of variables.

* Not significant at $P \leq .005$.

majors and for all grants, and about one-quarter for the minors. These are summary estimates of the degree to which overall interfield distributional policies fit the universalistic competitive norm of cumulative advantage through rewards to the productive and productivity among the rewarded.⁷ This process accounts for some, but no more than one-third, of the total effect produced on grant getting by distributional inequality among fields, leaving the larger direct effect to field favor.

⁷ Note that this is not inconsistent with Merton's (1968) notion of the "Matthew effect" or what is elsewhere (Allison and Stewart 1974) analyzed as cumulative advantage in career-phased meritocratic stratification.

Finally, concerning *direct* effects, there is somewhat stronger evidence of competition on grounds of merit than of favoritism between fields. Specifically, individual productivity has a stronger direct impact on individual grant getting for all grants ($P = .344$) than does field resource availability ($P = .275$). A similar difference, but with smaller coefficients, is found for the paths to the majors. For all grants and the majors, a small direct effect of career articles adds to the overall effect of productivity in general. It may be concluded that in grant getting greater direct influence is exercised by individual productivity than by all of the residual advantages of being in fields favored by agencies.

The reverse of these findings holds in the minor league. In the awarding of the more local grants, for which there is less competition, favor of fields slightly exceeds productivity in direct effect. Both factors together explain much less variance in minor league grant getting ($R^2 = .160$) than they do in the majors ($R^2 = .278$) or for all grants ($R^2 = .328$). Apparently the minor league is not only guided slightly more by field favor than by productivity criteria, but is also subject to substantial idiosyncratic decision making unrelated either to fields or to productivity.

ANY OTHER FAVOR?

Other possible bases of favor on which data were available include several indicators of institutional status (from Creager and Sell [1969], updated through 1974): percentage of faculty with doctorates, undergraduate admissions selectivity, number of periodicals in library, revenue per student, percentage of revenue allotted to research, and dummy variables for affiliation with a university, a private institution, and a traditionally Negro school. Zero order correlations with all grants ranged from .25 for the university dummy to $-.10$ for the private school dummy. Possible regional favor was indicated by four dummy variables for schools in the Northeast, Southeast, Midwest, and West. None of the 12 zero order correlations of region with grant getting exceeded $\pm .06$.

Several personal status variables (all from the 1972-73 ACE faculty survey) indicate other possible bases of favor. Number of years since highest degree ("career age"), when taken separately from productivity, denotes the favor accorded to length of service alone. A doctorate, though an achievement of merit, is also a professional union card which could be used to the disfavor of productive non-card holders. Dummy variables for respondent's sex (1 = female; 0 = male) and race (1 = black; 0 = not black) were also included. Finally, attempts were made to assess the effects on grant getting of what might best be called "fiscal entrepreneurship" and the power of personal contacts, indicated by self-reported nine-month salary and by two dummy variables, one for being a paid consultant

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and the other for ever having had nonacademic work experience in research or administration. These personal status variables had zero order correlations with all grants ranging from .28 for having a doctorate to -.18 for being female.

Overall, none of the zero order effects on grant getting of any of these 19 variables was larger than the smallest of the total effects of field resources or productivity reported in table 2. Third order partial correlations, with field resources and the productivity variables controlled, further reduced most coefficients to insignificance at the .005 level. The meager evidence of favor is efficiently summarized in table 3, showing full regression models containing all significant independent variables.

TABLE 3

STANDARDIZED REGRESSION COEFFICIENTS AND R^2 IN REGRESSION OF GRANT GETTING ON ALL INDEPENDENT VARIABLES, FOR ALL GRANTS, MAJORS, AND MINORS ($N = 4,947$)

INDEPENDENT VARIABLES*	STANDARDIZED REGRESSION COEFFICIENTS†		
	All Grants	Major League	Minor League
Field resources269	.248	.237
Recent pubs.284	.261	.219
Career articles093	.096	...
Career age	-.082	-.047	-.062
Paid consulting092	.064	.082
Nonacademic research or administrative work047045
Percentage of faculty with doctorate (institution)054054
Revenue per student (institution)045	.116	...
Periodicals in library (institution)040
R594	.544	.422
R^2352	.296	.178

* See text. Only variables attaining significance for partials at the .005 level were retained in the final models presented here. Variables deleted for lack of significance include dummies for university, private, and Negro institutions; admissions selectivity and percentage of revenue for research; region (dummies for Northeast, Southeast, Midwest, with West suppressed); salary; and dummies for doctorate, female sex, and black race.

† All are significant at $P \leq .005$.

It is clear that we have found very little that makes much difference in grant getting other than the field and productivity variables. Even university status and the percentage of institutional revenue allotted to research do not directly influence the nationwide distribution of grants to faculty. There is a slight tendency for faculty members with contacts acquired through consulting or relevant prior work to get grants regardless of productivity or field, and for those at wealthier and better staffed institutions to be favored. But these weak relationships are insufficient to support claims of elite institutional privilege or of a need to have contacts in order to get grants. The slight distributional favor for young faculty members even suggests a small measure of counterelitism.

In general, we have evidence of a broadly based system for the distribution of research grants that is more competitive with regard to individual productivity criteria than it is biased by field favor. There is very little evidence of situational or personal particularism in the all-faculty nationwide data analyzed here. Note, however, that these findings tell us virtually nothing about the concentration and dispersion of grant *money*. There are also other data limitations in this study, but not all would suggest opposite results. For example, although other ways of identifying field favor might indicate that it had greater impact, a more fully reliable interval level measure of publications and of their quality might produce stronger evidence of universalism.

Perhaps the most important finding is the initial one, that large percentages of faculty members have grants. Considering that there must be some people who do not want grantsmanship roles, those who get grants could well be close in number to those who want them. Attempts at favoritism and partiality may be undetected by general surveys precisely because most are favored. To the extent that grant getting is neither meritocratically competitive nor biased by field, it appears to be relatively democratic or random rather than elitist. Among insiders, such a system is not likely to provoke charges of inequity or injustice. This, indeed, may be one of the principal functions of a broad-based competition having many winners. It gives a deeper legitimacy to sponsored research than if the vast majority of faculty were wholly excluded or were excluded by demonstrably nonuniversalistic criteria. A scarcity of grants might alter both actual distributing practices and their acceptability.

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Commentary and Debate

THE PRACTICE AND EXPLANATION OF COUPS D'ETAT: MEASUREMENT OR ARTIFACT?

A recent attempt in this *Journal* to explain "coup activity" during the 1960s in sub-Saharan African nations (Alan Wells, "The Coup d'Etat in Theory and Practice: Independent Black Africa in the 1960s," *AJS* 79 [January 1974]: 871-87) is methodologically weak and needs qualification. The validity of the suggested explanation based on the statistical analysis of aggregate data for these nations has to be seriously questioned, because (1) no attention is paid to the reliability of these data and (2) the data and the statistical analysis have no clear relationship to a coherent theoretical argument.

Since information comparable to that used by Wells had been published for all but one of the countries he analyzes (Morrison et al. 1972), reliability estimates for his data could and should have been calculated. Although Wells never clearly operationalizes his basic terms ("coups," "aborted coups," and "plots"), it is reasonable to assume that such definitions would not diverge substantially from those in Morrison et al. (1972, chap. 12). In table 1, the frequency counts of these events as coded by Wells and by Morrison and Stevenson (1971) and Morrison et al. (1972) are presented, along with a variety of indices which combine these counts into a single scale. Wells's index of coup activity weights the frequency of each type of event and then standardizes the sum by the number of years since 1960 or since independence, if that occurred after 1960. We calculated indices for both data sets, and in addition we present a previously published "elite-instability" index, using weights different from those used by Wells but using the same variables. This latter index is calculated both with and without standardization for the period since independence.

The reliability coefficients for the interindex comparisons are shown in table 2. These coefficients demonstrate moderately high intercoder agreement as well as an apparent irrelevance of differences between Wells's weighting and standardization procedures and those described in Morrison and Stevenson (1971). Nevertheless, the differences in the raw event codings need some explanation. First, it is apparent from table 1 that Wells consistently reports comparatively fewer plots and more aborted coups. The reasons for this cannot be inferred from the data as presented, however. Second, there are some significant differences in the codings of coups d'état: by our count there were two coups in Nigeria, both in 1966 (Wells had four); one coup (not two) in the Sudan, since the events of 1964 seem to reflect a mass revolt rather than a coup d'état; a coup in Uganda (Wells

TABLE 1
ALTERNATIVE COUP-ACTIVITY INDICES FOR 30 AFRICAN COUNTRIES IN THE PERIOD 1960-69

COUNTRY	Coups		ABORTED Coups		Plots		YEARS		COUP-ACTIVITY INDEX*			
	Wells	M/S	Wells	M/S	Wells	M/S	INDEPENDENT	DEPENDENT	CA1	CA2	CA3	CA4
Burundi	2	2	1	1	2	3	8		3.25	3.38	16	2.12
Cameroon	0	0	0	0	0	1	10		0.10	0.20	1	0.20
Central African Republic	1	1	0	0	2	3	10		1.30	1.40	8	0.90
Chad	0	0	0	0	0	5	10		0.10	0.60	5	0.60
Congo (Brazzaville)	3	2	3	2	2	7	10		4.20	3.40	23	2.40
Dahomey	5	4	0	0	0	6	10		5.10	4.70	26	2.70
Ethiopia	0	0	2	1	1	1	10		0.80	0.50	4	0.50
Gabon	0	0	1	1	0	0	10		0.40	0.40	3	0.40
Gambia	0	0	0	0	0	0	5		0.20	0.20	0	0.20
Ghana	1	1	1	1	1	4	10		1.50	1.80	12	1.30
Guinea	0	0	0	0	2	4	10		0.30	0.50	4	0.50
Ivory Coast	0	0	0	0	1	2	10		0.20	0.30	2	0.30
Kenya	0	0	1	0	0	1	7		0.57	0.28	1	0.28
Liberia	0	0	0	0	2	3	10		0.30	0.40	3	0.40
Malawi	0	0	0	0	0	0	6		0.17	0.17	0	0.17
Mali	1	1	0	0	0	2	10		1.10	1.30	7	0.80
Mauritania	0	0	1	0	0	0	10		0.40	0.10	0	0.10
Niger	0	0	0	0	1	1	10		0.20	0.20	1	0.20
Nigeria	4	2	0	0	2	2	10		4.30	2.30	12	1.30
Rwanda	0	0	0	0	0	0	8		0.13	0.17	0	0.17
Senegal	0	0	1	1	0	3	10		0.40	0.70	6	0.70
Sierra Leone	2	2	0	0	1	1	9		2.44	2.33	11	1.33
Somalia	1	1	1	1	0	0	10		1.40	1.40	8	0.90
Sudan	2	1	0	3	2	3	10		2.30	2.30	17	1.80

TABLE 1 (Continued)

COUNTRY	COUPS		ABORTED COUPS		PLOTS		YEARS INDEPENDENT	COUP-ACTIVITY INDEX*			
	Wells	M/S	Wells	M/S	Wells	M/S		CA1	CA2	CA3	CA4
Tanzania	0	0	1	0	2	0	9	0.67	0.11	0	0.11
Togo	2	2	2	1	0	7	10	2.70	3.10	20	2.10
Uganda	0	1	1	0	3	3	8	0.88	1.75	8	1.12
Upper Volta	1	1	0	0	0	2	10	1.10	1.30	7	0.80
Zaire	2	2	4	1	5	7	10	3.80	3.10	20	2.10
Zambia	0	0	0	0	2	1	6	0.50	0.17	0	0.17

SOURCE.—Wells's data are from sources as reported in Wells (1974). Our data were previously reported in Morrison and Stevenson (1971) and Morrison et al. (1972). Wells's table did not make it clear, but we assume that he coded for the period January 1, 1960–December 31, 1969.

* CA1 is Wells's coup-activity index as reported in Wells (1974); CA2 is Wells's index calculated with our recorded data; CA3 is the elite-instability index, as in Morrison et al. (1972); CA4 is CA3 standardised by years "independent."

TABLE 2

RELIABILITY INDICES* BETWEEN ALTERNATE INDICATORS OF 30 AFRICAN NATIONS'
COUP ACTIVITY IN THE PERIOD 1960-69

	COUP-ACTIVITY INDEX†			
	CA1	CA2	CA3	CA4
CA1
CA2	0.86
CA3	0.84	0.96
CA4	0.81	0.98	0.98	...

* Squared Pearson product-moment correlations are used as reliability measures in this table.

† See notes to table 1 for definitions.

had none) involving the unconstitutional seizure of the powers of the President of Uganda by Milton Obote in 1966; four (not five) coups in Dahomey in this period; and two (not three) coups in Zaire. These differences may reflect differences in event definitions, but we suspect that it is more likely that they reflect the difference between the use of extensive regional and country source materials as opposed to Wells's use of a single international source (*New York Times*).

While the reliability of Wells's dependent variable is broadly satisfactory (it would be surprising if it were not, given the public nature of most such events), the reliability of the independent variables is much less so. Our own data for the variables that Wells analyzes are taken from tables in Morrison et al. (1972). The calculation of reliability coefficients for comparisons of these with Wells's data was made difficult because of incomplete citation and missing data in Wells's cited sources. For instance, the table cited as the source for Wells's data on urbanization does not present data for 14 of the 30 countries being compared. Whether Wells simply proceeded with so much missing data is not stated, but it should be noted that seven of these 30 countries have *no* city with a population greater than 100,000, and several others have only one city which slightly exceeds a population of that size. Wells cites a source for data on literacy which gives no information on Ethiopia and Liberia. The source for the size of the military has missing data for seven countries. The source for population growth has no data on four countries, and the source cited for data on radio distribution has no data for four countries and data for a later year in the case of three other countries. We could not trace Wells's data on U.S. economic aid, because no full citation was given, and the World Bank IDA annual reports do not seem to report data in the specific manner reported by Wells. The data on military budget as a percentage of total government budget are presented by Wells for 1968, when the source he cites gives figures for 1969; and, although he complains of being unable to obtain

data for the military for earlier periods, the first page of the source he does use cites an earlier work in the same series which has such data.

With these obvious difficulties of missing data in mind, we give the reliability coefficients for the comparisons between Wells's data and our own in table 3. Where data are missing in one or both elements of an observation, those data are omitted, as is usual in handling missing data. Nevertheless, more than one-third of these measures are of questionable reliability. Wells proceeds, without questioning the reliability of his mea-

TABLE 3
RELIABILITY INDICES BETWEEN ALTERNATE CODINGS OF SOCIOECONOMIC AND
MILITARY-STRUCTURE MEASURES* FOR 30 AFRICAN NATIONS

Indicator	Reliability Index†
Socioeconomic variable:‡	
Population	1.00
Population growth rate	0.76
Urbanization (100,000)	0.75
Centrality	0.74
Literacy	0.25
Mass media	0.74
Economic growth rate	0.08
Average	0.62
Military variables:	
Military-participation ratio	0.74
Military size	0.79
Police size	0.86
Defense budget	0.53
Military/government budget	0.53
Military/GNP	0.31
Average	0.63

* As coded by Wells (1974) and by Morrison et al. (1972).

† Squared Pearson product-moment correlation coefficients are used as reliability indices.

‡ No reliability is given for the economic-level measure, since Wells uses the same data as that given in Morrison et al. (1972).

asures, to regression analysis as a means of discerning an "explanation" of coup activity.

Inspection of the zero-order correlations in Wells's data revealed (1) a generally low level of correlation between indicators of socioeconomic structure and coup activity; (2) that literacy and mass-media exposure had the only distinctly nonzero (and opposite) relations to levels of coup activity; and (3) that measures of military structure were strongly and uniformly positively correlated with coup activity. Replications of the simple-correlation results using our data demonstrate (see tables 4 and 5) important divergences from the results obtained by Wells. First, the replicated results indicate a generally more pronounced relationship between measures of socioeconomic structure and coup activity—the average correlations for these independent variables and the Wells coup-activity index

TABLE 4
PRODUCT-MOMENT CORRELATIONS BETWEEN SOCIOECONOMIC MEASURES AND THREE RECODED INDICES OF COUP ACTIVITY

INDICATOR*	1	2	3	4	5	6	7	8	COUP-ACTIVITY INDEX†		
									CA2	CA3	CA4
Population (1969)	07	-04	-25	44	-27	-26	-15	37	16	14
Population growth rate (1969)	-13	-10	17	-42	-39	-12	-02	00	-02
Urbanization (population in cities greater than 20,000)	33	36	36	55	-19	-46	-44	-44
Centrality	-35	12	-13	12	23	16	20
Literacy rate	02	24	-02	28	15	16
Mass media	53	-11	-19	-17	-14
Economic level (GNP per capita)	04	-12	-10	-05
Economic (GNP) growth rate	-18	-26	-28

SOURCE.—Data is from Morrison et al. (1972).
NOTE.—Decimal points omitted in the correlations in the table.
* See Wells (1974) for definitions of indicators.
† See note to table 1 for definitions.

TABLE 5

PRODUCT-MOMENT CORRELATIONS BETWEEN MILITARY VARIABLES AND THREE INDICES OF COUP ACTIVITY

INDICATOR*	1	2	3	4	5	6	7	COUP-ACTIVITY INDEX†		
								CA1	CA2	CA3
Military-participation ratio	25	11	22	22	54	51	12	13	18
Military size	89	91	24	80	28	40	23	24
Police size	76	19	68	16	21	06	07
Defense budget	24	79	36	39	28	31
Military/government expenditures	09	64	10	19	21
Military expenditures/GNP	29	26	15	19
Total U.S. aid through 1968	23	25	32

SOURCE.—Data from Morrison et al. (1972).

NOTE.—Decimal points omitted from correlation coefficients in the table.

* See Wells (1974) for definitions of the indicators.

† See notes to table 1 for definitions.

is 0.075 in his data and 0.23 in ours. Second, the relationships for urbanization (measured more appropriately, as indicated above, in terms of cities with populations greater than 20,000), centrality, and population are all considerably greater in our data than the corresponding relationships for literacy and mass media, the two which Wells judges to be significant. Third, the relationships between military variables and coup activity confirm the uniform direction of association reported by Wells and those previously reported in Morrison and Stevenson (1972*b*). Our correlations for these latter variables are, however, generally lower than those reported by Wells and suggest that his emphasis on the greater explanatory value of the military as opposed to other socioeconomic structures is open to question.

When we move next, as Wells does, to multiple-regression analysis, additional qualifications of his results emerge. We present in table 6 standardized regression coefficients for multiple-regression equations incorporating military and socioeconomic variables. Wells reports unstandardized coefficients, making it impossible to compare the relative importance of different independent variables to the explanation of variance in coup activity; but our results indicate again the relatively greater importance of socioeconomic variables other than literacy and mass media to the explanation of coup activity. Second, Wells fails to take note of the important possibilities of changes in the signs of these multiple-regression coefficients from the corresponding zero-order coefficients. There are at least three such changes in our results, but their significance for theory construction and explanation must be ambiguous for Wells, who has offered no coherent theory to guide the interpretation of these results. Third, com-

TABLE 6
STANDARDIZED PARTIAL-REGRESSION COEFFICIENTS OF SOCIOECONOMIC AND MILITARY MEASURES WITH THREE INDICATORS
OF COUP ACTIVITY (ELITE INSTABILITY)

INDICATORS	SOCIOECONOMIC VARIABLES WITH				MILITARY VARIABLES WITH				ALL VARIABLES WITH			
	CA2	CA3	CA4		CA2	CA3	CA4		CA2	CA3	CA4	
Population	-.013	-.012	-.013			-.005	-.071	...	
Population growth rate	-.024	-.025	-.025			-.029	-.027	...	
Urbanization	0.26	0.11	0.13			0.22	
Centrality	-.033	-.039	-.038			-.046	-.043	...	
Literacy	0.12	0.13	0.13			0.06	0.23	...	
Mass media	-.017	-.019	-.021			-.025	-.021	...	
Economic level	-.029	-.026	-.021			-.048	-.041	...	
Rate of economic growth	-.010	-.025	-.027		-.020	...	
Military-participation ratio	0.14	...	
Size of military		1.39	0.79	0.59		1.09	1.65	1.64	
Size of police		-.075	-.067	-.057		-.094	-.093	...	
Defense budget		-.012	0.20	0.31		
Defense/government budget		-.016	0.03	0.02		-.027	-.009	...	
Defense budget/GNP		-.027	-.023	-.023		-.009	-.037	...	
U.S. aid		0.21	0.11	0.15		-.007	-.015	...	
Multiple correlations62	.56	.56		.57	.45	.47		.74	.69	.68	

NOTE.—No coefficients are significant at the .05 level. See notes to table 1 for definitions of CA2, CA3, and CA4.

parison of our solution for the multiple-regression equation using socioeconomic variables with that using military variables again qualifies Wells's suggestion of the greater explanatory value of these latter measures. Fourth, comparison of our solution of the equation using all independent variables with that produced by Wells shows that seven of the 15 coefficients either have different signs or go from omission to inclusion in the two analyses.

An explanation of coup activity in these nations requires some resolution of questions such as those raised by this replication of Wells's analysis. This resolution must entail attention to both theory and measurement. Wells's concluding theoretical comments speculate that coup activity is explained by an "interaction" between military structure and socioeconomic factors, both of which in turn are affected in some undefined way by centrality and external influences. These speculations are not tested by his data, nor is it clear how they constitute an interpretation of his data analysis. Furthermore, they are developed without any systematic reference to the very considerable body of previously published research on political instability, socioeconomic development, and external dependency.¹

Theory must provide the arbitration between explanation and speculation, but we cannot expect to develop confidence in theory from the manipulation (even when grounded in theory) of data whose reliability and dimensionality are questionable. The use in multiple-regression models of a large number of measures which are highly intercorrelated (military variables) as well as unreliable, as in Wells's analysis, creates problems of multicollinearity, leads to large standard errors for the regression coefficients, and provides no clear accounting of the unique contribution the measures make to the explanation of variance in the dependent variable. The resolution of what is and is not artifact in such results and the clarification of the kinds of differences that distinguish our results from those of Wells can only be expected after serious attention has been given to the reliability and dimensionality of the measures involved. Factor analysis, for example, may assist the development of error-reduced and statistically independent measures. Our own investigation of a principal-components analysis of these data shows that three factors account for over 66% of the variation in the socioeconomic variables and that three factors account for over 90% of the variation in the military variables, when each set of measures is separately analyzed in each data set. These factor structures are not identical in each data set, but they are sufficiently similar to suggest that greater agreement between ourselves and Wells would result from the analysis of a reduced set of six rather than the original 15 independent variables.

¹ Representative works in these areas available to Wells are Gurr (1970), Felerabend and Feierabend (1966), Huntington (1968), Melson and Wolpe (1970), Morrison and Stevenson (1971, 1972*a*, 1972*b*), O'Connell (1967), Levine (1970), Arrighi (1970), Nelkin (1967), and Welch (1970).

Wells's neglect of the theoretical literature, empirical studies, and the quantitative-data archives developed primarily by political scientists to deal with the questions he investigates makes the publication of his article anachronistic with respect to the present state of interdisciplinary research on this topic. His inattention to methodological issues that have been a major focus of concern in recent sociological research makes its publication anachronistic in this *Journal*, which is so much identified with pioneering methodological research. The important search for answers to the problems of development in contemporary African nations is ill served by such work.

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COMMENT ON ALAN WELLS'S "THE COUP D'ETAT IN THEORY
AND PRACTICE: INDEPENDENT BLACK AFRICA
IN THE 1960s"

In a recent article in this *Journal* (*AJS* 79 [January 1974]: 871-87), Alan Wells provided a multivariate analysis of several socioeconomic and military variables in an attempt to explain the incidence of military coups in 31 black African countries during the 1960s. On the basis of his analysis, Wells concluded that the data provide some empirical support for the argument that socioeconomic and military variables affect the likelihood of coups separately and exert a fairly strong influence when all such variables are combined. It appears, however, that the greater part of the R^2 values results from a statistical artifact.

The "coefficient of determination" (R^2) that results from regression analysis on any sample has been shown to be a biased estimate of the "true" coefficient of determination for the population (Barten 1962). Where the number of independent variables is small relative to the number of observations (as in most sociological research), this bias is negligible; but when the number of variables in the regression model is high relative to the number of observations, the resulting R^2 may be greatly biased. This bias is always positive (Montgomery and Morrison 1973, p. 1010; Christ 1966, pp. 509-10).

According to Montgomery and Morrison (1973, p. 1011), R^2 should be adjusted according to the following formula when the number of variables in the model (k) is high, relative to the sample size¹ (N): $\bar{R}^2 = 1 - [(1 - R^2)(N - 1)/(N - k)]$. Wells's regression analysis is greatly modified when this adjustment is made (see table 1).

After the R^2 values are adjusted, neither the set of socioeconomic variables nor the military variables appear to explain any of the variance in the incidence of coups. Taken together (analysis 3), they explain merely 12% (as opposed to 56%, according to the unadjusted R^2 !) of the variance in the military coups d'état.

The sharp reduction in the R^2 values has two important implications. First, given Wells's data, military coups appear to be far less predictable than the Wells article indicates. Second, in the rather rare situations in which data are based on low N and the number of variables in a regression analysis is high, R^2 values should be adjusted to avoid the possibility of severe upward bias.

The adjusted R^2 of .12 for Wells's analysis 3 suggests that the incidence

¹ Statistics texts written for sociologists usually mention this in discussions of multiple-regression analysis. The issue is not emphasized, however, perhaps because sociological data are rarely based on small sample sizes.

TABLE 1
A COMPARISON OF UNADJUSTED AND ADJUSTED R^2 's FOR THREE ANALYSES

	Analysis 1*	Analysis 2†	Analysis 3‡
Multiple-correlation R41	.48	.75
Unadjusted R^217	.23	.56
Adjusted R^2	0	.04	.12
N	31	31	31
k	8	7	16

* "Socioeconomic variables," consisting of population, population growth, urbanization, centralization, literacy, mass media, and economic level.

† "Military variables," consisting of military-participation ratio, size of military, size of police, percentage of budget devoted to defense, percentage of GNP allocated to defense, and amount of U.S. economic aid.

‡ Combination of the variables above, with economic growth and size of defense budget included.

of coups may be facilitated under certain circumstances, as Wells argued. That Wells's data provide even this relatively weak coefficient is encouraging, given the likelihood that the production of coups involves events which are contingent on other events, and all possible "causes" of coups are subject to a necessary social-psychological condition—a conspiracy. Replication is clearly in order. Only further examination of coup activity in other areas and times will determine the extent to which coups are predictable and which models best predict them. Wells's speculation that coups are dependent on a complex sequence of events and subject to several contingencies is well taken. Far from wanting to discredit Wells, we believe that, but for his research, we would not have been able to see the inadequacy of the present explanations of coups d'état.

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REPLY TO MORRISON AND STEVENSON AND TO PHILLIPS AND BA-YUNUS

I wish to thank both sets of critics for their useful comments. Despite their rather pointed attempt at demolition, Morrison and Stevenson's data ac-

tually strengthen my case. Since their paper is considerably more involved and so severely questions my competence, I shall devote most of my response to it.

First, how does one account for the tone of their comment? Perhaps it is because I overlooked their own superior data book and their other recent works; but they should be aware of writing and publishing time lags. I make no preemptive claim about my data—they were just the best I could find at the time.

I will readily concede that their data are superior, even for the dependent variable. However, a mere comparison with my indices does not necessarily demonstrate the extra reliability of theirs, even though they drew on more sources. Some coding differences are a matter of judgment, not fact, and this indicates the crudeness of such data. For example, Nigeria was highly volatile during the 1960s. Morrison and Stevenson scored it two coups in 1966. I counted four: the first military coup of Ironsi (actually a coup and counter coup by some accounts), Gowon's seizure of power in the north and west regions (extended by protracted warfare to the rest of Nigeria), Ojukwu's control in the east and the creation of independent Biafra, and the short-lived midwest region coup. While debatable, my scoring does indicate the extraordinary instability of Nigeria. I did not score a civilian ouster of a traditional regional chieftain (in Uganda) as a *military* coup. Morrison and Stevenson did, for reasons unclear to me.

There is no doubt that their measures of independent variables are better than mine. But, unless one overinterprets such data, I do not think that their analysis demonstrates "important divergences" from my results. Although I erred in unnecessary verbal interpretations of tables, my primary claim was simply that socioeconomic and military variables *are* related to the incidence of coups. My theoretical model (and I view my paper as primarily a theoretical contribution) recognizes that such structural factors are mediated by people: military officers (or noncommissioned officers) plot coups, and their underlings carry them out. The validity of most indicators is open to doubt, so whether the "real" multiple correlation for military variables is higher or lower than that for a particular set of socioeconomic measures (in Morrison and Stevenson's analyses they are about the same) is probably a measurement artifact. My interpretation stems in part from my model. Until I see evidence to the contrary, I will go on the assumption that the plotters' military milieu has more immediate impact than, for example, national economic well-being (measured very crudely as GNP). Some of my speculations about coups are not tested, as these critics correctly point out, although centrality and external influences are adequately linked to coups in my theoretical model. Such a model, I believe, has to be partly speculative. Although much of it may be tested,

unless we gain access to the minds of real and would-be coup perpetrators and learn how they are affected by their milieus, we will not fully understand military intervention.

Morrison and Stevenson are correct in their criticism that I did not exhaustively interpret the empirical analysis. My major concern was not to explain a rather nebulous body of data but to explain military coups. It may be anachronistic, but I think there is a difference. I will let others judge whether Morrison and Stevenson's analysis is more adequately explained or set on better theoretical foundations than mine. What benefit, I wonder, have they gleaned from "systematic reference" to all of the works (including their own) that I willfully overlooked? The *Journal* and I, they speculate, have not only set back sociology but sabotaged developmental problem solving in Africa: quite an impact. Personally, I think the swords of conspirators are a shade mightier than my pen.

Phillips and Ba-Yunus appear to have a better understanding of the aims of my paper, and they have pointed to a valid statistical adjustment that I overlooked. Unlike Morrison and Stevenson's analysis (which exhibits the same statistical flaw), it does seriously modify my findings and suggest areas where future investigation is needed. I doubt that we need factor analysis of crude macrodata, but we do need more information (gleaned of necessity indirectly) on the complex sequence of events and pressures that induce some military men to plot and carry out coups d'état. The socioeconomic setting and crude military dimensions have some impact, but quantitatively less than my statistics indicated.

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COMMENT ON HUBER'S REVIEW OF *THE INEVITABILITY OF PATRIARCHY*

It is not possible, within the limitations imposed on a comment of this sort, to present a summary of my *The Inevitability of Patriarchy* (1973) sufficiently detailed to demonstrate fully the inadaquacy of Joan Huber's muddled and misrepresentative summarization (*AJS* 80 [September 1974]: 567-68). Fortunately, it is not necessary to present an extensive summary here as I have done so in the *American Anthropologist* (Goldberg 1975). I would, however, like to discuss the few points relevant to Huber's review that I did not cover in the latter article.

Patriarchy is an attempt to explain the fact that every society has manifested patriarchy (male attainment of hierarchical positions and high-



status roles) and male dominance (the social expectation that dominance in male-female relationships resides with the male). It argues that the most tenable explanation—in terms of parsimony, internal logic, and concordance with the anthropological and physiological evidence—is one postulating a male-female differentiation of motivational response to the environmental presence of (1) a hierarchical situation or (2) a member of the other sex. In the present context it is of little theoretical importance whether one conceptualizes this physiologically rooted, emotional-behavioral differentiation in terms of a greater male “need,” a greater male “drive,” a greater male readiness to learn dominance behavior, a lower male threshold for the release of dominance behavior, or a weaker male “ego.” It is the *differentiation*, and a population’s observation of its emotional and behavioral effects, that sets limits on socialization and institutionalization.

After attempting to demonstrate that the physiological evidence provided by the endocrinological study of human beings, and by hundreds of studies of the effects of cross-sex hormonalization of other mammals, fully justifies our postulating the differentiation that the anthropological evidence alone would force us to postulate, *Patriarchy* attempts to elucidate: (1) the manner in which the emotional-behavioral differentiation acts as the causal link between physiological differentiation, on the one hand, and differentiation in social values, socialization, and institutions, on the other; (2) the manner in which socialization conforms to a population’s observation of the reality of the differentiated male and female behavior that results from the male’s having a lower threshold for the release of dominance behavior; and (3) the manner in which such socialization exaggerates the differentiation of behavior by making qualitative, discrete, and absolute a differentiation that is, on the physiological-behavioral level, quantitative, continuous, and statistical (so that the physiologically generated reality—that males more often and more readily manifest dominance behavior—becomes the stereotype, “Men are aggressive and women are passive”). Since Huber hardly mentions the chapters discussing these issues, I will concentrate on her attempts to demonstrate the irrelevance of the physiological evidence.

Huber makes the frequently expressed, yet nonetheless fallacious, argument that it is incumbent upon me, if I am to give priority to the psychological factor, to demonstrate that this factor operates *within* the group comprised of males as well as between the groups of males and females. This is incorrect. It is not necessary for the theory presented in *Patriarchy* for physiological variation among males to affect the degree to which different males manifest dominance behavior. An analogy should make this clear. If one wishes to present an explanation of why the best boxers are males and why, in socialization, fighting ability is associated with males, it is sufficient to point out that there is a strength requirement for this ability

that is met by many men and by few women. One need not argue (and indeed it would not be true) that within the group of males strength is the crucial factor. Moreover, even if physiological differences among males *are* involved in different males' differing propensity for manifesting dominance behavior, such within-group differentiation cannot be observed (by the population) to be associated with identifiable categories of males. Thus, such differences cannot be manifested in the socialization system.

Huber is, I think, a bit disingenuous in her attempts to cast doubt on my use of the physiological literature. She refers to Money and Ehrhardt's statement that "aggressiveness" is not a trait associated with tomboyism and then fails to quote their next sentence: ". . . The correct variable is dominance assertion, and striving for position in the dominance hierarchy of childhood" (1972, p. 99). The very point of the footnote of mine is that "dominance assertion" and "striving for position in the dominance hierarchy" are precisely what "aggression" (as I use the term) *is*. "Aggressiveness" (i.e., fighting behavior) is irrelevant to the theory I present.

A typical example of Huber's penchant for quotation out of context is the following: "[Goldberg states that] even if the boy is more aggressive than the girl only because society allows him to be, his socialization still flows from society's acknowledging biological reality" (p. 568). Presented out of context, my point seems silly, if not altogether meaningless. In context, it is clear that I am making the point that—*even if* it were true that physiological differentiation were irrelevant to the little boy's being more aggressive—socialization (the preparation of children for adult social life) would conform to the reality of the adult male's far greater testosterone level. In fact, the little boy's greater aggression is not owing only to such socialization. This misconception, which derives from the fact that prepubertal boys and girls have roughly equal testosterone levels, is based on an utterly simplistic view of the physiology of dominance behavior (for which the fetal hormonal masculinization of the central nervous system—and the resulting increase in the sensitivity of the CNS to the dominance-related properties of testosterone—is as important as the levels of testosterone).

I cannot here straighten out the many similar misrepresentations presented by Huber. I might, however, make the general point that sociology is not well served by the inevitable vague reference to "the enormous literature on technological changes." There is not the slightest shred of evidence that any change in any social or economic factor reduces patriarchy. The percentages of males in the upper hierarchical positions in modern societies are as overwhelming as in the most primitive societies. The attempt to explain this in purely social terms is not an explanation, but a begging of the question: *why* does every society's socialization system associate dominance with the male? *Patriarchy* argues, in an extensive analysis, that

explanations that fail to postulate the central importance of the physiological-motivational factor (for example, an analysis emphasizing the woman's maternal role or the male's physical strength) can maintain their internal logic and concordance with the ethnographic data only by surrendering parsimony and plausibility and by ignoring the direct physiological evidence. I cannot offhand think of a single other nontrivial hypothesis in all of sociology for which one can invoke both anthropological universality and direct physiological evidence; is there not something a bit absurd about the penchant of sex-role sociology for dismissing on ideological grounds a hypothesis so supported, while treating as fact the implausible conclusions of a hundred dubious little studies?

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Money, John, and Anke A. Ehrhardt. 1972. *Man and Woman, Boy and Girl: The Differentiation and Dimorphism of Gender Identity*. Baltimore: Johns Hopkins Press.

HUBER'S REPLY TO GOLDBERG

Professor Goldberg's comment on my review illustrates his naïve reductionism so effectively that I hardly need to reply. Although he serves up an unusually rich collection of errors, the most basic difficulty with the book, as with his comment, is his failure to understand the requirements of scientific argument. He claims that differing testosterone levels in men and women produce a behavioral difference—greater male aggressiveness—that results inevitably in patriarchy. Since sociologists doubtless agree that male/female testosterone levels differ and that all known societies have been more or less patriarchal, the issue is whether aggression is the intervening variable that links hormones and social structure.

But Goldberg never defines or conceptualizes aggression precisely, hence its effect on stratification systems cannot be spelled out. Instead, hard as it may be for readers to believe, Goldberg claims that it makes no difference to his theory how aggression is defined—it is the stuff that flows from hormones and to which social structures conform. Readers who think I am making this up may consult the book's index, which indicates the definition of aggression on page 91. The following sentences appear in sequence:

"I use *aggression* only as a convenient hypothetical term, a nexus which flows from hormones and to which certain societal institutions conform. The reader is free to substitute *the X factor*, *male behavior*, or any other term that represents an element that flows from specifiable hormonal factors and that determines the limits of specifiable social institutions (patriarchy, male dominance, the male attainment of high-status roles and positions)." (Here and in the quote below, the italics are Goldberg's.)

In turn, Goldberg's squishy definition of aggression invites his misleading comparison of his work with Money and Ehrhardt's. A writer who claims that he spoke only of "aggression" and not "aggressiveness" is careless with ordinary rules of English usage. Indeed, on page 93 of his book Goldberg tells us that it makes no difference how aggression is defined: "What is crucial here is that men and women differ in their hormonal systems and that every society demonstrates patriarchy, male dominance, and male attainment. *The thesis put forth here is that the hormonal renders the social inevitable.*"

In principle—although it is unlikely—one of the many biological differences that statistically distinguish men and women could inevitably produce a categorical social difference. But Goldberg has not demonstrated a link between hormones and patriarchy; he has only asserted it. What he does demonstrate effectively is that simple monocausal models continue to attract the unwary.

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Review Symposium

Sociobiology: The New Synthesis. By Edward O. Wilson. Cambridge, Mass.: Harvard University Press, Belknap Press, 1975. Pp. ix+697. \$20.00.

DARWIN RIDES AGAIN

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Sociobiology is the study of the biological basis for all social behavior. It is the integration of neo-Darwinian theory with the observations of animal behavior research. Thus, while the relevant principles come from modern population biology and genetics, the raw data come largely from ethology. This book represents the first major attempt to synthesize this vast array of material.

Even before it was published, *Sociobiology* received rave notices in the popular press. The early reviews that followed in the academic journals were no less laudatory. Edward O. Wilson, almost everyone agreed, had put together probably one of the most impressive and formidable textbooks ever published in either the natural or social sciences. The 700 oversize pages are tightly written, well documented, and abundantly illustrated. The book most certainly is an excellent introduction to the broad field of evolutionary biology, although it may be too advanced for the average college freshman. Some parts assume at least a rudimentary knowledge of evolutionary theory and genetics, as well as elementary probability theory and calculus.

Not everyone will like what Wilson has to say. Sociologists in particular may feel put down, a point that will be taken up at the end of this review, while politically minded academics in general may feel that Wilson has committed the same sin as Jensen, Shockley, and Herrnstein in arguing that human behavior is conditioned genetically rather than shaped wholly by the environment. Indeed, *Sociobiology* already has been denounced by Science for the People and other radically oriented groups around Harvard as the latest attempt to reinvigorate social Darwinism, the law of the survival of the fittest.

These critics, however, have largely distorted the fine print and have generally misrepresented the main content of Wilson's book. It is true that Wilson suggests a possible genetic (evolutionary) basis for all kinds of social behavior appearing in nature, such as "infanticide" and "cannibalism" among bees and wasps, "harem formation" in mammalian societies, "soldier" termites, "castes" and "slavery" in ant colonies, "homosexuality," and so on. He even suggests that variations in behavior between different human cultures may have some genetic basis and says that "at the very least, we should try to measure" it (p. 550).

Sexism, militarism, racism? This is *not*, as Wilson's critics would have us believe, what this book is mainly about. On the contrary, to the extent that there is a central theme running through the text, it deals with altruism, that is, self-sacrificing behavior that helps insure the survival of others. From Wilson's point of view, and that of many other zoologists, the central theoretical problem of sociobiology is just how altruism arises by natural selection when it is detrimental to an individual's own survival.

But even here the subject matter is not basically about human beings. It is about altruism as found in the whole range of social species, from cold-blooded vertebrates to carnivores and other mammalian orders. Only the last of 27 chapters deals directly with man, and that chapter is relatively short. Before turning to it, let me review the main sections of the book.

Wilson begins with five chapters on social evolution, that is, the evolution of social behavior in animal societies. Numerous phenomena and concepts are introduced, many in ways not unfamiliar to sociologists, such as socialization, communication, coordination, hierarchy, age pyramids, and so forth. Other key concepts, like behavioral scale, phylogenetic inertia, heritability, density dependence, reproductive effort, and kin selection, have meanings rather specific to evolutionary biology and population genetics.

One of the initial problems the sociobiologist faces is to define the boundaries of the particular group of conspecific organisms he is studying and to codify the kinds and degrees of sociality and group structure present. Having laid this groundwork, called the "natural history" or "phenomenological" stage of sociobiology, Wilson begins to develop his fundamental theory. The major predetermined variables of social organization are the demographic parameters of the population and the rates of gene flow. These parameters, in turn, are determined by ecological (environmental) pressures and the phylogenetic inertia of the species, that is, the deeper properties of the population (such as its genetic variability) which determine the extent to which its evolution can be deflected in one direction or another. Thus, social evolution, according to Wilson, "is the outcome of the genetic response of populations to ecological pressure within the constraints imposed by phylogenetic inertia" (p. 32). Phylogenetic inertia and ecological pressures, then, are the "prime movers" of social evolution.

Using examples from field studies of particular social species, Wilson reviews the ecological factors thus far identified that tend to induce social evolution. This evolution includes the diverse ways in which social behavior assists a species in converting other organisms into energy, as well as in helping to prevent it from being turned into energy by its predators (eat or be eaten). Most explanations for the ecological correlations found in this and other sections of the book unfortunately tend to be ad hoc, and the data often lend themselves to more than one interpretation. Wilson nevertheless is uncompromising in the rigor with which he applies the general theory of natural selection to the findings.

He next gives a cogent synopsis of the basic principles of natural selection theory itself, as it is now embodied in population biology. Discussed are

mutation pressure, genetic drift, gene flow, and selection (overwhelmingly the most important force in evolution); the manner in which genetic variation is maintained in a population; measures of inbreeding, kinship, and assortative mating; and the various mechanisms by which the density of a population is controlled.

Still early in the book, one full chapter is devoted to the origins of altruism. At the expense of going hungry themselves, honeybees regurgitate food on their nestmates. Termites will explode themselves to protect the colony from predators. Some birds will postpone mating in order to help care for the offspring of a close relative. Numerous illustrations of this kind can be documented. Given the iron laws of natural selection, however, how can altruism evolve if it essentially means surrendering one's personal genetic fitness for the genetic fitness of others? (Genetic fitness is defined here strictly in terms of the number of surviving offspring one leaves.) Animals exhibiting self-sacrificing behavior normally would be expected to die out while more selfish individuals prospered.

The answer lies in recognizing that not the individual but the entire family or group may serve as the unit of selection. Wilson calls this "kin selection," which means that the average survival and fertility of the group as a whole can under some circumstances counteract the effects of "individual selection." Thus, according to current theory, altruism is most likely to have evolved in relatively small groups in which the kinship unit benefited from the self-sacrificing behavior of some of its members *whose genes the family shared*.

Except for the fact that the data concern mostly insects, birds, fish, and mammals, the second and longest part of the book reads almost like a text in sociology or anthropology. Wilson reviews in 13 chapters the social mechanisms that mediate the effects of ecological pressures (to which a species in the long run must adapt or become extinct) on the demographic parameters that measure successful adjustment. He begins by examining the determinants of group size, basing the discussion on Poisson distributions and optimization theory. This is followed by a discussion of some of the mechanisms different species use in adapting to environmental changes of varying duration, including the functions of hormones, learning, socialization, play, tradition and culture, and toolmaking.

Three chapters in this section are devoted to communication. Sociologists who are unclear in their own minds about the differences and similarities between human and animal communication, including current myths about dolphins which Wilson later debunks, will find here the clearest and most interesting review of the topic probably available in the literature. Did you know that one of the most complex courtship systems is that used by grasshoppers, or that the most elaborate single display of any kind known in the world may be the song of the humpback whale? The remainder of this part of the book contains separate chapters on aggression, social spacing and territoriality, dominance systems, roles and castes, sex and society, parental care, and social symbioses, that is, the close, protracted relationships that sometimes develop between organisms belonging to different species. Each

of these forms of social behavior is discussed within the theoretical framework outlined earlier.

The third and final part of the book contains separate chapters on social evolution in the colonial microorganisms and invertebrates, the insects, the cold-blooded vertebrates, and the birds, plus five chapters on the mammals, including man. The theme running through much of this part of the book is the interesting paradox that, as evolution has proceeded from more primitive and older forms of life to more advanced and recent ones, some of the key forms of social behavior, such as cohesiveness, altruism, and cooperativeness, have generally declined.

The jellyfish-like siphonophores, for example, are so fully interdependent in both function and structure that a colony of zooids is hardly distinguishable from a multicellular organism. The genetic identity and plasticity of members of the colony allow an extreme degree of altruism in which the individual is totally subordinated to the advantages of the colony. From a genetic point of view, the colonial invertebrates come close to producing perfect societies.

Insect colonies are much less perfect. Despite the typical self-sacrificing behavior and highly specialized division of labor between castes that maintain the colony, social insects are physically separate entities whose independence sometimes takes the form of overt conflict between members, such as the occasional competition between workers and queen to produce offspring.

Among the vertebrate societies, including nonhuman mammals, aggressiveness and conflict are carried much further. In fact, by human standards, interindividual behavior in a school of fish or in a baboon troop is really quite brutal. Individual members of a vertebrate society are more on their own than in insect societies, and group membership is seldom well coordinated. Altruistic behavior, in the genetic sense, is relatively uncommon. These societies usually consist of selfish subgroups of close kin which compete among themselves at the expense of the society as a whole. Status in the society is based upon competition and individual adaptability.

Man, in Wilson's view, has reversed the downward trend in the evolution of social behavior. While increasing individual fitness and freedom from societal constraints, human societies at the same time exhibit a degree of cooperativeness about as high as that found in insect societies. What Wilson believes accounts for man's unique position in the social species are his greater intelligence and unique syntactical language which have permitted him to consult the past and plan the future and, in turn, to develop a complex level of social organization based upon acts of reciprocal altruism (reciprocity).

In the remainder of this review, I would like to comment on Wilson's treatment of man and sociology. Wilson is not ignorant in this area, but selective and biased. On the evolution of social organization and behavior in human societies, it is apparent that he has read Homans, Parsons, Lévi-Strauss, Goffman, Weber, and Durkheim. It also is apparent that he understands structural functionalism. The theory of natural selection is itself

directly analogous to functionalist theory as employed by sociologists, and Wilson applies it well in seeking the functional similarities among societies within and across phylogenetically diverse species in order to reveal evolutionary rules of universal application.

Like some of his brethren in sociology, Wilson has been attacked by his critics on these very grounds. The problem is inherent in any claim that some forms of socially undesirable behavior (in terms of our own standards), such as aggression, competition, and dominance, have adaptive value, that is, are functional. Given the strong tendency for some readers to assume that to explain something in terms of its adaptivity makes it "good," functionalists are forever condemned. If certain forms of social behavior are proven to be adaptive and "natural," it presumably is easier to justify the present social order. This is true irrespective of whether the behavior pattern is genetically or environmentally determined. But this is one of the perils of both sociobiology and sociology, since the foremost goal of science has always been (and I suspect always will be) to explain what is and not necessarily to say what ought to be.

On the other hand, I have the impression that Wilson has not read much of the empirical work in sociology. He accuses us, that is, sociologists specifically, of largely ignoring process and even first-order correlations and concentrating instead on phenomenology, concepts, and unaided intuition. On the contrary, I believe sociobiologists, including Wilson, will eventually need to learn much more than they apparently have about causal modeling, multivariate analysis, sampling, measurement theory, tests of significance, and the like. The advocacy method, which Wilson has no taste for since it invariably muddles through to an answer, no doubt is still more respectable in sociobiology than in sociology.

What about Wilson's claim that sociology is one of "the last branches of biology waiting to be included in the Modern Synthesis," that is, neo-Darwinist evolutionary theory? Can, or should, the social sciences be biologized? Admittedly, many functional similarities can be drawn between invertebrate, vertebrate, and human societies regarding contemporary social behavior. Moreover, some basic generalizations about common properties in the evolution of social organization across many different species, including man, may hold true. Sociobiologists may help shed considerable light on the biological basis of human behavior and even, as Wilson contends, on the biological basis of moral values and ethics.

We probably need not fear, however, that sociology will be reduced to another field of biology in the near future any more than we need worry about psychological reductionism. Psychology has not succeeded in, nor does it appear predisposed toward, reducing our understanding of either social structures or its own subject matter to descriptions of neurons and genes. Considering the almost incomprehensible causal distance between gene action and phenotypic behavior in man, it is unlikely that biology can succeed where psychology has not.

This does not mean, on the other hand, that Wilson's views, while overstated, should go unheeded. "Mankind has never stopped evolving" (p.

575). He is correct. "To maintain the species indefinitely we are compelled to drive toward total knowledge, right down to the levels of neuron and gene" (*ibid.*). Again, he is probably correct. The time indeed is approaching when the intellectual tradition of the social sciences might be enriched substantially by a cautious, limited convergence with the new sociobiology.

ON WILSON'S *SOCIOBIOLOGY*

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Edward Wilson has written an excellent survey of empirical studies of animal social behavior, probably the most comprehensive work of its kind. He has, in addition, brought together recent theories from ecology and population genetics, merging them with the empirical studies to form an impressive work—massive, attractive, and highly publicized. My intent here is to describe the book briefly and then to evaluate its usefulness for the sociology of human beings.

The book is divided into three parts. The first, "Social Evolution" (128 pp.), sets out Wilson's major themes, particularly (1) that certain unifying principles, such as altruism, underlie all social organization from termites to man; and (2) that social behavior is the product of natural selection and therefore has evolved just as the shape of our bodies has evolved. Part 2, "Social Mechanism" (248 pp.), has chapters on the size of social groups, genetic and environmental determinants of behavior, communication, aggression, territory, dominance, sex and role behavior, parental care, and social symbioses. Each topic is illustrated across a wide range of species from insects through primates. In the discussions of primates, the animals I know best, I found very few errors (grooming direction is *not* a good indicator of dominance in macaques; squirrel monkeys *do* have dominance hierarchies). Part 3, "The Social Species" (199 pp.), provides overviews of social behavior among large classes of animals: invertebrates, social insects, cold-blooded vertebrates, birds, ungulates, carnivores, nonhuman primates, and finally, in the last chapter (29 pp.), man. Primary behavioral features of each animal class are summarized in convenient charts, and this section contains excellent illustrations by Sarah Landry.

The final chapter, "Man: From Sociobiology to Sociology," is disappointing. Portions are trite ("Roles in human societies are fundamentally different from castes of social insects" [p. 554]), value-loaded ("Human beings are absurdly easy to indoctrinate—they *seek* it" [p. 562]), or wrong (Wilson claims on p. 522 that human social organization is generally matrilineal). Wilson is uncritical in his use of data, accepting the most doubtful of Colin Turnbull's descriptions of Ik society (p. 549) and Alfred Kinsey's biased sampling of American society (p. 555). His major socio-

logical source appears to be Gerhard and Jean Lenski's introductory textbook (pp. 560-61, 569, 573). Yet Wilson makes gross generalizations about sociology: "The transition from purely phenomenological to fundamental theory in sociology must await a full, neuronal explanation of the human brain" (p. 575). If Wilson makes a contribution to sociology, it appears earlier in the book.

Wilson's aim is a general evolutionary theory of social behavior, applicable to all social species. If he had accomplished this, then we sociologists might ignore his last chapter, extracting the theory from the rest of the book and making our own applications to human society. But I cannot find much usable theory. I know this will prove to be a controversial opinion, so I will make my objections explicit, starting at a point where we all agree.

Wilson insists that good theories must be capable of being disproved: "a theory that cannot be mortally threatened has little value in science. Most of the art of science consists of formulating falsifiable propositions in just this spirit" (p. 28). Since we are in full agreement here, it will make a good point of departure. I claim that the bulk of Wilson's theorizing is not falsifiable and therefore is of little value. There are several reasons why a theory may be untestable. It may be based on a tautology and therefore be true by definition. Or its concepts may be so vague that we find it impossible to unambiguously specify empirical indicators to represent them. Or, even when theoretical concepts are well defined, the data required for testing may be intrinsically inaccessible, as is the case, for example, when the theory concerns some past event which was not observed and which has left insufficient traces. I will show examples of each of these.

Genetic fitness—the ability to produce surviving offspring—is to evolutionary theory what *utility* is to economic theory: it is the quantity to be maximized. For example, in discussing the evolution of sexual behavior, Wilson writes, "The male will profit more if he can inseminate additional females. . . . Conversely, the female will profit if she can retain the full-time aid of the male, regardless of the genetic cost imposed on him by denying him extra mates. The offspring may increase their personal genetic fitness by continuing to demand the services of the parents when raising a second brood would be more profitable for the parents. The adults will oppose these demands by enforcing the weaning process, using aggression if necessary" (p. 314). The utility theory of economics has the danger of being circular: We choose the alternative which maximizes our utility, and whichever alternative we choose has maximum utility. In one sense utility theory explains everything, while in another (predictive) sense it explains nothing. The theory of evolution by natural selection, which may as well be called the theory of maximizing genetic fitness, has the same problem: "The fitter genotypes are those that leave more descendants, which, because of heredity, resemble the ancestors; and the genotypes that leave more descendants have greater Darwinian fitness" (p. 67).

Economists convert their tautology into testable theory by operational-

izing utility in some way that is independent of the maximizing decision of the actor. Utility may be operationalized as profit in dollars, and then we can test the theory by letting a subject choose between two alternatives, one bringing him more dollars than the other. If we are to make evolutionary theory testable, then genetic fitness must also be defined in some way that is independent of the behavior it is supposed to explain. Wilson has not done this. To the contrary, he almost invariably argues that the particular behavior which has evolved is the one which maximizes genetic fitness (because if it did not it would not have evolved).

We may take an example of this from Wilson's discussion of sexual behavior. He observes that men usually become aggressive when their mates commit adultery, and this is explained as a maximization of men's genetic fitness. This is pure tautology; no falsification is possible, since we have no way to measure maximum genetic fitness except in terms of the behavior we observe. If the behavior occurs, it must maximize genetic fitness; otherwise it would not occur. For Wilson, all that remains to complete this explanation is to specify in some detail a plausible selection mechanism whereby the maximization of fitness occurs. Here is Wilson's mechanism: A man who allows his mate to be fertilized by other men will waste his resources raising children who are not his own, and there will be few, if any, of his own descendants in the next generation. On the other hand, a man who acts to prevent adultery is insured that the resources he puts into child care will benefit his own offspring, and his genes will predominate in succeeding generations (p. 327). This mechanism is plausible—though it is rather specialized since it does not explain why women, too, object to their spouses' adultery—but I do not see how it can be tested. We have no fossil remains of the behavior of our evolutionary predecessors. The explanation must remain, at best, pure speculation. (Wilson's data on contemporary societies, given at this point in the text, are irrelevant to testing the evolutionary mechanism.)

One can supply a plausible selection mechanism, indeed several mechanisms, for most kinds of behavior that are observed, as well as for most kinds of behavior that are not observed. Suppose that men, when on long trips, typically invited their brothers to copulate with their wives. We could "explain" this by saying that the husband's genetic fitness was being maximized, in effect, by the efforts of his brother, who carries most of the same genes. This is precisely the mechanism of "kin selection" which Wilson invokes to explain the evolution of altruism (pp. 117–20). If men typically pimped for their wives, this too could be explained, since the profits of prostitution would help sustain the pimp, his mate, and his offspring—all maximizing the male's genetic fitness.

Here is an example of the many assertions that are untestable because of vagueness in their terms: "All social traits of all species are capable of a significant amount of rapid evolution beginning at any time" (p. 145). Even assuming that the words "significant" and "rapid" are well defined, which they are not, I cannot conceive how one could falsify this statement. Any given experiment which failed to produce a change in social traits

would only demonstrate that that particular experiment had failed; it could not demonstrate that the traits themselves were incapable of change.

Wilson's much-discussed claim that altruism has evolved as a major form of behavior in most of the social species from insects to man is another example of untestable theory. I cannot imagine how one could falsify the claim. How, as a practical matter, could one even disprove the conjectured existence of "altruistic genes"? The issue is confused by Wilson's unusual use of the word "altruism." One is said to have acted altruistically if the act increases another person's genetic fitness at the expense of one's own fitness. A woman who gives her life to save her baby is *not* altruistic in this sense, because she is acting to maintain her own fitness (p. 117). If instead the woman gives her life to save a stranger's baby, then that could be altruism. However, if she is a spinster or an old woman whose child rearing is done, then her self-sacrifice for the stranger's baby is not altruism, because death would not affect her genetic fitness. A man who murders his child is altruistic, in Wilson's sense, if by the act he frees resources (e.g., food, shelter) which enable another person to have a child. Needless to say, a simple good deed would not be altruistic if it did not affect anyone's production of offspring. Commentaries on Wilson's work erroneously use his word "altruism" in its conventional sense rather than its special genetic sense. Indeed, he blurs the distinction himself (e.g., p. 120). Much confusion would have been avoided by coining a new term.

As a historical note, the sociologist and naturalist Petr Kropotkin, in his uncited classic work *Mutual Aid* (1903), argued that altruism, in the conventional sense of seeking the welfare of others instead of oneself, has evolved through natural selection in a wide range of species, from social insects to man.

Wilson and I accept as gospel that our social behavior, like our physical form, is a product of evolution; that genes influence some forms of behavior; that studies of other animals will help us to understand man. These ideas have circulated in numerous books and articles; some, such as Alison Jolly's *The Evolution of Primate Behavior* (1972) and Robert Hinde's *Biological Bases of Human Social Behaviour* (1974), I judge to be more useful than *Sociobiology* for sociologists. But they have escaped our attention. Popular works with comparable themes have had ample publicity but have been largely ignored by sociologists—deservedly so. Wilson, with his brilliant scholarly reputation and Harvard credentials, has both the visibility and credibility to legitimate the biological approach to sociology. For me, that is his major contribution.

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BIOSOCIAL MAN, *SIC ET NON*¹

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Entomologists seem to have a knack for writing major and controversial works in the field of social behavior. Who can deny that, since a Harvard-trained specialist in wasps (stet lower case), Alfred C. Kinsey, produced the epochal *Sexual Behavior in the Human Male* (1948)? In the process of empirically documenting the great variability in the forms and frequency of sexual behavior of *Homo americanus*, Kinsey's study unwittingly provided a leverage for sexual pluralism as a radical alternative to the prevailing Puritan dictates of appropriate sexual comportment. Nearly 30 years later, a world authority on ants has produced another major study of social behavior which, since its publication, has aroused a great deal of admiration and passionate discussion, probably more so than any work since Kinsey's. In *Sociobiology: The New Synthesis*, Edward O. Wilson, curator in entomology at the Harvard Museum of Comparative Zoology, has provided an imposing attempt to update Darwinian-derived evolutionary theory by examining and codifying the existent comparative materials pertaining to social behavior.

One does not need much "insider's knowledge" of the subject matter to see why this work deserves high praise. Obviously, only a first-rate scholar with a tremendous cognizance of a wide-ranging literature and with an equally orderly mind could have produced this attempt at synthesizing highly diversified, voluminous materials pertaining to animal social behavior, a literature which is amorphous and characterized by a multiplicity of overlapping terms, ambiguous research findings, and ex post facto theories and explanations. Sociobiology is not an organized scientific discipline but, rather, still at the stage of "natural history"; the codification of the materials, utilizing precisely defined terms and well-articulated processes and mechanisms involved in the evolutionary process, is Wilson's endeavor to bring this field of studies to the stage of a paradigmatic formulation. What Wilson presents us with in this huge and technically splendid book is somewhat akin to a one-volume synthesis of the 16 substantive volumes of the *International Encyclopedia of the Social Sciences*, with the synthesis indicating the logic of the categorizations, their interrelationships, and what directions, if any, seem to be emergent. Perhaps Wilson has not quite achieved such a Herculean task, but surely he has cleaned out much of the Augean stables of sociobiology.

Although not addressed to sociologists, and drawing very little from sociology (despite its title which initially made me think the synthesis intended is one of biology and the social sciences), the study has to be

¹ I would like to express my appreciation to Professor Gregory Lockhead and to Dr. Josefina Tiryakian for their thoughtful comments on an earlier draft of this essay.

treated as an extremely important challenge to our discipline. Why? Because of the underlying reductionist perspective, namely, that human social behavior can be understood in terms of social evolution and group selection, that evolutionary theory and population genetics can uncover the deeper structures underlying social behavior, and that the basic parameters operative in insect society, particularly in ant society, are the building blocks of a general theory of social behavior, of which *Sociobiology* may be taken as a prolegomenon. In setting up the scaffolding, Wilson waves aside assistance from sociology because of the latter's scientific backwardness: "Much of what passes for theory in sociology today is really labeling of phenomena and concepts in the expected manner of natural history" (p. 574). He identifies sociology today as being at a very elementary "phenomenological" level of development. He informs us that cultural anthropology, social psychology, and economics will soon provide sociology with "its phenomenological laws" (p. 574), while "the transition from purely phenomenological to fundamental theory in sociology must await a full, neuronal explanation of the human brain" (p. 575). Wilson may be using "phenomenological" rather loosely, but, in any case, it does not seem that sociology has evolved sufficiently beyond an "elementary form" of science for Wilson to take it seriously.

Consequently, Wilson's biological codification of social behavior offers even less of a dialogue with sociology than Dupree's recent essay (1975), which seeks to bring together biological and sociological theory (in the case of the latter, that of Parsons); to be sure, as Platt trenchantly notes (1975), the end result of that endeavor is to downgrade cultural systems (or to turn the hierarchy of controls of action systems upside down).

Wilson comes closest to adding a sociological dimension to his synthesis in his discussion of "role," which plays an important part in his treatment of the differentiation of behavior among age- and sex-graded categories of individuals within a society. This is to be found in the important chapter 14, where Wilson elaborates his "ergonomic theory" that takes the colony (rather than the individual) as the unit of evolutionary selection. Essentially the theory states that the insect colonies survive best that have an optimal mix of the different castes in the overall production process; the output is the average number of queens and males produced per unit of energy expended by the colony (p. 306). For Wilson, the concept of role, borrowed from sociology by primatologists, is totally inadequate for invertebrate sociobiology and is of limited use in nonhuman primate social behavior analysis. Though he closes this chapter by drawing upon Goffman to highlight the importance of role playing as something qualitatively differentiating human social behavior from that of all other species, Wilson's reductionist tendencies get the better of him in his concluding chapter 27. There he leaves the door open to an ultimate biogenetic account of even the most complex forms of social behavior. So, for example, social stratification and changes in social stratification may be due to group differentials in "upward-mobile genes" (p. 554), and a rigidification in class structure may preserve "genetic differences in mental traits" (p. 555). Even the

qualification that cultural evolution is very rapid and fluid, making a stable ensemble of genes very problematic, can be hedged by the argument that, "Even so, the influence of genetic factors toward the assumption of certain broad roles cannot be discounted" (p. 555). Personally, I might go along with the proposition that human society provides survival value to individuals who have in their genetic makeup a general capacity for role playing, but I am dubious about the merits of imputing behavior patterns to specific "upward mobile" genes, "conformer" genes, *ad infinitum*.

Owing little to sociology and having more to offer to demography than any other subfield of our discipline, *Sociobiology: The New Synthesis* does contain many thematic chapters, however, which are stimulating and perhaps heuristic. Let me mention in this context the chapters dealing with communication (8 and 9), social spacing (12), social symbioses (17), and even sex and society (15)—the latter an eye opener which taught me some facts of life about the birds and the insects which I had never suspected. Reading these materials may enable social scientists to relate some cultural phenomena to a biogenetic infrastructure. For example, after reading Wilson I am now tempted to explain the spring phenomenon of college students heading for a compact beach arena in Fort Lauderdale as an instance of communal "lek" displays (pp. 331-34), and, on a more sober note, his presentation of materials pertaining to termites putting to death the queen who is no longer sufficiently reproductive (p. 437) makes me think there may be some nexus between this and the doing away in several preindustrial societies with monarchs who have lost their capacity to procreate. Suggestive as all this may be, however, I do not leave this volume with the feeling that it offers sociology a new methodology or a new synthesis of social knowledge which would lead us to search for homologues in human society.

The last third of the book offers synoptic materials on the four major groupings (or "pinnacles") of social evolution, namely, the colonial invertebrates, the social insects, the nonhuman mammals, and man. Taxonomic tables present the distribution and degree of sociality, as well as major source references, for each grouping. Although the most controversial aspects of the volume are Wilson's pronouncements on human social behavior, in contrast to the 13-page table for insects, his taxonomic data are skimpiest on man. Thus, table 26-1 (pp. 519-21), which is the synoptic table on the living primates, has a seemingly perfunctory entry on man, who forms, uniquely it seems, both a superfamily Hominoidea and a family Hominidae, there being just one entry (or species) in this peculiar case. And the table which treats general human social traits as either unique, shared with some other primates, or shared with almost all other primates (p. 552) is only suggestive of continuities and discontinuities. The approach to the theory of social behavior undertaken by Wilson depends heavily on generalizations taken from the invertebrates (although the number of unclassified invertebrate species far surpasses the number of remaining unknown or undiscovered vertebrate species). Somehow, I leave with the feeling that for Wilson, the "perfect society" is that of the ants,

marked by a high level of so-called altruistic behavior, a society not only in which is found the contribution of the individual to the survival of his/her "kin" but also in which the caste (or group) is contributing to the survival of the colony and thereby working for the principle of natural selection. The latter remains for Wilson what it was for Darwin, namely, the basic evolutionary principle or central dogma, which serves Wilson as a *deus ex machina* whenever it seems appropriate and necessary.

Very up-to-date in canvassing the social behavior literature of ethology, zoology, and other fields, Wilson does not deviate from orthodox Darwinism in his interpretive schema. That means, in part, that essentially ongoing behavior patterns are explained by tracing them to their genetic origins. How do we get at the origins of human sociality? Well, a Jolly argument invoked by Wilson is that men stand out among the primates because one triggering thing has led to another, and that the crucial adaptive trait of our earliest ancestors was bipedalism, which came about because "Man, in short, became bipedal in order to pick seeds" (p. 569). I find this an instance of functional teleology (i.e., you account for a present adaptation in terms of an anterior state which sought to bring about the present-day circumstances by the set of its actions), which seems to be endemic to most evolutionary explanations. Incidentally, Wilson here takes no note of Ghiselin's recent criticisms (1974) of Wilson's earlier writings as well as his taking to task much of biological thought for its continued teleological foundations. In any case, it seems unfortunate to me that so much of the rather sterile search for origins (of the family, of politics, and other social institutions) and the devising of evolutionary scales, which took up so much energy of 19th-century social scientists who accepted social Darwinism in one form or another, may be repeated anew by today's neo-Darwinists.

By now, no review of Wilson's work would be complete without mention of the storm it has aroused, particularly its treatment by the Harvard-MIT Sociobiology Group. Their first vocal outcry—in the November 13, 1975, issue of the *New York Review of Books*—may have thrown rabid punches in denouncing Wilson's work for its political message. Wade's review (1976) of the dispute, in *Science*, is worth consulting. I did find the Sociobiology Study Group's second collective effort (1976) to be a sophisticated methodological and scientific critique; I underscore the group's conclusion that "the individual's social activity is to be understood only by first understanding social institutions" (p. 186). I am not in accord with them on a couple of minor points. First, to label the sociobiological argument as one of a "deeply conservative politics" is to do an injustice to Wilson. Of course, the genetic structure may be said to be predominantly conservative in nature, but Wilson's pronouncements, in a political sense, are much more "liberal" than "deeply conservative." His discussion of religion and ethics, particularly his landing on the side of moral pluralism (p. 564), is clearly indicative of a liberal orientation—one whose inspiration can be traced back to the genus of 18th-century philosophers/scientists, such as Condorcet, d'Alembert, Diderot, and Helvetius, for whom,

like for their descendant Wilson, intelligence seems to be the crucial factor in the differentiation of human social evolution. Second, where the Sociobiology Group says that "sociobiologists give us no example that might contradict their scheme of perfect explanation" (p. 186), I would substitute "evolutionists in general" in place of "sociobiologists."

In the interest of fair play, one should read as a "postscript" to *Sociobiology* Wilson's rebuttal (1976) of his critics in the same issue of *BioScience*. Frankly, it leaves me as uneasy and skeptical as when I finished the original work. I question his affirmation that "science is now in a position to approach the very origin and meaning of human values, from which all ethical pronouncements and much of political practice flow" (1976, p. 189), for I certainly did not see a rigorous, scientific treatment of "the very origin and meaning of human values" in the rather frantic and sweeping last chapter of *Sociobiology*. I also think that Wilson should make clearer what is "the unique human genotype," what conditions exist today or no longer exist today that were operative during the millions of years of prehistory which marked the evolution of genetic constraints, and just how social progress in the future can "only be enhanced, not impeded, by the deeper investigation of the genetic constraints of human nature" (1976, p. 190).

However chary I am about the significance of sociobiology for sociology, I do think that sociologists need to be better informed about contemporary research and theoretical models in biology. Wilson's *Sociobiology*, and the more comprehensive overview of biology contained in the splendid volume edited by Handler (1970), are the sort of first-rate works which we need to study in order to come to grips with the biological environment or milieu which provides one major matrix of social behavior. This does not mean that the models of biology need to be taken as magic keys for sociological explanation, though some of them in the past have opened up important doors. Recall the various 19th-century biological models which framed early sociological conceptualizations, particularly the many strands of social evolutionism derived from Darwin. Recall here that Alfred Espinas, who became Durkheim's senior colleague at Bordeaux, wrote the first sociology doctoral dissertation (1877), which is still a fine, readable study in sociobiology. Recall also that Park, McKenzie, and others in the Chicago school put to fruitful use, in studying the city, the ecological concepts that had been developed before World War I by plant ecologists Haeckel, Forbes, and particularly Clements. Let me suggest that a critical history of the various interchanges between biology and sociology would be of immense value.

Between a naive ignorance, a polemical rejection, or a docile acceptance of Wilson's *Sociobiology*, with its not-so-implicit biogenetic reductionism, there is another stance I would advocate in evaluating this work and the field behind it. Namely, it deserves to be taken as a stimulating prodding of the sociological imagination. The sort of response to sociobiology which psychologist Donald Campbell gave as his APA presidential address (1975) is very much illustrative of what I hope will be the major socio-

logical acknowledgment of this work. I also hope that a team of sociologists and biologists might be formed to prepare a scholarly assessment of (a) the variability of human social behavior within and among human historical societies, and (b) overlaps and cutoff points between human and nonhuman social behavior. Such a volume (or volumes) would be necessary for a genuine paradigmatic "new synthesis," or what might also be called a "social mutualism," to borrow a term from *Sociobiology* (p. 356), between sociology and biology. The area of sociobiology and its counterpart in biosociology have undoubtedly made some significant progress, and Wilson's book may ultimately be taken as an early pinnacle of a field that needs much greater evolution. Hopefully, the next pinnacle in social behavior will be written by sociologists rather than remain the near monopoly of entomologists!

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Book Reviews

Man in Society: A Biosocial View. By Pierre L. van den Berghe. New York: Elsevier Scientific Publishing Co., 1975. Pp. xiv+300. \$10.95 (cloth); \$6.95 (paper).

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Sociology, we are oft told, is the queen of the social sciences: the most encompassing of the disciplines devoted to a true and general account of the human universe. The sociologist, jack of diamonds, is a pledged knight wielding the sword of objective knowledge in the service of human understanding. Onto this familiar stage Pierre van den Berghe introduces the king, and in so doing changes the whole show—a paradigm shift, as it were. The king, in van den Berghe's family drama, is the principle of male domination personified by the contemporary industrial and political elites who control the funding of the playgrounds where academic knights enjoy the leisure to joust with windmills. The queen is false: her wisdom is the social philosophy of enlightened imperialism. The sociologist, now knave of hearts, serves faithfully by Sanskritizing conventional wisdom, thus furthering the general deception and ensuring his initiation into the priesthood of fools.

In van den Berghe's version of society, man is less than a killer ape. He is a successfully aggressive scavenging primate who, because of his success, must deal with the problem of scarce resources. Van den Berghe suggests several biosocial mechanisms that mitigate the competition for scarce resources—territoriality, hierarchy, gregariousness, etc.—but he is primarily concerned with dominance orders. The archetypal dominance order of human societies is sexual and arises from the sexual dimorphism of the species. Male dominance is the natural order of human societies. The overwhelming majority of societies are patrilineal and conform to patrilocal residence rules. Even in matrilineal societies, authority is held by the men. Polyandry is an exceptional institution, an "ethnographic curiosity" whose circumstances suggest that "it is merely a way of making the best of a bad bargain" (pp. 85–86). Marriage in most societies "is primarily an exchange between men of nonmarriageable kinswomen against marriageable women from other kin groups" (p. 81). The family, despite its well-known virtues, "is a male-adult dominated microtyranny ridden with age and gender conflict" (p. 88). The ubiquity of family conflict suggests that "hierarchy is a universal aspect of even the simplest forms of social organization; and . . . that hierarchy is resented and generates conflict" (p. 88).

Masculine dominance orders mean social inequality. The forms of inequality, be they organized in terms of caste, race, or class, are institutional expressions of the struggle for dominance and power. The sociological tra-

dition of liberal egalitarianism is unconvincing, for inequality is a necessary feature of dominance orders, and the human need for dominance orders appears to be programmed into our biogrammar.

Political and economic institutions are the primary cultural elaborations of the dominance motif. Both are loci of male dominance activities. Both have grown increasingly independent of kinship organization. Modern systems of superficially democratic, managerial state capitalism constitute the most sophisticated human models for stabilizing dominance orders of tyranny and exploitation.

Religion and the arts are secondary structures that serve to legitimate the primary dominance orders, often through celebrating the myth of benevolent paternalism. Neither is seen as an avenue to transcendence. Instead, religion is pictured as an opiate serving to console the vanquished, and art is seen as a luxury commodity produced for the diversion of the elite. The priesthood, secular as well as sacred, is an agency of mystification, and artists are the indulged parasites of the parasite classes. Even play, which some might see as the domain of freedom and spontaneity, becomes a harmless and therefore trivial sublimation of dominance urges.

The book is written from a standpoint of extreme pessimism and alienation. It is an outlaw book, and as such it should irritate many audiences. Liberal academics will not appreciate being portrayed (again) as servants of power who have falsified the record. Radicals who have advocated a conflict sociology will be dismayed to see dominance orders written into the biogrammar of the species. Feminists will be annoyed by the suggestion that male domination is the natural order of society. Activists of varied stripes will object to the contention that there is nothing to be done about our present situation. Nor is there any support for the student who still believes in sunlight. Perhaps this is to the author's credit: "If I leave my readers with a gnawing and lingering sense of dissatisfaction with their own society, their place in it, and my own irreverent and impertinent remarks about it, I shall consider that I have accomplished my purpose. The rest is up to them" (p. 278).

Ex unum pluribus.

Women in the Kibbutz. By Lionel Tiger and Joseph Shepher. New York and London: Harcourt Brace Jovanovich, 1975. Pp. viii+334. \$10.95.

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Tiger and Shepher's work is the most systematic, comprehensive, and painstaking examination available of the process of sexual role differentiation in the Israeli kibbutz. On the basis of extensive demographic data from two kibbutz federations, a subsample of four kibbutzim, and qualitative information based on participant observation in two settlements, the authors document the extent to which this initially egalitarian utopian movement reverted to a sexual division of labor resembling that found in any

modern society. Separate chapters are devoted to the analysis of sex-role differentiation in the various spheres of life: work, communal politics, education, military service, and family life. It is all the more astonishing, then, that despite the thoroughness of the presentation there are but few original findings. Much of the ground has already been covered by others (Y. Talmon-Garber, "Sex Role Differentiation in an Equalitarian Society," in *Life in Society*, ed. T. E. Laswell, J. H. Burma, and S. H. Aronson [Glenview, Ill.: Scott Foresman, 1965]; and E. Cohen and E. Leshem, "Public Participation in Collective Settlements," *International Review of Community Development*, nos. 19-20 [1968], pp. 251-70). Only the chapters on education and the military service report new data.

The intention of the authors, however, was not merely to demonstrate sex-role differentiation as a pervasive trait of the contemporary kibbutz. Rather, they view the kibbutz as a "massive experiment" (p. 5) enabling them to examine "a fundamental question: Do the physical and physiological differences between the sexes inevitably produce social differences in how men and women live and in how communities organize themselves?" (p. 10). The authors promise that they "will report data which . . . convincingly demonstrate that changing social variables designed to provide formal structural opportunities for men and women to act similarly in public life have failed in their purpose in an important and adventurous case" (p. 13). The authors reason that, if even in the kibbutz, which provided all the opportunities for sexual equality, such an equality has not been achieved, then the origin of sexual differentiation in human society cannot be cultural; hence, it ought to be sought in biology and biologically conditioned behavior—ethology. The book, then, is first and foremost an attempt to demonstrate the relevance for sociology of modern biological theories and theories of animal behavior. Hence, what is at issue is whether the authors indeed demonstrate that such theories succeed where other sociological theories fail.

Curiously, though they present a mass of data, the authors fail to examine those variables which are directly relevant to an ethological explanation. Their argument rests, strictly speaking, on the demonstration of *congruence* between their data and ethological theory. This congruence makes their argument plausible but does not really prove it. Everything depends on whether other, more specific and parsimonious explanations are possible. Tiger and Shepher do not really succeed in showing that they are not, despite their efforts (pp. 263-69). In particular, they disregard the power of an orthodox "functional" explanation, namely, that the exigencies of childbearing and child rearing made it more expedient to move women from production to child- and consumption-related services, thus performing on the communal level roles which are similar in content to the role of the urban housewife. Their failure to examine carefully the interrelationship between changes in sex roles and general changes in the kibbutz and its position in Israeli society prevented them from giving proper weight to such an explanation.

Though the main thesis is respectably presented, the book is quite

annoying in other respects: the description of the "Way of Life in the Kibbutz" (pp. 34-67) is superficial and does not incorporate the many findings of recent studies. Though it is claimed that the book is based on "two years of participant observation" (p. 72) in two kibbutzim, the presentation of qualitative data dispersed throughout the volume is more in the nature of anecdotal reporting than serious anthropological description. The book suffers from an unabashed ideological bias in favor of the kibbutz way of life; this often induces the authors to naive and inaccurate statements like: "The Kibbutz has been conspicuously successful with other aspects [except the equality of sexes] of its ideology" (p. 269). They tell us little of the profound changes which transformed the modern kibbutz. It almost seems that they desire to overemphasize their approval of and loyalty to the kibbutz (of which one of the authors is a member) despite the implications of their findings on sex-role differentiation. There is also a gratuitous and unnecessary apology of Israel's right to self-defense (pp. 204-5).

A major flaw of the book is bad editing. Data which even the authors find "not surprising" (pp. 104, 106) are repeatedly presented in different forms. The style of the book sometimes is that of a research report and sometimes suffers from flowery, quaint rhetoric. The English is often faulty. Printing errors and quotations of authors whose works were not included in the bibliography (e.g., pp. 184, 185, 188) do not add to the pleasure of reading.

Women and Islamic Law in a Non-Muslim State: A Study Based on Decisions of the Shari'a Courts in Israel. By Aharon Layish. New York: John Wiley & Sons, 1975. Pp. xix+369. \$22.50.

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This is both an interesting and valuable book, as it deals with an important subject which is rarely discussed, namely, the current change in the status of Muslim women (and, by extension, that of the Arab family) in the context of Israeli society. The author, Dr. Aharon Layish, was formerly a deputy adviser on Arab affairs to the Israeli prime minister, and even though this book is based on his doctoral dissertation, it is clear that Layish is also sensitive to the special problems involved in the position of the Arab-Muslim minority in Israel. His data consist primarily of the judgments and orders contained in the records (*sijjils*) of the four Muslim or Shari'a courts in Israel, those at Jaffa, Acre, Nazareth, and Tayyiba, supplemented by interviews with judges (*qadis*), *Official Gazette* statistical data, and newspaper accounts. The period covered extends from 1948 to the late sixties.

Although he focuses on the status of women, Layish's objectives are much broader. His concern really is to evaluate the position of Islamic law or Shari'a (especially as it pertains to matters of personal status) in a

modernizing non-Muslim state which has actively sought to impose a unified secular law. "This study attempts to demonstrate through the medium of the *shari'a* courts, how the Shari'a has reacted to the challenges thrust on it in an alien environment" (p. xvi). The *shari'a* courts in Israel are thus considered to constitute the principal arena where Muslim jurisprudence, local custom, and secular legislation meet and are reconciled. Within this arena, the religious judges or *qadis* emerge as the pivotal figures, for it is they who interpret the various legal codes, resolving their inherent inconsistencies as well as accommodating local custom. Moreover, these judges function as the mediators between the Muslim population and the representatives of the state. Layish is fully aware of their important role and includes opinions and rulings of several named judges on key issues; but he does not discuss the *qadis* in the detailed manner befitting their crucial role in the evolution of Muslim society in Israel.

The book is divided into a number of chapters, each devoted to a schematic presentation of one basic aspect covered by the Muslim Law of Personal Status, such as age at marriage, dower or bridewealth, divorce, etc. The author summarizes the traditional orthodox Islamic law regarding the issue at hand as well as the modifications introduced in 1919 by the Ottoman Family Rights Law before dealing with the Knesset legislative acts and their present impact on familial relationships.

Apart from the ban on polygyny, the most important piece of Israeli legislation affecting the status of women is the Succession Law of 1965, which instituted the principle of absolute rights with respect to how much of one's estate could be willed, and to whom. Under the Islamic Inheritance Law, a person can only dispose of one-third of his estate before his death; the rest goes in fixed proportions to his legal heirs, which include the women. Thus, even though the woman's share was always less than the man's, she was assured of a measure of protection. Now, under the new secular law she can be totally excluded. Thus, whereas the original objective of the Knesset was to upgrade the woman's position by allowing her an equal share of any estate, in practice the new law often works to her disadvantage. "Moreover, the Knesset, in its eagerness to impose on the Muslim public progressive norms, tailored to Israeli society, has sometimes harmed more than helped Muslim women. The ban on polygyny has caused an increase in the divorce rate and reduced the opportunities for levirate marriages (by which the dead man's brother marries his widow), which are the structural solution for widows wishing to remain united with their children at the house of the husband's family" (p. 330).

The sporadic character of Israeli secular legislation as well as the mixture of two legal systems based on different norms have certainly led to a number of anomalies and stresses which have disturbed the traditional balance of rights and duties in the Arab Muslim family. However, one must bear in mind the fact that these very same anomalies and stresses that are changing the traditional roles as well as the structure of the Muslim family may have their source outside the legal system. Certainly the improved social position of Arab women in Israel is related to their

increased access to education, their joining the labor force, and the transition from extended to nuclear family, all processes implied under the general rubric of "modernization." This is not to detract from the importance of the deliberate legislative efforts to improve the position of women, particularly in matters of divorce and succession, but to draw attention to the need for caution in interpreting causal relationships. As Layish writes, "The importance of the secular legislation lies mainly in increasing the wife's legal security by the creation of a mechanism enabling her to realize her rights through the threat of penal or other legal sanctions. The mere existence of such a mechanism, even if it is not frequently resorted to, may to a certain extent deter potential offenders" (p. 331).

It is certainly difficult to draw up a balance sheet to assess the current position of Muslim women in Israeli society; to do that, we need to have valid knowledge of their traditional status (i.e., pre-1948), something we do not have except in a piecemeal and impressionistic way. Nor can we, for that matter, talk in any meaningful way of their present generic status: Muslim women by no means form a homogeneous group; and, as Layish indicates, there are wide variations between the urban and the rural ones, the educated and the illiterate. On the other hand, to point out the complexity of the problem is not to criticize the value of studies like the one at hand. As a student of Middle Eastern society, I found the book both informative and suggestive. Not only does it provide specific information about the problems involved in the application and manipulation of a number of different legal codes dealing with family roles and relationships, but it throws some light on the evolution of the traditional, patrilineal, patriarchal family in its adaptation to an alien sociopolitical environment. This subject lies at the very heart of all studies of the global process called "modernization."

For Harmony and Strength: Japanese White-Collar Organization in Anthropological Perspective. By Thomas P. Rohlen. Berkeley and Los Angeles: University of California Press, 1974. Pp. ix+285. \$12.50.

Robert M. Marsh

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This is a well-written account of one of Japan's largest banks, called Uedagin, which employs some 3,000 persons in 120 branches throughout Japan. The aim is "to explicate selected fundamental matters of organization in terms of the general Uedagin context" (p. 259), most particularly, to understand how this particular company "arrives at the degree of integration it does" (p. 266). Most of the fieldwork centered in the bank's home office, located in a regional urban center of 800,000 people, and was done during 1968-69.

Rohlen's initial interest was to study the bank's "spiritual training" of its personnel, and he admits he pays less attention to its business oper-

ations than to the ways in which employees are integrated into the organization.

For Harmony and Strength is an excellent example of its genre—the descriptive ethnography—emphasizing the meanings of the actors themselves, distinctive cultural symbolism and ideology as they operate in everyday activities, and, in general, the cultural variation within ostensibly universal, standard, bureaucratic organizational forms. Its 11 chapters develop a rounded picture, describing and analyzing the basic form of the company, songs, ceremonies and ideology, mobility into and out of the organization, the office group, senior-junior (*senpai-kohai*) relationships, promotion, pay and other aspects of the reward system, the bank's union, socialization, dormitories and company apartments, and marriage and the family. Although the author tends to dismiss causal analysis as too narrow and mechanistic, he does resort to explanations in terms of "viewpoints [which] dominate the organization"—the company ideology of community and the logic of business. The nature of work groups, the family, and the *senpai-kohai* relationship also "greatly color the company system and its ideology" (p. 260).

The main contribution of the book is to add to the literature in English the first major study of a Japanese white-collar, commercial organization, thereby filling a gap in the case studies available, which concentrate on various industrial manufacturing firms and their blue-collar workers. To Rohlen's credit, he is not so carried away with the bank's "harmony and strength" ideology that he fails to note the numerous points of tension, dissatisfaction, and conflict.

Earlier studies of "Japanese organizational style," though focusing on manual workers in factories, noted that the core of the "paternalism-life-time commitment" model was the higher-status, male white-collar segments of the organization. In contrasting the variations in integration into the organization of these two strata of employees, these earlier studies anticipated many, if not all, of Rohlen's main findings about bank employees. In consequence, while the Western reader for whom this is the first study of a Japanese formal organization will learn much, those who know the literature will find Rohlen's account all too familiar. However, some particular ethnographic points are brought into new, bolder relief here.

On the negative side, the book suffers the defects of its virtues as an anthropological case study of a single organization. Though Rohlen makes frequent references to other case studies of organizations in Japan and the West, what is always lacking is rigorous, systematic, *comparative* analysis of organizations, where the number of organizational units is large enough to really test propositions about the variable relationships between organizational properties as such, as well as about the behavior, attitudes, and values of members of different organizations. In other words, Rohlen's perspective (as he would admit, I think) illuminates the culturally distinctive behavior of people *within* an organization but tells us little about variations in the structure and functioning of organizations as such, and therefore contributes little to answering the central scientific question:

Under what conditions do organizations vary? In concentrating on only one set of conditions—national culture and subcultural meanings and symbols—Rohlen largely ignores the range of sociological properties (size, technology, complexity, etc.) that make for variations among organizations, even among those within a single industry, such as banking.

The main advances in the *comparative* analysis of organizations in recent years have been made not by anthropologists, but by such sociologists as Robert Blauner, Joan Woodward, Charles Perrow, the Aston group, and Wolf Heydebrand. Rohlen seems to reject this perspective, but one is not sure he really knows it, since most of these writers are not mentioned. Rohlen in part admits this limitation when he says, "The major problem, to sort out the sources of variation in both [Japan and the United States] so that their comparison may be more precise and revealing, is still to be satisfactorily accomplished" (p. 63).

A second weakness is in the area of theory. Rohlen grants that he has not made any theoretical innovations, but he does not meet even a lesser standard of theory. Rohlen praises the fieldwork method because it avoids "one theoretical approach" (p. 268), but his theoretical eclecticism is more apparent than real. Those theories which stress universal factors, and the theoretical categories of the observer, rather than only the concepts of the actors themselves, are not given a fair hearing. Rohlen thus falls into the common trap of treating competing theories as warring humanistic "schools of interpretation." The pity of all this is that competing theories are not brought to bear on the same phenomena, and their alternative explanations are not competitively tested. Admittedly, each theory is oriented to some problems rather than others, but a number of the questions with which Rohlen deals have alternative explanations.

My central objection to Rohlen and other "culturalist" students of organizations brings together the problems of both systematic comparison and theory. To assert that cultural context is the most fundamental determinant is to commit the fallacy of premature theoretical closure. Before one uses culturally distinctive meanings, etc., as explanatory variables, other formal and structural dimensions along which organizations vary—size, task structure, interorganizational relationships, goals, etc.—should be allowed to explain as much of the variance in the dependent variable(s) as they can. All too often, culturalist studies of organizations attribute to "cultural context" what probably is more a function of these structural properties. As long as we have only studies of the Rohlen type, we shall remain unable to answer a central question in organizational theory: To what extent do organizations and their members vary as a result of differences in cultural setting, and to what extent do they vary as a result of structural factors that cut across differences in national culture?

Despite these criticisms, *For Harmony and Strength* is a stimulating book and a pleasure to read. As a case study it deserves a wide audience. My criticisms can be summarized by saying that the faults of the book lie not in Rohlen's execution of the method of the cultural case study, but in the method itself.

Cultures in Conflict: The Four Faces of Indian Bureaucracy. By Stanley J. Heginbotham. New York: Columbia University Press, 1975. Pp. vii + 236. \$20.00.

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Yet another social scientist has discovered that Indians are different from Westerners. In this case, the differences are seen to lie in their respective conceptual models of how bureaucracy, and perhaps the world as well, should be organized. And that, the author tells us, is why Indian bureaucracies do such a poor job.

This newest approach to understanding the mysterious East seems to posit four different "cognitive models of organization" (p. 10) deriving from the Indian experience that, taken together, influence bureaucratic structure and performance. These cognitive models can be described by posing the traditional-modern dichotomy against the Indian-Western dichotomy in a two-by-two table. In the first cell, we find the "dharmic" category, a marriage of village culture and high culture that emphasizes caste and one's duty. It is both Indian and traditional. In the next cell, we have the Gandhian model. This emphasizes the dignity of traditionally demeaning work and the importance of selfless service. It is Indian also, but it is modern. In the third cell, we have the British colonial model: meritocracy at the top, elaborated rules and supervision at the bottom. That model is Western and traditional. Finally, there is the community development model, which emphasizes mobilization at the grass roots, villager expression of felt needs, and facilitation of villager demands. This goes in the Western and modern cell.

One could quibble about what is traditional, what is modern, what is Western, and what is non-Western in these models, but the problems with them are far more serious. To begin with, the author never specifies who carries these models, or where each operates, or how they influence each other, or the circumstances in which one or more might come into play. Second, these models omit what was probably the most important influence on bureaucracy in India, the Moghul administrative tradition. That tradition shaped the administrative service for the British as well, and the Indians retained many aspects of it after independence. Third, and most important, the descriptive materials the author provides rarely seem to fit any of his four models. Instead, one can equally well understand his examples as solutions to problems inherent in the subordination-superordination relationship and in the control and deployment of large-scale organizations. Two examples will illustrate both how the author is confused by his models and how the behaviors he describes can be understood much more easily if one bypasses his approach.

The first example concerns the Nyapati family, members of which were deeply entrenched in key positions at the lower reaches of the British administration. This family "systematically falsified records on a massive

scale, extorting millions of rupees from the villagers and diverting it from the British coffers into their own pockets" (p. 35). Why? Not, according to the author, because they were greedy. Rather, "their loyalties were to their own social and religious institutions" (p. 34). Now, any student of bureaucracy and administration knows that revenue collection is a ruler's greatest problem: How does one keep tax collectors and revenue officials from pocketing the funds intended for the ruler? Solutions have ranged from close supervision of agents, to frequent transfers (so officials cannot develop an independent power base, to the Moghul solution of setting a fixed sum that agents must remit while allowing them to keep all the rest they can collect. These Nyapati fellows did just what revenue collectors have done the world over. If one had to assign a cognitive tradition to them, the Moghul one strikes me as most appropriate.

The second example is a little more complicated, but in its outlines represents a classic bureaucratic malfunction. In this case, low-level officials were given tasks to perform in implementing a program. The nature of the tasks, however, was such that superiors could not easily measure whether or how well their subordinates had performed them. The superiors dealt with this by getting the subordinates first to set a variety of targets and then to report on the degree to which these targets were achieved. The subordinates, who were overworked anyway, figured out which few aspects of the program their supervisors really cared about and ignored the others, thus violating the intent of the program they were administering. The supervisors, realizing that something was amiss but having insufficient information to identify what that something was, asked for more reports. The results were predictable: The papers piled up, and the low-level workers became disillusioned and frustrated at spending all their time filling out forms. One has only to look at the New York City school system as documented by David Rogers (*110 Livingston Street: Politics and Bureaucracy in the New York City School System* [New York: Random House, 1968]) or at Peter Blau's analysis, *The Dynamics of Bureaucracy* (Chicago: University of Chicago Press, 1955), to see similar patterns. The author's analyses repeatedly strike one as naive, ignoring as they do the behavior of officials in much of the rest of the world—even the supposedly superior, instrumentally oriented Western world.

The book has other features that detract from its utility. Theoretical and empirical sections are juxtaposed, but the one fails to illuminate or inform the other. The theoretical material is highly abstract and reads—perhaps unwittingly—like ill-digested Parsonianism: "The construct of a cognitive model of organization permits a more sophisticated introjection of cultural dynamics into the analysis of organizational dynamics than has heretofore been attempted" (p. 11). The descriptive materials, by contrast, are so concrete and detailed—indeed, the author seems unable to leave anything out—that their utility is likewise impaired. For example, the author describes a particular encounter by relating first that he had had his dinner and was comfortably situated in government lodgings. A man rode up on a motorscooter. The author then reports what this man

was wearing, what he had around his neck, what he had on his forehead, and how he carried himself physically. These descriptive materials are not analyzed at all. Apparently, their relationship to the theoretical material is meant to be self-explanatory. It isn't.

At its best, this book is another case study of local-level Indian bureaucrats. The analytic models the author imposes, for academic respectability, on the data do not enhance our understanding, but impede it. Indians probably are different from Westerners. Certainly, they have different beliefs about the nature of the world and their position in it. These beliefs may even have consequences for the way they behave in large-scale bureaucratic organizations. But the author's elaboration of formal models does little to illuminate these differences. It does, however, contribute to the cultural chauvinism that informs so much of this type of analysis.

Change in Contemporary South Africa. Edited by Leonard Thompson and Jeffrey Butler. Berkeley and Los Angeles: University of California Press, 1975. Pp. xv+447. \$16.50.

Martin Legassick

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This book is concerned with identifying processes of structural change in South Africa "*away from* the present coercively and racially stratified society" (p. 170). Through four sections of the book—"Changes within the White Oligarchy," "Changes among Africans," "Intercaste Relations," and "External Factors"—there is a remarkable level of agreement among the 17 contributors (whose initial offerings were to a conference in the spring of 1974) as to what changes are occurring. The white oligarchy is growing more pragmatic, with Afrikaner nationalism and *baasskap* deemphasized in favor of white identity in a plural society; power is devolving to increasingly assertive Bantustan leaders; the changing racial allocation of work in the economy has potential effects for the bargaining power of black workers. The differences emerge in the interpretation of these changes. Some contributors (perhaps especially Albie Sachs, Michael Savage, Sean Gervasi, and Sam Nolutshungu) see them as constituting essentially "reactionary reformism," a perpetuation of the status quo under new conditions. A number of others, perhaps most strongly the editors, see the changes as presaging more fundamental restructuring of the distribution of power and wealth: "Although current reforms may be reactionary in intent they will become revolutionary in effect. . . . Blacks will inexorably generate enough power sooner or later to effect fundamental change" (p. xiii). Not so clearly spelled out is another, and not entirely identical, debate: Will this fundamental change be achieved by escalated reform or by revolution?

Thompson claims that these different interpretations are based simply on different value premises, and, indeed, with the concepts and the information provided, this tends to be the case. Despite its length, the book

has important historical and conceptual gaps. Historically, it is only the white oligarchy that is flattered with any systematic treatment of changes in policy over time (the chapter by André du Toit is here the most insightful). Despite some scattered remarks, there is no real assessment of the historical tradition of African worker and peasant organization and consciousness, of trade unions, or of nationalist organization. For this, a good survey of the contemporary politics of Kwazulu (L. Schlemmer and T. J. Muil) and a methodologically weak "anthropological" survey of African attitudes in Soweto (P. Mayer) are no substitute. Nor is there any conception of "industrialization" as, not an abstraction, but a process existing in history whose initiators, reproducers, and beneficiaries need examination, which is here accorded mainly to "Afrikaner nationalists." Moreover, without a systematic investigation of the historically changing organization of work in industry and agriculture, assertions about new African "skill levels" and consequent increased bargaining power are vacuous and speculative in the extreme.

A focus on these questions might well reveal that what are here presented as innovative changes in a previously static system are in fact only moments in a cumulative process of historical change which, within an overall context of racial differentiation in South Africa, has affected different whites and different blacks in various ways at various times. This might then provide some levers of purchase on the question, debated between Sachs and Heribert Adam, of whether in the current context the effects of current change will be "co-opted" or will encourage rising expectations among blacks. In particular, though various contributions hint at the existence of divergences of interest within the black population, there is an overall tendency to accept the "actors' definition of the situation," whether it be the racial categorization of the South African government or the "African nationalist" and more recent "black consciousness" responses of sections of the black population. Particularly when several contributors remark on a potential "deracialization" within South Africa, it might not be inappropriate to consider the variable of *class*. Considered in terms of class interests, "black power" becomes an ambivalent concept. Does it mean greater power for what might be called the black petty intelligentsia? Does it mean power for black peasants or black workers? What are the different conditions of change which could create either of these situations? What are the differential effects of current changes on such groups? Within the conceptual structure of this book, there is little access to an answer.

Such increments of change as are occurring in South Africa are being subjected in the Western world to intense scrutiny. This, argues Gervasi in his essay, is a reflection of the "Kissinger doctrine" aiming to bolster a minimally reformed white-dominated South Africa against the increased successes of South African liberation movements. Two significant recent South African events are the African strikes in Durban in 1973 and the repercussions of the coup in Portugal in 1974. The timing of the conference from which this collection derives, and even of the revised papers, was such

that the implications of neither event are adequately reflected. What these events, seen in the context of Kissinger's policy, do seem to suggest, however, is that the different assessments of change in this volume derive not so much from value premises as from the expression of different, and conflicting, interests. In practice, common desires for "fundamental change" are forced into the polarities of endorsing either minimal (and perhaps reactionary) reforms from above or revolutionary change from below, because that is how the forces at work present themselves. It may be, as Thompson argues, that the internal changes that are occurring will transform incremental into fundamental change. But it is hard to see how this "inexorability" can turn from an idea to practice without the continued pressure of events "from below."

Elites and Power in British Society. By Philip Stanworth and Anthony Giddens. Cambridge: Cambridge University Press, 1974. Pp. 261. \$6.95.

William G. Roy

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Elites and Power in British Society is the report of a working conference held at Kings College, Cambridge, in March 1973. It not only is a succinct account of the state of the art of the study of elites in Britain but is an important contribution which should be read by students of elites and power.

The book is an integrated set of pieces, all taking off from a conceptual agenda developed by Giddens in his introductory chapter. The book's major flaw is that the empirical works included fall short of illuminating Giddens's conceptual contribution.

The primary issue confronted by the book is the relationship between economic dominance and political power. The extent to which wealth co-occurs with power is, of course, a familiar theme to all those who have followed the pluralist-elitist debate. Giddens steps out of that arena and argues persuasively for an agenda based on locating and analyzing the *institutional mediation* connecting class and power. Forcefully presented, this is a major contribution to the study of elites and power.

The elitists, Pareto and Mosca, reject economic class as a political factor and treat the composition and succession of elites as purely political phenomena. Mill's "power elite" reunites economy and polity but continues to reject the notion of class. Domhoff misuses the term "upper class" to refer to what was really a wealthy stratum. Giddens brings Marx back into the picture by positing the institutional mechanisms by which class is mediated into power. He asks how control of the means of production (and not just wealth) is concretely translated into control of the means of power. This implies an institutional rather than an individualistic research methodology. Unfortunately, most of the empirical presentations are on the personal level.

Giddens's agenda concentrates on three aspects of elites:

1. *Recruitment*.—Giddens challenges sociologists who equate fluid social mobility with democracy, as well as those who equate personal elite origins with elitist actions. Instead, he suggests, the capability of reserving elite positions for people born and bred in aristocratic society is but one mechanism by which an established aristocracy maintains control of society. Recruitment must be viewed in this light and must be studied in relation to other mechanisms. Personal origins has been one of the most heavily researched topics of elite studies, and despite Giddens's playing it down, it subsumes the lion's share of empirical data presented in the contributions. We learn from the readings that men from aristocratic origins who attended one of the Clarendon schools and have degrees from "Oxbridge" continue to dominate high finance, big business, the Anglican church, and the ranks of higher civil service in Britain.

2. *Structure*.—Giddens is not the first to make explicitly problematic the relationship between "structural" and "moral" integration of the elite, but he states the issues well. High cohesiveness is an asset for any group realizing its interests. Giddens argues that structural cohesiveness (interlocking directorships, common social affiliation, etc.) does not necessarily imply or create moral cohesiveness (a common world view, a sense of "we-ness"). Nor is structural cohesiveness a necessary condition for moral cohesiveness. More research is needed to learn the conditions under which the two converge or diverge. Unfortunately, this is the issue that receives the least empirical attention. There are several studies that examine the connections among elite members, but this is not tied to data about moral cohesiveness. Two of the articles—on financial-industrial interlocking directorships and on Anglican bishops—acknowledge the need for such data but present none.

3. *Power*.—Giddens provides some needed conceptual clarification in what has been a muddled field by outlining some useful distinctions between terms like *ruling class*, *governing class*, *power elite*, and *leadership groups*. He presents a typology based on distinctions such as decentralized versus centralized authority and broad issue area versus restricted issue area. He then briefly addresses himself to such issues as the separation of ownership and control and the decomposition of the traditional British aristocracy. It is left to John Rex in a concluding overview to tease out some of the implications of Giddens's approach. Giddens defines power as the ability to effect desired outcomes. Rex emphasizes that the proof of power is in the payoff, which comes in a currency other than power. This raises the issue of the economic benefits of power. His perspective combines Marx and Weber. Economic power is defined as control of the means of production. But we are taken beyond Marx to look for the mechanisms by which control of the means of production is translated into political power. These mediating mechanisms are what we associate with Weber. It is in the institutional structure of society that economic power is related to political power. In this perspective, relations among elite individuals in institutional sectors are seen as projections of the institutional operating

relationships among the sectors. In other words, it is argued that elites be studied not just as sets of individuals but as agents of interacting collective actors in the pursuit of collective interests.

The empirical works, which form the bulk of the book, are addressed to the agenda in Giddens's introductory chapter. Elites are defined by position as the people who occupy formal roles of higher authority in various social institutions, including finance, industry, university, and church. The overriding empirical issues throughout are: (1) the extent to which incumbents of elite roles are drawn from the traditional aristocracy via upper-class institutions and (2) the extent to which memberships in various institutional elites overlap. There is a good deal of original data presented which should be of keen interest to those interested in British society. Particular subjects covered include: the distribution of political power among sectorial elites, as seen in a Dahl-like study of major postwar national decisions (Hewitt); the overlapping of industrial and financial directorates (Whitley); a demographic profile of large-company chairmen (Stanworth and Giddens); a myth-debunking field-observation study of the behavior of boards of directors of a sample of corporations, which students of economic power have long sought and which delivers concrete data on issues that have previously only been speculated on (Dahl and Winkler); a no-surprises account of intergenerational transmission of wealth (Harbury and McMahon); a closely related one on inheritance and occupation (Rubinstein); a replication and updating of Kelsall's classic study of higher civil servants; and studies on the recruitment and connections of university elites (Wakeford and Wakeford) and clerical elites (Thompson).

The reader is treated to a potent dose of information on the men who control the span of British institutions. However, the data do not fulfill the agenda to which they are addressed. Whereas Giddens proposes interpreting characteristics of elite individuals as clues to institutional structures, most of the data on recruitment are used to show how common aristocratic origins reveal the class loyalties of powerful individuals. Whereas the reader is urged to rethink the significance of overlapping elite memberships, interlocking directorates are interpreted as further evidence that a single group is in control. Giddens exhorts the reader to go beyond identifying *who* controls in favor of identifying *how* institutions are controlled. The data demonstrate little in this regard.

A notable exception is the observation study of corporate directors, which sheds new light on the operation and power of directors. The debate on management control of corporations is addressed by the observation that, while directors (especially interlocked directors) are easily manipulable by corporate managers on matters of operation, they regularly assert their authority on allocative decisions. Moreover, they do so with an acute awareness of operating within the framework of a capitalist economy and act accordingly to maximize the position of the firm in capital markets.

The portrait of British elites that emerges from the book conforms closely to our situation on this side of the Atlantic. A major difference

between British and American elites is the importance in Britain of the traditional aristocratic gentry and its institutions. British institutional elites continue to be drawn from this segment of society, although the significance of this is not agreed upon by the authors. Giddens contends that aristocratic institutions are a means by which a ruling sector maintains its power in successive generations, while Rex suggests that perhaps these anachronistic trappings, especially as displayed by symbol-manipulating institutions such as university and church, serve the function of making liberal reforms, in contrast to old-line conservatism, look good to the working class. However, comparison with the United States makes one skeptical of the necessity of such institutions to elite operation. While there are aristocratic institutions such as Domhoff's Bohemian Grove or Baltzell's metropolitan men's clubs, external aristocratic trappings do not surround them. And while there are Groton and Harvard, these hardly monopolize ascendancy to elite status as Eaton and Oxbridge do in Britain. In short, membership in American upper-status organizations is as much a reward of elite incumbency as a route to it.

It is unfortunate that the data and the theory do not fit together better. The theory presented argues for an institutional approach to the study of elites, but the data are primarily personal. Recruitment to elite positions is discussed theoretically as one of many structural mechanisms to ensure continuity of aristocratic domination. However, it is presented empirically as documentation of elite hegemony. Likewise, we are exhorted to differentiate between moral and structural integration and to search for the conditions under which they coincide and diverge. But there are no data on moral integration nor even an indication of how we would recognize it if we found it.

Despite methodological shortcomings, the book should be read. For those with a particular interest in British society, the data presented here are invaluable. Those with a theoretical interest in elites, stratification, power, and related issues will find the book a highly significant contribution that not only stands on its own but should generate further work as well.

The Bohemian Grove and Other Retreats: A Study in Ruling Class Cohesiveness. By G. William Domhoff. New York: Harper Colophon Books, 1975. Pp. 116. \$2.45.

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Recently social scientists have come to understand more and more the influence on scientific theory and research of what Thomas S. Kuhn calls paradigmatic assumptions. Alvin Gouldner is basically correct: a sociological theory is often accepted or rejected by a social scientist because it is compatible or incompatible with his own "background assumptions." G. William Domhoff's latest work will probably experience the same fate as did his other important works (most notably *Who Rules America?* and

The Higher Circles). Sociologists of a less crusading nature will pass over this work as representing "sloppy" research, while sociologists of a critical nature will accept it as a significant contribution.

Perhaps more than any other area of sociological research, the "power elite debate" has been clouded by what can only be described as politically significant background or paradigmatic assumptions. Much of this is due to the extremely difficult nature of the research required in this area; C. Wright Mills was only the first of many to point out the problems involved in obtaining valid data on the upper class. It seems that when adequate research is hard to achieve, background assumptions can have great influence on theory construction and validation (especially when the questions are extremely sensitive to political values).

But beyond the background assumptions involved, we must understand the point from which Domhoff begins. His study of the Bohemian Grove represents only a piece of the puzzle that was laid down in his earlier works. His basic thesis, taken partly from C. Wright Mills and partly from ruling-class theorists, is essentially that there is a unified upper class which through various means has developed into a governing class in contemporary America. Domhoff's earlier works were guided by what he termed a "sociology of leadership method." His primary task was to demonstrate first that there was a somewhat unified upper class and then to show that this upper class was a governing class as a consequence of its overrepresentation in key leadership positions in our society. One of the most persistent criticisms of the governing-class thesis made by pluralists has been directed to the alleged cohesiveness, consensus, or unity within the upper class. In his study of the Bohemian Grove and other upper-class retreats, Domhoff attempts to provide more data on this question of upper-class unity.

The book has three parts: the first and most important looks in detail at the upper-class summer retreat operated by San Francisco's exclusive Bohemian Club; the second deals with two other "upper-class" summer retreats (the Rancheros Visitadores in southern California and Roundup Riders of the Rockies in Colorado); and the third part is a summary of the significant theoretical issues related to the above-mentioned upper-class-unity argument. I state that the first part is the most important because the third is basically a summary of much of Domhoff's previous work and the second concerns two retreats which are, by his own admission, not of the Bohemian Grove's status.

The importance of the 94-year-old Bohemian Grove to the question of upper-class unity is indicated by some of the following membership characteristics provided by Domhoff: 27% of its 928 resident members are listed in the San Francisco Social Register, 45% of its 411 nonresident members are listed in other social registers in the country, and, in 1968, 40 of the top 50 corporations and 20 of the top 25 banks had representation. These facts are significant for Domhoff's overall thesis, because it is partly at retreats and clubs like the Bohemian Grove that members of the upper class come together, get to know one another and each other's views, and work out some consensus regarding the major issues facing the country

(and big business) at any given time. Though the Bohemian Grove was established as a retreat from its members' worldly problems of running corporate America, Domhoff's researchers and inside informants describe how, in fact, these issues are often of primary concern. An example of this is the daily political and intellectual seminars at the retreat featuring prominent professors and such speakers as Henry Kissinger, Melvin Laird, William P. Rogers, and Richard Nixon (a longtime member in good standing).

Turning to the overall significance of the study, the pluralist's response will no doubt be, So what? Domhoff has shown that some wealthy members of the economic and political elite spend time together socializing and discussing mutual problems. But does he show that a consensus or united front is thus established to deal with their common problems? We must conclude that Domhoff does not. And further, even if Domhoff were correct in the above assertion, would this help show that this "governing class" can make national decisions which favor the exclusive interests of the corporate rich? Again, we must conclude that it does not. Domhoff has problems in his other works with answering the question of how much overrepresentation by the upper class in key positions in the society is enough to say it is a governing class (and in specifying further the consequences of this overrepresentation for upper-class interests); similarly, he has problems here with showing the outcome of these clubs and retreats with respect to upper-class unity. All Domhoff is able to do at this point is to introduce evidence from a number of studies in psychology on the process of opinion formation in small groups.

Domhoff's most critical problem, however, is that he overstates the significance of his data. Contrary to his belief, the book does not provide "significant evidence for the existence of a cohesive American upper class." But what the book does provide is no less important. Along with studies like those of E. Digby Baltzell, it does provide another small bit of research indicating that the governing-class argument is at least plausible. At present, we know much more about the poor in our society than about the rich. But with more work along the lines of that presented in this book, we can at least be optimistic about the chances for the elimination of this gap in our knowledge. And it is only when more research is done in this area that we will be able to reduce the influence of politically relevant background assumptions.

In short, Domhoff's book represents only a modest study. But even with its limitations it is at least a beginning and deserves attention by everyone interested in the question of political and economic power in a postindustrial society.

Generational Change in American Politics. By Paul R. Abramson. Lexington, Mass: Lexington Books, 1975. Pp. xvi+140. \$13.50.

David G. Lawrence
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Paul R. Abramson begins the last chapter of this book by stating that he has "demonstrated some basic trends in American politics, and by studying age-group differences, [shed] light on the process by which they occurred" (p. 123). He has indeed done so.

Abramson begins with a very brief overview of American party systems up to the class-based New Deal system which emerged in 1936, producing a high level of class voting between 1936 and 1948. Contrasting the Campbell and Alford positions on post-World War II trends in class voting, he finds Campbell to be essentially correct: class voting has decreased in the postwar period, albeit nonmonotonically, falling from a rather high level in 1948 to a very low one in 1956, increasing moderately again until 1964, and then falling off again and disappearing entirely in 1972. For the most part, Abramson is convincing on the decline of class voting, although the pattern would be far less clear (showing no particular post-1952 trend) were the 1972 disappearance of class voting to prove merely a short-term response to the extraordinary presidential election of that year.

Abramson argues that the changing level of class voting is due to generational change in the electorate: voters who came of political age after World War II are considerably less likely to vote by class than are their elders. The postwar period has seen only a very slight decline in the extent of class voting among those who were politically mature before the war, while citizens who have come of political age since then vote in a way much less related to class than is the case for their elders. The primary generational change in class voting has been an increase in the proportion of middle-class citizens who vote Democratic, a phenomenon only slightly explained by the increased tendency of members of the new middle class to be children of traditionally Democratic blue-collar or Catholic parents. These younger citizens constitute a constantly increasing porportion of the electorate, and their level of class voting shows no sign of increasing over the elections in which they have been eligible to vote.

But although the middle-class young are more Democratic than their elders, the Democrats even in 1972 did not get a presidential majority from these citizens. Nor did the Democrats in 1972 do better among the middle-class young than among the working-class young; only in 1972 and only among the very young voters who came of political age in the post-Eisenhower years was the traditional tie of class to voting reversed, and even there only slightly.

Abramson argues that the trend toward less class voting is likely to continue: citizens born before 1924 will continue to constitute a decreasing proportion of the national electorate, and younger middle-class voters, given their social origins and positive orientation toward government activity in the economy or toward race, are likely to continue to vote more

Democratic than their elders. This last argument is perhaps somewhat less convincing than what precedes it: the introduction of attitudinal factors is somewhat ad hoc, and the behavior of the youngest respondents, who have been observed in two elections at most, may be particularly due to short-term factors.

In chapter 4 Abramson changes his focus from class voting to party identification and the recent decrease in partisanship in the electorate. Despite the arguments in *The American Voter*, he finds a generational explanation for weakening partisanship to be more convincing than a life-cycle explanation: there has been no increase in partisanship of the postwar cohort over time, as a life-cycle explanation would suggest.

Abramson pays explicit, systematic attention to the attitudinal bases of voting behavior only in analyzing the 1970 and 1972 survey data. He finds that attitudes on race and toward established authority are largely independent of party identification but are related to vote choice in 1972, particularly among young voters. He finds no generational differences in attitudes among relatively uneducated citizens, but the college-educated young are considerably more liberal on race and hostile to established authority than their elders are. Generational change has taken place, in other words, but only among the educated. This change has created a situation in which young, well-educated whites constitute the segment of the population most similar in political attitudes to blacks; there is at least some attitudinal basis, Abramson suggests, for a political coalition of the socioeconomic top and bottom against the middle.

This book is a welcome addition to the literature on voting behavior and electoral change. It is written in a straightforward style, tightly focused on a limited number of theoretically significant substantive problems and attentive to existing literature in the field. One major substantive complaint should be made, however. Abramson's analysis of the attitudinal bases of vote choice, class voting, and electoral change seems to me to be highly underdeveloped. His explicit examination of attitudes is limited almost exclusively to 1970 and after, and he tells us nothing about the attitudes conducive to high levels of class voting. Examination of the attitudinal mechanisms underlying class voting early in the period would be quite useful for sensitizing us to the process through which class voting takes place and to the extent of present attitudinal change and its possible consequences.

Measuring Occupational Inheritance. By Thomas W. Pullum. New York: Elsevier Scientific Publishing Co., 1975. Pp. xiv+184. \$12.75.

Keith Hope

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The first four chapters of this short monograph discuss quantitative techniques which have been proposed for the detection of patterns or regularities in a mobility table. Most of the methods reviewed treat the categories of

such a table as nominal rather than ordered. The discussion is avowedly demographic and presociological in intent. Later chapters elaborate methods of assessing the degree of fit of a model to an observed table, and the methods are applied to several sets of tables in an exploration of models of independence and quasi independence.

The author discusses and dismisses coefficients for the comparison of observed and expected tables and, rightly in my view, comes down in favor of the index of dissimilarity, which is the minimum number of people who would have to be moved to bring one table into line with the other. He wishes, however, to use the index to compare different-sized tables and models with differing degrees of freedom. He therefore norms the index by dividing it by the same coefficient computed for the distance between the observed table and the "most different table" (MDT). Discussion of the MDT and presentation of an algorithm and program for computing it are central to his work. It is not clear whether the norming is successful or not, since it seems to wash out most of the differences between tables. It may be that a further constraint should be imposed on the MDT to ensure that it lies on the projection of the line joining the observed to the expected table. If the index is to be considered as a distance, it seems plausible to demand that two distances should lie in the same direction before one is divided by the other. This is easier said than done.

Several chapters in the book make use of the iterative technique for adjusting one table to get as close as possible to another, which was introduced by Deming and Stephan for the work of the U.S. Bureau of the Census. The method was later employed in the statistics journals to fit a multivariate frequency distribution, and it was subsequently brought to the notice of research workers by Bishop, Goodman, Kullback, Mosteller, and others. It is not, however, widely realized among sociologists that the technique was invented, I believe independently, by Richard Stone at Cambridge, and it has been much used by economists in the analysis of input-output matrices. They refer to it informally as "the RAS method." Its properties have been exhaustively explored by M. Bacharach (*Biproportional Matrices and Input-Output Change* [Cambridge: Cambridge University Press, 1970]). I mention this here in the hope of persuading sociologists to take the economists' work into account in their efforts to impute substantive meaning to the technique.

Pullum indicates that he does not intend to present an analysis of the logical and sociological status of his various techniques and the concepts associated with them. Nevertheless, he does drop hints from time to time which settle some conceptual issues and raise others. I propose to pursue these issues in the remainder of this review in the hope of arriving at an invidious, even if dogmatic, assessment of the sociological worth of these complicated mathematical methods.

Pullum explicitly says that the assays of his models are not tests of significance but measures of relative extent of failure to fit—hence his emphasis on norming the index of dissimilarity. I am inclined to doubt the value of models which fit so loosely to the data that tests of significance

are deemed inappropriate. My preference if a model fails to fit the data at anything like an acceptable level of significance (in a moderate-sized sample) is to resort to descriptive quantification such as the plotting of categories in the canonical space of a mobility table. (It is not often appreciated that the canonical decomposition of a contingency table into additive "contingency hierarchies" is a multiplicative or log-linear analysis which differs from other such techniques only in that it involves a posteriori maximization rather than a priori estimation; see K. Hope, "Quantifying Constraints on Social Mobility: The Latent Hierarchies of a Contingency Table," in *The Analysis of Social Mobility: Methods and Approaches*, ed. K. Hope [Oxford: Clarendon Press, 1972].) Pullum's reanalysis of Blau and Duncan's 17×17 mobility tables reinforces this opinion: the investigations are long, complex, based on arbitrary models, and in the final analysis uninformative. They fail to detect the important class distinction between self-employed professionals and other occupational categories which is evident in the canonical space. Pullum is to be congratulated on making a serious attempt to apply the demographic techniques to relatively disaggregated data. The failure of the attempt is a contribution to progress.

One is accustomed, in sociology, to models which achieve only a modest fit to the data, and it may be asked why a high level of fit should be demanded of mathematical models of mobility processes. The answer is that the mathematical or demographic model is, conceptually, trying to achieve a fit to just a single point, albeit one with several degrees of freedom. The mobility table is, as Pullum rightly says, a macrosocial characterization of a whole society. It is quite definitely not being treated as a statement about how people arrive at their jobs; in attempts to answer this latter question, father's occupation features as only one among a number of determining variables. The theory appropriate to the generation of models which will fit mobility tables must therefore be macrosociological. And since the classical argument of students of society has been that social forces are revealed in all their purity by being filtered through large aggregations of people, there is not a great deal of scope for whole societies to deviate from the positions to which theory would assign them.

What then has gone wrong? The answer, surely, is very simple. The models which generate the expectations have no grounding in theory, and so there is little reason to investigate one model in preference to another.

Much of the obscurity which shrouds mobility analysis is a consequence of the hazy nature of the categories in the typical occupational mobility table. These categories are so vague in their connotation that they can be assigned several different meanings to suit the analysts' several purposes. Categories which were initially supposed to represent quasi status groups are later employed as a means for quantifying degrees of occupational "inheritance." But being in the same status group as one's father is not in any sense equivalent to inheriting his occupation. In general, we cannot assume that people in a particular cell of a mobility table are homogeneous with respect to characteristics which might enable us to decide whether to "block" that cell in constructing a model of quasi independence. We

cannot assume that people in a diagonal cell are uniformly the same, or that people in an off-diagonal cell are uniformly different, in the relevant respects. The mover-stayer model, for example, makes sense if "staying" means that a worker has retained the same job, employment, or occupation. It makes sense, too, if "staying" means that a man is in the same distinguishable class or status group as his father. But it does not make sense to confound the two approaches and to pretend that being in the same class or status group is an instance of occupational inheritance.

This point becomes obvious as soon as one asks how one would set about delineating the space of a macrosociological theory in which each society would appear as a point to be predicted. Clearly there would be little mileage to be gained from comparative analysis in terms of classes or status groups, since only hard-bitten ideologues would suppose that classes or status systems are isomorphic across societies. In order to seek out differences between two societies, one must postulate some respects in which they are the same. Occupation presents itself as a plausible vehicle of macrosocial analysis, because, in general, an occupation in one society is readily mapped into an occupation in another, and this is even more true of categories of occupations (provided they are constructed with comparative analysis in mind). If our level of analysis is, as Pullum points out, insipidly macrosociological, and if macrosociology is confined to the study of occupational categories, then we have no grounds on which to identify men as manifesting social, economic, or biological *inheritance*. How then can we identify a particular cell of a mobility table as a candidate for "blocking" or similar a priori manipulation? Only by reference to submacrosociological considerations: we may have a theory which tells us that certain mechanisms of transmission are important, for example, private schools, or inheritance of capital, or membership in a social network. Then the direct method of testing this theory is not to make ecological assumptions about whether a particular category or cell is "loaded" with such mechanisms, but to include the supposed factors explicitly in the analysis as independent variables. We may believe that, of two boys starting off at the same level and rising to the same degree, the son of a shopkeeper tends to become a manager while the son of a skilled tradesman tends to become a professional. In order to investigate intergenerational transmission of this sort, it is necessary either to define the occupational categories so as to reflect such distinctions (as we have, to some extent, in constructing the Hope-Goldthorpe scale of British occupations) or to introduce equivalent variables into the analysis. Few people doubt that specific tracks of mobility occur and can be revealed by sufficiently detailed investigation, but they are not the stuff of macrosociological theory, which paints on a broader canvas.

The qualitative techniques we are discussing are able to reify the categories of mobility tables because those categories are so hazy in their definition. The clearer we become about our categories, the less we are willing to claim that our classification scheme captures all and every distinction of status, class, capital, elite membership, and power.

Where the techniques come into their own, even when they are applied to hazy categories, is in *comparative* analysis, over time and between societies. Haziness is not, of course, to be preferred to clarity, but in scientific work one may use inaccurate instruments in a standardized manner in order to effect comparisons. Furthermore, in comparative analysis one is able to dispense with the assumption, implicit in some of the analytic techniques applied to a single table, that the row and column distributions of a mobility table are on the same logical footing. R. Mukherjee ("A Further Note on the Analysis of Social Mobility," in *Social Mobility in Britain*, ed. D. V. Glass [London: Routledge & Kegan Paul, 1954]) and O. D. Duncan ("Methodological Issues in the Analysis of Social Mobility," in *Social Structure and Mobility in Economic Development*, ed. N. J. Smelser and S. M. Lipset [Chicago: Aldine Publishing Co., 1966]) have pointed out that a mobility table does not represent exchange between categories of the occupational structure at two points in time. One consequence of this is that inflow analysis may be performed (if the whole working population has been sampled) because the distribution of respondents reflects the occupational structure, whereas outflow analysis cannot. Another consequence is that the initial stage of a Markov analysis can only be the calculation of what I call "provenance probabilities," that is, *backward* transition probabilities. However, those probabilities, having been applied to the starting vector once, cannot be applied a second time. The second step cannot be taken, because it must start from the same sort of occupational distribution as the first step, but the first step delivered the analysis to a different sort of occupational distribution.

When we subject a single table to these kinds of analyses, we implicitly, and erroneously, treat the two dimensions as if they were on the same conceptual footing. However, when we perform a comparative analysis we compare fathers to fathers and sons to sons. Such matchings are not impregnable (in any particular case it might be possible to show that factors such as differences in differential fertility cast doubt on the match); nevertheless, they have *prima facie* plausibility. The data for the 1972 British social mobility inquiry have been coded (rather uncertainly) into the Hall-Jones categories employed in the 1949 inquiry (D. V. Glass, ed., *Social Mobility in Britain* [London: Routledge & Kegan Paul, 1954]). I have compared the two data sets matching fathers to fathers, respondents to respondents, and birth cohorts to birth cohorts, and I have found no significant or patterned evidence of change in the underlying pattern of mobility. R. M. Hauser and his colleagues at Wisconsin have undertaken a similar analysis of American data (not broken down by cohorts; see R. M. Hauser et al., "Temporal Change in Occupational Mobility: Evidence for Men in the United States," *American Sociological Review* 40 [1975]: 279-97, and "Structural Changes in Occupational Mobility among Men in the United States," *American Sociological Review* 40 [1975]: 585-97, where my as yet unpublished analysis is alluded to). The next stage of the work is to compare the American and British patterns of mobility.

Lacking strong theory, what we are doing in these analyses is generating

expectations on the basis of comparisons between periods or societies. If we find invariances or regularities, then we have a macrosocial problem crying out for a theory to explain it. It must always be borne in mind, however, that an invariant pattern of exchange within one set of occupational categories, hazy or clear, does not entail invariance if the same data are coded into an alternative set of categories.

It is no criticism of a methodologist to say that his technique is not very helpful in dealing with the data which he had in mind when constructing it. When we are elaborating a new method of analysis, any data are grist to the mill. Once the technique exists and its properties have been demonstrated on (possibly inappropriate) data, then the practitioner is at liberty to apply it in quite different fields. O. D. Duncan is currently applying Goodman's cell-blocking techniques in the analysis of intransitivities and Guttman scales, and that, rather than the analysis of mobility tables, would seem to be a profitable future for the concept of quasi independence. Pullum is to be congratulated on making more explicit than his predecessors the conceptual problems and assumptions of the demographic analysis of mobility tables. The lesson I draw from his endeavors is that some other field of application should be found for these models.

As far as mobility research is concerned, I would expect illuminating *descriptive* accounts of mobility in a single society to emerge from the study of relatively homogeneous categories such as those which have been constructed for the British inquiry. I would expect strong *theoretical* propositions at the individual level to emerge from the application of ratio-scaled variables such as the utility of income and the social standing of occupation (R. L. Hamblin, "Social Attitudes: Magnitude Measurement and Theory, in *Measurement in the Social Sciences*, ed. H. M. Blalock, Jr. [Chicago: Aldine Publishing Co., 1974]). And I would expect problems and propositions of macrosocial analysis to emerge from the sorts of comparative analysis I have briefly outlined.

Introduction to Structural Equation Models. By Otis Dudley Duncan. New York: Academic Press, 1975. Pp. ix+180. \$12.95.

Thomas W. Pullum

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It is quite likely that Dudley Duncan's impact on the conduct of quantitative sociology exceeds that of any other currently active social statistician. Much of this impact has resulted from his exemplary use of techniques new to sociologists, as in *The American Occupational Structure* (with Peter Blau [New York: John Wiley & Sons, 1967]). Much has been directed through his many student-apprentices who have caught his spark. Some of his influence has been through relatively expository works which are accessible and understandable to a rather large readership—such as *Statistical Geography* (with R. P. Cuzzort and Beverly Duncan [Glencoe, Ill.: Free Press, 1961]) and the classic article on path analysis in this

Journal (72 [July 1966]: 1-16). The present book fits into the latter category, and this review is intended for its potential readers.

Introduction to Structural Equation Models should be understandable to anyone who has successfully completed a graduate social statistics course which adequately treats multiple regression. A few other books have been marketed for this slot in recent years, but most of them seem aimed at an unrealistically sophisticated audience. In the present book, Duncan avoids matrix algebra and specific testing procedures, for example. His emphasis is consistently on the assumptions, the logic of the substantive problem and its representation with a model, and the limitation on our inferences from statistical models. There is not the slightest attempt to duplicate econometrics textbooks. His orientation is precisely the one that is needed if the uses and misuses of structural equations models are to be understood by the majority of sociologists. Duncan would be the first to say that the techniques he describes, most of which originated in the high-status field of econometrics, can and have been used to mystify, to obfuscate, and to generate large numbers of coefficients which the reliability and validity of the measured variables simply cannot sustain. A major function of this book will be to raise the general literacy in the subject, so that the misuses will be exposed rather than glorified out of ignorance.

The models under discussion can be represented by a set of equations or, equivalently, by a diagram. The connecting links in the diagram are referred to as paths. However, so that the prevailing terminology may be better recognized, I might mention that the term "path analysis" is now out of vogue. This initial term was coined by Sewell Wright when he developed a form of the procedure within genetics. It was this line of research that Duncan showed to sociologists in his 1966 article. However, the technique had (and has continued to develop) a much more general framework within econometrics under the labels "errors in equations" and "structural equations." The terminology of this more developed intellectual tradition is in the ascendancy in sociological applications.

Duncan considers the following fundamental themes. First is the distinction between recursive and nonrecursive diagrams (in the latter there may be reciprocal or feedback effects). In nonrecursive diagrams there may be a shortage or (yes!) an excess of information concerning the magnitude of an effect; there is a very clear discussion of these two possibilities, under- and overidentification. The third major issue is the impact of specification error—of having the wrong model, in the sense that, for one reason or another, some variable is omitted. If an omitted predictor variable is correlated (in fact) with an included predictor variable, then the estimated effect of the included predictor can be wildly off the mark. This is a pervasive and haunting problem which cannot be wished away.

Two other issues are raised which require a more complex presentation and which, in a course, would be better deferred until some experience has been built up. These are the related matters of measurement error and multiple indicators. Elsewhere there has been considerable detailed work on these issues, much of which, in my view, has little application to actual

substantive research. Duncan confines himself to the more useful aspects of this work.

There are some exercises but virtually no examples of substantive research in the book. Published examples are referred to and would be indispensable to a genuine comprehension of the procedures. I hope for a future edition which will include several full-scale illustrations of all the reasoning and trial and error involved in reaching and estimating the "final" model.

A review of this book should not pass over its last chapter. Its essence is that statistical techniques are not a substitute for a theory and that they cannot provide a recipe for sound research. These are points which almost all social statisticians recognize but which very few of us have fully internalized. (The same could be said for most other methodological orientations.)

A final observation on the writing style: On several occasions Duncan refers to a diagram and discusses alternative modifications. In some of these he refers to the researcher as "he" and in some as "she." I find this refreshing and preferable to a consistent use of "he" or of the cumbersome "he or she."

Social Indicator Models. Edited by Kenneth C. Land and Seymour Spilerman. New York: Russell Sage Foundation, 1975. Pp. xi+424. \$15.00.

Glen G. Cain

University of Wisconsin

This book is an outgrowth of the Conference on Social Indicator Models held in 1972 at the Russell Sage Foundation. It contains 14 papers by 17 sociologists and one economist. If one sentence can express the motivation for the conference and book, it might be the editors' claim that "both for reporting changes in social conditions and for the guidance of social policy, there are great benefits to be derived from the analysis of interrelationships among social indicators" (p. 2).

The term "social indicators" signifies quantitative measures of the social health or well-being of the nation and its constituent population groups. The case for the development of social indicators is often advanced by the analogy with economic indicators. With the indispensable aid of national income accounts and econometric models, economic indicators cover a variety of policy-relevant indices, some that are well known, like GNP, unemployment rates, the inflation index (CPI), per capita income, and poverty statistics, and some that are not so well known, like the full-employment deficit, energy reserves, unemployment insurance exhaustees, and measures of economic discrimination experienced by women and minority groups. For nearly 30 years the executive branch of the federal government has issued an annual Economic Report of the President, compiled by the influential Council of Economic Advisors; during the past decade proposals have emerged for a corresponding social report.

What sort of report might this be? What indicators would be reported? What social accounts, social forecast models, and theoretical framework would be used? For those who seek answers to these questions, *Social Indicator Models* will probably be disappointing. Except for Kenneth C. Land's introductory chapter, these questions are not directly confronted. Instead, the chapters report on research techniques, plans, and works-in-progress of the sort of research that has been carried out in sociology prior to the 1966-69 period, when the call for social indicators surfaced. I find the contents disappointing on their own merits. The empirical research reported is skimpy, and the methodological techniques are not new, generally are not explained as well as in a good textbook, and, most unfortunately, are not carried through to any application that persuades the reader of their usefulness—except in the most general way. We already know that probability-transition matrices, regression analysis, and contingency tables *can* be useful.

Land's "Overview" chapter reports the historical background and early thinking about social indicators, and he provides several taxonomies for indicators, sources of data, and analytical techniques. But it is all very general—arrows point from variables labeled "exogenous" to variables labeled "endogenous"—although sometimes the vague concepts are mixed with specific analytical terms in a jarring way, as in the following passage: ". . . it is usually assumed that objective conditions and subjective value-context mutually affect each other and that these two determine subjective well-being, although not in any simple linear, additive fashion" (p. 27). The remoteness from empirical application of this implicit model of subjective well-being makes the final phrase in that passage comical.

The second chapter, "Theoretical Domains and Measurement in Social Indicator Analysis," by Arthur L. Stinchcombe and James C. Wendt, is almost entirely methodological. The main messages seem to be: (1) that functional form in quantitative models is important, and, in particular, that time as a variable may be additive or interactive; (2) that time may represent either "time to adjust" in an environment "held constant" or a changing environment; (3) different sources of data—cross section or time series, for example—constitute measures of different social processes and these, in turn, affect the variances and, therefore, the explanatory power of the variables.

"The Log Linear Analysis of Survey Replications," by James A. Davis, is also an exposition of methods and also warns that the regressor variables, x and t (for time), may have interactive as well as additive effects. The discussion becomes more complicated as he develops a dummy variable specification for a contingency table involving two endogenous variables, say y_{1t} and y_{2t} , along with x and t , which are also dummy variables. The y dummy variables are aggregated to form proportions and transformed as the logarithm of the proportion of occurrences over one minus the proportion, that is, the log of the odds of the event, y . (The models have become prominent in sociology by virtue of several cited papers by Leo A. Goodman and are elaborations on the logit model of Joseph Berkson. Economists may

find the logit model discussed in Henri Theil, *Principles of Econometrics* [New York: John Wiley & Sons, 1971], pp. 632 ff., where Berkson's works are also cited.) Davis introduces the complicated case of multiple dependent variables by examining the conditional expectation of, say, y_1 given x , t , and y_2 . To the extent that I understand the models, however, I think Davis's treatment is weakened by the inattention to the underlying causal model and to the problem of correlated disturbances between y_1 and y_2 .

The chapter by Otis Dudley Duncan, "Measuring Change via Replication of Surveys," and that by Beverly Duncan and Mark Evers, "Measuring Change in Attitudes toward Women's Work," are both brief progress reports of current research using the Detroit area surveys. Both use the log linear model for contingency table analysis described by Davis. O. D. Duncan's chapter describes the use of a graduate student research seminar for carrying out research with replicated surveys. The example of research in this chapter is an analysis of the effects of age, education, and cohort on responses to a question, asked in 1956 and again in 1971, on whether "the younger generation should be taught by their elders to do what is right" or "to think for themselves even though they may do something their elders disapprove of." B. Duncan and Evers's chapter analyzes the effects of mainly age, sex, and cohort on the 1956 and 1971 questions: "Are there some kinds of work that you feel women should not have?" and, if yes, "What are they?" and "Why do you feel this way?" The difficulties in coding are also described. Both chapters seem to emphasize their research methods rather than the substance, which is quite limited.

The chapters with the most substantive empirical research content are by Donald J. Treiman and Kermit Terrell, "Women, Work, and Wages—Trends in the Female Occupational Structure"; H. H. Winsborough, "Age, Period, Cohort, and Education Effects on Earnings by Race"; and Aage B. Sørensen, "Growth in Occupational Achievement: Social Mobility or Investment in Human Capital." Treiman and Terrell discuss occupational trends and analyze the female proportion in 67 occupational categories in relation to the following independent variables: the means of age, hours worked, income, and education of white males in the occupations, and the percentage urban, public employee, and self-employed in the occupations. Two novel conclusions of this research may be questioned. First, they claim that "the general pattern is one in which men who are employed in predominantly male occupations have benefited from the inflationary trend in wages since 1940 much more than have women in general . . ." (p. 189). This statement is misleading because it completely overlooks the rise in female wages for labor market jobs relative to their most common alternative job, in the home sector. The large increase in labor force participation of married women since 1940 is evidence that women may have benefited more than men in general. Whether women who worked full time in the occupation of, say, schoolteacher, salesperson, or factory operative fared as well as or better than, from 1940 to 1960, men who made similar investments in training and experience is a question that cannot be answered by the empirical model used by the authors. Second, they conclude that "there

was no tendency for occupations employing many women to lag in their economic returns to women" and that there is "no support for Oppenheimer's thesis that women move into jobs that pay them well" (p. 184). These propositions confuse dynamic adjustments to disequilibria with different equilibrium positions. Thus, more women can be attracted to an occupation that has an increase in wages, but by examining beginning and end points over a 10-year period, the wage increase will not be observed if the supply response restores the old wage equilibrium. A fully simultaneous model could examine the mutual causality between a change in wages and a change in numbers, but this type of model is not estimated.

Winsborough develops an economic human capital model that is hard for me to follow, because I was never clear about all the assumptions needed to apply empirically a model containing the unobservable variables—raw ability, on-the-job investments, and depreciation—and the observed variables—years of schooling, age, and period (or cohort)—along with interactions among these variables. Winsborough ends up interpreting the observed age and education effects for different periods as reflecting cohort differences in raw ability, which he defines as preschool development of human capital. In any event, the empirical results, using five cross-section surveys from 1960 to 1970, are interesting, although his interpretations are not always clear to me. For example, Winsborough states that the rates of return to education "are highest for the completion of one of the traditional breaking points in the educational system, i.e., 8, 12, and 16 years" (p. 208). However, for whites the highest rate is for 5–7 years and the rate for 9–11 years is higher than for 12, which leaves only the 13–15 and 17 as low. For blacks the highest rates are 5–7 and 17. An attempt to determine a business-cycle/race interaction (p. 211) is not convincing because the only change from a steady rise in economic prosperity over the decade is the mild recession in 1970. There is not much more interpretation of the age–education–period effects on earnings, partly because the results do not provide neat answers and partly because Winsborough mainly emphasizes the technique of pooling several cross-section data sources.

Sørensen also uses the economist's human capital model, but as a competitor to the social mobility model. This contrast is ill-advised. The former deals with personal attributes—supply-side characteristics of the labor market—and the latter deals with the "opportunity structure" on the demand side of the labor market. Both *must* be involved in the determination of the level and distribution of wages, employment, promotions, etc. To test the two models, Sørensen declares that the human capital model assumes that earnings are constant after schooling ends (unless other sources of investment in human capital are assumed), and he finds that earnings are not constant. But this tests nothing, because there are several sources within the human capital model for the typical inverted U-shaped age-earnings profile: (1) economy-wide, secular growth in all wages; (2) a growth due to on-the-job experience or training; (3) growth due to "learning by doing" and/or maturation; and (4) an eventual down-

turn as a consequence of physiological deterioration, technological obsolescence of skills, and the transition to full retirement. There *are* separate human capital (supply) and opportunity (demand) effects, but Sørensen does not separately measure them or prove that one set of forces dominates. One cautionary remark about his methods should be added: all job changes are treated as separate observations, but we may be more interested in the average change in status for an individual making a move than in the average change in status per move. The distribution of job changes per person is severely skewed to the right, so the two questions may have very different answers.

The chapter by Richard Stone, "Transition and Admission Models in Social Indicator Analysis," is a purely expository treatment of a familiar technique, probability transition matrices, but with a number of interesting variations and, to me, enlightening and sobering judgments about the strengths and weaknesses of the method. A similar objective is attempted by Judah Matras, "Models and Indicators of Organizational Growth, Changes, and Transformations," but is more elementary and is discussed in an overly general way. The example Matras offers is internal mobility within a bureaucracy, which is relatively remote from the subject of social indicators.

A third chapter on transition matrices is by James S. Coleman, "Analysis of Occupational Mobility by Models of Occupational Flow." The technique is to use a first-order Markov process to examine mobility among four states—school, military, white collar, and blue collar—for black and white males age 30–39 in 1968. The empirical results are probably not intended to do more than illustrate the method, and it is true that the first-order process is appealing in its simplicity, because it is mechanistic and relies on no behavioral or institutional theory. I seriously question, however, whether these models are appropriate for predicting (let alone understanding) the educational attainment, military service, and occupational mobility of whites and blacks. One may abandon the first-order assumption and/or make the transition probabilities conditional on the abundant data on individual and environmental characteristics, but then the model loses its simplicity.

A chapter by David L. Featherman and Robert Hauser, "Design for a Replicate Study of Social Mobility in the United States," describes the authors' 1973 survey and their plans for analyzing social mobility, with special reference to comparisons with earlier studies. I found their discussion of the policy implications of their research (pp. 225–29) pointed and refreshing.

The last two chapters return to the theme of methodological exposition that characterizes the first two chapters by Land and by Stinchcombe and Wendt. "Models Involving Social Indicators and the Quality of Life," by David D. McFarland, seemed to me to be the worst offender in offering: (1) overly general and simplistic discussions of methodology, specifically of "models" and "benefits from mathematical models," and (2) frustratingly sketchy applications, specifically to population, divorce, crime, health,

sickness, hospitalization, and longevity. In the final chapter, "Forecasting Social Events," by Seymour Spilerman, there is, in addition to some methodological issues (e.g., explanation versus prediction), a reasoned argument for structural models as a means of estimating robust causal parameters. Spilerman includes a discussion of the difficulties in forecasting social events that are rare or subject to fads.

The contributors to this volume are, as Eleanor Bernert Sheldon says in her foreword, "powerful," and the reader will recognize this throughout. Still, the sum result is disappointing.

The Sociology of Housework. By Ann Oakley. New York: Pantheon Books, 1975. Pp. xiv+242. \$10.00 (cloth); \$3.95 (paper).

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Being a housewife is a family role, a gender role, and, although this is often overlooked by all but housewives, a job. Ann Oakley treats housework in this last respect, describing women's perceptions about their work conditions, their standards, and their routines, as well as their satisfactions—or, more precisely, dissatisfactions. She interviewed 40 London wives between 20 and 30 years of age, each with at least one child under five.

Oakley sees housework as alienating. She finds that the majority of housewives are dissatisfied with their work. They do not regard it as intrinsically satisfying or creative but report instead its monotony, fragmentation, and excessive speed. When compared to industrial workers, housewives report these problems as frequently as assembly line workers but much more frequently than other factory employees. Social isolation is an important reason for dissatisfaction with housework. Another is the enjoyment of previous employment. In some cases there are few labor-saving devices, but dissatisfaction is not related to the use of modern equipment. In fact, one housewife expressed high satisfaction even though she lived with her husband and two children in a two-room apartment with a shared bathroom, had no refrigerator, and did all of the family wash by hand.

The work week for these women was long—on the average, about 77 hours of housework including child care. Working hours did not vary with the number of children in the home or with the possession of labor-saving devices. The longest hours were worked by women with high housework standards. But all women specified standards and routines to some degree. Although housework is not structured, housewives feel the need to define it as a job.

Oakley's findings on housework patterns are generally supported by time-use research. However, the contribution of the study lies less in this than in what it tells about the meaning of work, about the perceptions and attitudes housewives have about their job. The study is also important because Oakley introduces conceptual distinctions which clarify existing material on the housewife role. She probes the meaning of satisfaction

with housework, showing that it is more complicated than is usually supposed. Contrary to the conventional view, here, working-class and middle-class women turn out to be equally dissatisfied with housework. But status differences occur in another dimension of satisfaction. Orientation toward the housework role tends to be more positive for working-class women. For example, complaints about the label of "just a housewife" are less common in the working-class than in the middle-class group. Attitudes toward the housework role and attitudes toward what a housewife does (household tasks) are not identical. A working-class woman may identify more strongly with the domestic role. But even this cannot counterbalance the day-to-day dissatisfaction of doing housework.

Oakley also disagrees with the family literature which stresses the sharing in modern marriages. In her interviews, wives say their husbands do little work in the home. She criticizes many of the earlier studies, showing that their method gave a false impression of sharing work.

According to Oakley, the housewife's situation is rooted in childhood, when girls learn to equate their femaleness with domesticity. But the notion of a gender role and early socialization are not sufficient to explain the perceptions and attitudes of contemporary housewives. Oakley has greatly oversimplified the causal forces operating, even ignoring (strangely) her own earlier work. In *Women's Work: The Housewife Past and Present* (New York: Pantheon Books, 1975) she describes how consumerism, the dedication to a rising standard of living, and the privacy of the family amplify the role of women in the home. Moreover, the interviews themselves suggest another cause: housewives express the need to do their part for the family. In the absence of a visible product and earnings, the housewife feels the need to specify high standards and work long hours. To change the housewife role, it would be necessary to alter factors in addition to early sex-role training.

Oakley stresses her feminist values. She criticizes sociology for its male orientation, for studying women much less than men, and for regarding women only in areas of life deemed to be unimportant. A feminist view is also evident in her assertions about the oppression of housewives. Although these housewives did not see themselves as oppressed, Oakley interprets the high dissatisfaction as an indication of the oppressiveness of the domestic role. She implies that housewives are more oppressed than their husbands: they work longer hours and feel they work twice as hard, and their job experiences are reported as more isolating, monotonous, and fragmented and as requiring more speed. The data could, however, be interpreted in an alternative way. Housewives put great value on the autonomy of their job. Certainly their autonomy in work is higher than that of their husbands. Some women say their marriage allowed them to move to a higher social class and enjoy a standard of living they would never have achieved on their own. Finally, a significant proportion of women are satisfied with housework. Are the work satisfaction levels of wives lower than those of their husbands? Do husbands also feel trapped by their jobs? Oakley's approach is too narrow to tell us. When she brings a political

interpretation to the data, her argument becomes weak. Nonetheless, the book is important, providing information about a large but little-studied group of workers.

Adolescence in the Life Cycle: Psychological Change and Social Context. Edited by Sigmund E. Dragastin and Glen H. Elder, Jr. Washington, D.C.: Hemisphere Publishing Corp., 1975. Pp. xi+324. \$14.95.

Elizabeth Douvan

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This book represents the outcome of the third in a series of conferences initiated by the National Institute of Child Health and Human Development in response to a recommendation by its National Advisory Council urging an expanded research effort in the area of adolescence. Eleven substantive papers commissioned for the conference are presented with excellent introductory and commentary-conclusion statements by the editors.

The conference focused on intellectual development and socialization during adolescence. (Previous conferences dealt with biological forces in puberty and nutritional needs in adolescence.) A decision (inevitable, since the mandated area is so broad) was made to seek "thorough coverage of selected areas" rather than comprehensiveness. The book specifies four areas: (1) cognitive development and the social meaning of physical development, (2) analytic issues in adolescent development, (3) social transitions in late adolescence, and (4) adolescence in the sociohistorical process. Each section of the book includes one to three papers meant to sample the area rather than exhaust it. The hope of the conference organizers was to "generate fresh problem foci on adolescence within a life span framework" (p. 3).

By and large, the book succeeds in its goals. It summarizes empirical data in selected important areas of adolescent development, and it draws attention to areas critically needing systematic research. The individual papers are quite variable. Several summarize and integrate the empirical work in an area with an eye to raising the next crucial questions and stimulating theoretically relevant research. David Elkin on cognitive development and Diana Baumrind on early antecedents of adolescent competence are outstanding examples of this approach. A few focus on one particular theoretical model and bring data to bear on that model or its derivatives: Morris Rosenberg tests the effect of dissonant environmental context on self-esteem, and John Clausen assesses somatotypes and development rate as factors in social and personal adaptation. Dorothy Eichorn deals with the effects of asynchrony in development and stresses the methodological knots to be untied before we can move on. Other papers represent other combinations of theoretical, empirical, and methodological emphases. Jessie Bernard's essay on socialization for motherhood is that remarkable amalgam of theory, large-scale data, small exemplary case

histories, historical perspective, and wonderful insight which is her trademark and our rare fortune whenever she turns her mind to a problem.

In working with graduate students who want to do exams or research on adolescence, I impose one stringent requirement: the problem they choose and the literature they use must deal with adolescence as a developmental variable or concern a problem demonstrably unique to adolescents. The fact that subjects for a particular study happen to be in the age range does not mean that the problem addressed concerns adolescence or is theoretically germane to adolescent growth.

Applying this criterion would lead me to question the inclusion of some of the contributions in this book. For example, the Rosenberg chapter: while adolescence is, as Lewin noted so long ago, a period of quintessential marginalism, Rosenberg's concern with the effects of a minority position (i.e., placement in a dissonant context) on self-esteem in no way speaks directly to this marginalism and presumably could be investigated as reasonably in an adult population. One could make a case for it on grounds that self-attitudes first arise in adolescence—as Adelson does for politics—but the article does not make that case. It is a fine article, proposing an interesting hypothesis about contextual effects on self-feelings. I understand why the editors did not want to lose it. But it is not about adolescence, despite the age of its subjects.

As long as I'm grouching about coverage, I may as well mention the two topics I missed and regretted in a book addressed to adolescent socialization: close friendship and personal commitment. Both represent crucial processes by which adolescents are socialized, and both have their beginnings and most focused expression during the adolescent stage. Both are touched on glancingly or by implication in the volume. Obviously they have some part in vocational and marital choice, and commitment is the *sine qua non* of social movements. But neither is addressed explicitly.

Several of the papers discuss age segregation in our society and place considerable stress on the peer group as norm setter. But friendship socializes in special ways, not all of which are captured by the concepts of role and norm. The self and experiments with the self are affirmed in the response of a regarding friend. With its mutuality and sharing, friendship enlarges the adolescent's view of the range of roles and styles of performing them—enlarges the options beyond those available at home and in the family.

Our understanding of friendship and commitment is still vague and sketchy, and data are hard to come by. Howard Becker has made explicit some of the ways in which commitment to a future role or self socializes the individual. An exploration of either of these critical adolescent issues as thoughtful as the essays of Elkind, Eichorn, Baumrind, or Bernard would have advanced our understanding of adolescence enormously. Perhaps the next conference and volume will include them.

But one can always find some pet concern that has not received full treatment in a single volume. And the editors didn't promise it in any case. Two other important volumes have appeared very recently which deal

with adolescence and youth: James S. Coleman, chairman, *Youth: Transition to Adulthood*, Report of the Panel on Youth, President's Science Advisory Committee (Chicago: University of Chicago Press, 1974), and Robert J. Havighurst and Philip H. Dreyer, editors, *Youth*, Seventy-fourth Yearbook of the National Society for the Study of Education (Chicago: University of Chicago Press, 1975). The three volumes mesh well in their inclusions and exclusions, complementing and balancing each other in many areas. The three together constitute a remarkably full statement about the state of the field, a superb exploration of many theoretical and methodological issues, and an outline for further research steps. Together they form an impressive response to the need expressed by NICHD's advisory panel and a signal beginning for this new phase in research on youth.

Leisure: Theory and Policy. By Max Kaplan. New York: John Wiley & Sons, 1975. Pp. vii+444. \$13.95.

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Max Kaplan's book is, in its way, an exemplification of McLuhan's maxim that the medium is the message. He argues in the context of a densely textured analysis of the leisure topic that we are evolving toward a new kind of social order, a *cultivated order*, in which the highest value is placed on creating and sustaining a richly textured environment based on a new fusion of art and science in the service of humanity.

The leisure topic, the author, and the book display this kind of texture, ranging across disciplines (Durkheim, Weber, and Parsons are discussed as knowledgeably as Piaget, Freud, and Jung) and through the ages (Aristotle is cited as frequently as Jacques Ellul—10 times); and the links between the humanities and the social sciences are ever at hand (in the index, Goffman rubs shoulders with Goethe, Riesman with Rilke, and Herman Kahn with Emanuel Kant). As Joffre Dumazedier says in the foreword, "American sociologists are usually blamed—rightly or wrongly—for visualizing the world only through their own country. This is not true of Max Kaplan. There is not a single sociologist in the field of leisure whose international awareness is superior to his" (p. vii).

Indeed, the book is interesting as a companion piece to Dumazedier's own book which appeared in 1974. Dumazedier's first book was called *Toward a Society of Leisure* (London: Collier Macmillan Ltd., 1967). His current book, *The Sociology of Leisure* (Amsterdam: Elsevier Publishing Co., 1974), is the consummation. Kaplan, like Dumazedier, entered the leisure field in its gestation period. Kaplan's *Leisure in America* (New York: John Wiley & Sons, 1960) was empirically grounded in research during the 1950s. The field was launched with a good deal of rhetoric, an astonishing ragbag of fragmented if fascinating aperçus, and an evangelistic ethos that made some of the early participants feel like misfits

as recession palled the imagery of affluence. But Kaplan and Dumazedier have been two of the stalwarts, following up their earlier field studies with mature and integrative overviews.

Kaplan consummates his own intense involvement in the field over the past two decades by basing the current book on an elaboration and extension of his original definition of leisure. The definition, an ideal-typical one of the "chef's concoction" variety, is as follows: "Leisure . . . consists of relatively self determined activity-experience that falls into one's economically free-time roles, that is seen as leisure by the participants, that is psychologically pleasant in anticipation and recollection, that potentially covers the whole range of commitment and intensity, that contains characteristic norms and constraints and that provides opportunities for recreation, personal growth and service to others" (p. 26). On the basis of this he constructs a conceptual model from which he draws 60 propositions, which are strung like beads on a thread with which he seeks to bind the diverse aspects of leisure into a coherent and unified entity.

Seen as a social institution, leisure is analyzed in terms of its structure, functions, symbolism, and organizational clusters of persons, groups, communities, nations, and the world. But the leisure construct presents some pretty intractable problems which even Kaplan's *savoir faire* cannot completely lay to rest. In proposition 22, he states, for example, that "leisure can be used as a social device to maintain and to nurture social and ideological pluralism as well as to provide an integrative or assimilative vehicle among subcultural groups" (p. 61). A more cynical reader might ask for evidence that leisure actually does function more to unite than to divide; and whether, if it can do either, it can be said to have a specific function. Kaplan states in his definition that it is capable of covering the whole range of commitment. He considers that it can provide for personal growth as well as service to others, that it can serve radical critiques of society as well as express acceptance of the established order. Can something that can be so many things to so many people be anything worth designating as a distinctive social institution? This is a problem that plagues the leisure field, and the fact that Kaplan has not yet resolved the issue does not detract from the many merits of the rich mental repast he provides.

As Kaplan expounds his 60 principles, relevant implications are suggested for policymakers, drawing partly on lessons from history, partly on applications of the model to the works of other social scientists in the leisure field. The climax of the book is in his formulation of a social evolutionary thrust in which he outlines the characteristics of the stage that is in process of becoming. In this, the leisure dimension of the postindustrial society, he sees as a key ideal the pursuit of knowledge and the cultivation of the arts. This will be the "cultivated society," in contrast to its predecessor, the "kilowatt society," where technology, materialism, and the consumer ethos were (and, for the most part, still are) supreme.

The reader finishes with no doubt in his mind that Kaplan knows what he is talking about. He has been up and down the country, around the world, back and forth through history, in and out of the disciplines, and

has come out with what must be a standard shelf book for anyone in the social sciences who wants to know what's what in the leisure field. But it is a plateau and not an end, as Kaplan would be the first to admit. There are many confusions still to be clarified, and many other searchings need consummation; but the book provides many leads to follow. May I suggest one? It might not be Kaplan's own choice, but it is certainly something stimulated by his work and therefore worth mentioning as an example of how an eclectic work of this kind can have many uses for many people. Having worked myself not only in the leisure field but in the field of changing roles of men and women, I have been impressed recently with the way in which the intractability of men's work roles has been used as a rationale for resisting alterations of domestic division of labor. The myth of the unique biological suitability of women for domestic work, in Ann Oakley terms, is no longer the basis for male resistance to change. There is now a new myth of the greediness of work organizations generally. Men argue on this basis that they cannot get their organizations to alter enough to make it feasible for them as individuals to give less to their jobs. But Kaplan gathers together arguments highlighting the fact that in contemporary society the meaning of work is a *value* issue as well as an economic one. The requirements of industrial production are shown, in the leisure field, to allow a much greater flexibility than is in fact adopted. This insight and the data on which it is based can be cross-applied. There is the necessary job of demythologizing the work ethic in relation to sex-role change. This book will be useful for that task as well as its own. It is in this way that it may also be useful to others—as a suggestive, stimulating, broad-ranging treatise rather than as a workaday model for leisure analysis.

Leisure and Popular Culture in Transition. By Thomas M. Kando. St. Louis: C. V. Mosby Co., 1975. Pp. xvi+308. \$11.95.

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In his preface, Kando states that his book is intended as an interdisciplinary work "to provide a more or less comprehensive text for the emerging field of leisure and popular culture." He further states that "the conceptual area carved out differs from such established subfields as the sociology of leisure . . . and popular culture" (p. xi). Thus, the book is explicitly not intended as a work on the sociology of leisure and popular culture even though Kando is a sociologist and the foreword to the book is by another sociologist, Don Martindale. However, the publisher is encouraging the book's sale for use in sociology courses, and nonsociologists in the field of popular culture are greeting the work as a sociological effort. While it may be somewhat unfair to the author, I will limit my reactions to the work viewed as a sociological text, since this *Journal's* audience consists primarily of sociologists.

At the outset, let me emphasize that the book has much to recommend it. Kando reviews a large amount of material in a lucid manner generally quite appropriate for undergraduate students. But the book does not attempt to create a general, analytical sociology of leisure or popular culture; instead, it consists of segmented chapters dealing with different aspects of leisure and popular culture, largely as delineated by the nine chapter headings: (1) "The Work Society"; (2) "What Is Leisure, and What Should It Be?"; (3) "What Is Popular Culture, and Is It Good Culture?"; (4) "Toward the Leisure Society?"; (5) "High Culture"; (6) "Mass Culture: The Printed Media"; (7) "Mass Culture: Cinema"; (8) "Mass Leisure: Sports, Outdoors, and Travel"; and (9) "Youth Culture and Social Change." This useful survey of much—but by no means most—of the relevant literature offers little synthesis or real integration.

Thus, Kando examines such terms as "leisure," "recreation," "play," and "games" by outlining the different definitional approaches that others have given to each, but he offers no integrating typology that might shed light on these terminological disputes or which might lead to analytically derived propositions. Instead of treating such basic concepts as constructed types, Kando appears to accept these terms as designating realities (in an almost Platonic, nonempirical sense) which he hopes to understand by examining the different (and often inconsistent) approaches available for each. He argues that "one uses leisure either as an empirical concept to describe existing recreational behavior, or one uses it as a normative concept, an ideal toward which people should strive" (p. 34). Kando takes the latter approach as his "solution," neglecting other alternatives. These might include not only the view of leisure as a multidimensional constructed type with definitional components that behaviors can approximate by degrees, but also the view of work and leisure as social labeling processes similar to those found in the deviance literature. Kando's definition of leisure as a normative concept still leaves it largely undefined except in relation to work, which Kando never clearly defines. Following De Grazia, Kando sees work and leisure as defined by the actor's state of mind toward his/her activity, but this cognitive definition does not help us much when persons are engaged in the same activity but view it differently; nor does it help us with cases where other actors define a person's activity as leisure while the person sees it as work (or vice versa). Such a psychological definition does little to help us with the sociological issues involved.

Although Kando's discussion of the high- versus mass-culture debate is clarifying, he does not attempt to resolve the general issues involved and, in particular, sheds little light on the problem of the demarcation between folk, popular, mass, and high (elite) culture. The major weakness of the book, then, is in its failure to provide a real framework for the analysis of leisure and popular culture and in allowing mere enumeration of the divergent orientations in the literature to substitute for the kind of synthesis so badly needed in this area. This criticism might be viewed as irrelevant to the book's goals (after all, why criticize an author's lack of answers to

questions he never asked?), but this lack of framework is probably the reason for the book's serious faults of omission.

Kando's lack of clarity about the scope and character of popular culture and the absence of any sort of taxonomy/typology for popular culture give him no rules for what to include or exclude. Though purporting to write a general work, Kando neglects most cross-cultural and historical research into popular culture outside the United States. The obvious omissions are detailed discussions of television, radio, and the music industry. This is especially surprising in light of the many recent works dealing with television and violence. Less obvious but possibly more serious areas neglected by Kando include the legal structure surrounding popular culture (especially the implications of copyright laws), the social organization of the industries for popular culture production, the issues of distribution and marketing networks and monopolies, the occupational roles involved, and deviant and illegal forms (e.g., pornography). Though Kando centers attention on consumers of popular culture, he gives little attention to mediating structures and networks. This is especially unfortunate since some of the best sociological work dealing with popular culture is to be found in these areas.

Other weaknesses in the book include its sometimes remarkable aesthetic and value judgments (e.g., reference to the "perversion of an *art engagé* policy" in the persecution of Solzhenitsyn [p. 46]) and Kando's wholesale identification with the values of the counterculture (chap. 9). Factual errors also occasionally creep in (e.g., Kando ignores the federal funding of WPA art when he states that, "until the mid-sixties, the only public support for the arts in America was at the municipal level" [p. 47]). And my own students in a course on the sociology of popular culture consistently complained about the book's wordy and repetitious character. Summaries are useful, but Kando provides summaries of summaries. The book could also well do without some of its overly elaborate sentences.

There is much need for a general text in this area, and Kando has made a good beginning. The weaknesses of his book largely reflect the deficiencies found in the sociology of leisure and popular culture as a whole. Someone needs to restructure the wealth of facts and debates found in this confused area. It may be that, for any such restructuring to be a success, the sociology of leisure needs to be analytically separated from the sociology of popular culture. At present, the field is a hodgepodge of "studies" and research into everything from movies to "Mother" pillows. Kando's work is an important move toward the proper mapping of this domain, but I look forward to the second edition of his book in the hope that it will tackle the basic sociological problems surrounding leisure and popular culture unanswered and neglected by this edition.

Interorganization Theory. Edited by Anant R. Negandhi. Kent, Ohio: Kent State University Press, 1975. Pp. viii+283. \$12.50.

Mayer N. Zald

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This volume draws together papers delivered at annual conferences (1970-73) of the Comparative Administration Research Institute at Kent State, plus three others. Most of the essays are theoretical, often extensions or reworkings of previously published pieces; but several of the essays do present data. Each group of essays is followed by an appraisal or a summary of the critical discussions that took place at the conferences. Two essays, by Negandhi and Bernard Reimann, are reprints of previously published articles based on data from Indian manufacturing firms.

It should be apparent to anyone following the literature that over the last decade and a half the study of organization-environment relations has been the major growth area in complex-organization research. This volume represents a significant summary of this research and theorizing. Many of the authors, such as Jay W. Lorsch, Howard Aldrich, Mark Lefton, Roland Warren, Jerald Hage, Paul White, and Sol Levine, have made major contributions to the growth of the area.

The essays can be loosely grouped as having three foci: (1) organization-environment interaction, where the question is how an organization differs depending on variations in environment; (2) organization-organization interaction, where the major focus is on interorganizational interaction, exchange, and domain consensus; (3) interorganizational fields, where the major focus is on the structure of the network, the patterned shape of interorganizational relations.

In a summary of the preceding decade and a half, several trends and generalizations emerge. First, it is easier to do research on the response of organizations to environment than on interorganizational fields or interactions. Our research methodologies and our conceptual tools are more developed there.

Second, as Negandhi argues in his concluding chapter, although interorganization interaction is fraught with conflict, many of our models have a built-in assumption that the goal of interorganizational coordination is to minimize conflict. Negandhi also notes that power differentials and domination are real but that interorganization theory tends to play this down. He then shows how propositions about power, drawn from the late James D. Thompson, would illuminate power and conflict in interorganizational relations.

Third, our conceptualization of interorganization relations tends to treat as a descriptive or historical fact the role of the state. Several of the essays mention changes in federal policy as the determinant of organizational interaction, but the tendency is to treat such policy as an exogenous variable. The focus is on local organizations locally interacting.

As is so often the case, our theoretical concerns are shaped by the empirical cases we wish to come to grips with. In turn, we then wear blinders

to avoid whole literatures that bear on our concerns. As represented in this volume, the interorganization literature has focused on either multi-product firms or health and welfare agencies. It leads us to ignore much. In this case, we miss the cooperating interorganizational ventures called lobbies or pressure groups, economic literature on bargaining in bilateral monopoly, and a rich literature on public goods, collective choice, and property rights.

Finally, the way in which the field has been defined abstracts the study of organizations from the central dynamics of modern society—the slow, tortuous growth of the social control of organizations and industry. We should be thankful for what we have in this area, but we must push way, way beyond where we are.

Operations Research Models for Public Administration. By Jack Byrd, Jr. Lexington, Mass.: Lexington Books, 1975. Pp. xxi+277. \$18.50.

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University of Michigan

Within the past 30 years, the concept of operations research has evolved as an increasingly appealing technology to the managers of large and complex systems. This volume makes a useful contribution as a basic-training manual or guidebook to some of the most common perspectives and techniques. The first part of the book, a "General Framework for the Operations Research Study," provides an overview of systems analysis followed by chapters on the planning of the specific study and the use of information systems. There are good sections on PERT (Program Evaluation and Review Technique) and CPM (Critical Path Method). Part 2, "The Development of Operations Research Models," discusses some common operations research methods, including linear programming, network models, simulation models, and inventory replacement models. The last part, "Implementation of the Operations Research Study," contains one chapter on implementation itself and another on the future of operations research in the public sector. The appendix contains a table of compound-interest factors. The author offers to supply by mail (and quickly provides) a set of problems which could be assigned to students.

Applied sociologists will find the book useful because it describes types of tasks they are asked to undertake by governments and others but do not always know how to accomplish. For this same reason, the book would be useful as a supplement in methods courses so that those in training can be exposed to this line of thinking.

The techniques and their application are clearly presented. The bibliography, though not lengthy, is certainly adequate. There are, however, several difficulties with the book, and these problems seem, ironically enough, to be of the same type that operations research experiences in the larger society. Basically, Byrd tends to be uncritical about the application of operations research in several serious ways.

First, he does not deal with some of the serious limitations and difficulties which have already been experienced in the use of operations research within the public sector. The New York City/RAND Institute operation had important problems, and we are not exposed to them in this volume. That experience is mentioned briefly and uncritically. Byrd seems less sensitive than one would like to organizational culture and structure as limitations to the operations research enterprise. He does take into account three levels of organizational activity (operational, strategic, and policy) but seems unaware of the fact that these are quite similar to Talcott Parsons's three levels (technical, managerial, institutional) and, in fact, with the exception of Amitai Etzioni, to the large organizational research literature in general.

Second, the title raises expectations which are not fulfilled. He does not actually talk about the application of operations research to public administration; he teaches the methodology through public administration examples. Although chapter 2 is titled "Description of the Public System," it is simply a discussion of "systems" in general, not of the special problems of governmental systems. Yet in the last chapter he quotes (p. 242) Raitt's list of special problems in such applications, and then only in the context of the "future" of operations research: (1) The degree of interdependence between elements presents difficulties of boundary definition for analysis. (2) Researchers have little experience in dealing with the purpose or goal-seeking properties of components of such systems. (3) Performance criteria in such cases may be complex, qualitative, or undetermined. As a case in point, Ralph E. Strauch ("Squishy Problems and Quantitative Methods," *Policy Sciences*, vol. 6 [June 1975]) provides some thinking on how such problems might be handled. In short, the techniques are presented without an attempt to deal with the problems raised in the context of application.

Finally, Byrd bypasses discussion of those ethical problems that the operations research practitioner must face, including questions about the scientific method itself. In the public arena of decision making, the "objective" posture of science is often severely questioned, and the researcher needs to be especially aware of the assumptions underlying his or her work.

In fairness, the author sought to write a beginning, not an advanced, textbook. But a textbook has a special responsibility to present the full range of salient issues. Byrd does succeed in showing that operations research can provide a fresh perspective on public programs, if not a completely unambiguous one. Indeed, in our interest-group society, where factions have relatively well developed goals in mind, we may find that operations research can provide a lens through which that elusive image, the public interest, can be specified. If, for example, the differing preferences of the relevant groups on some issue could be placed in an overall model, then perhaps an "optimal" solution could be offered. In sum, Byrd's volume is a useful beginning. A little more attention to the larger issues would have been welcome to do justice to operations research itself, and as a guide to the student.

Queuing and Waiting: Studies in the Social Organization of Access and Delay. By Barry Schwartz. Chicago: University of Chicago Press, 1975. Pp. v+217. \$12.00.

Julius A. Roth

University of California, Davis

Queuing and waiting are receiving increasing attention as theoretical issues and as social problems involving control of behavior and control over the expenditure of people's time. Barry Schwartz's contribution to this issue is, on balance, a welcome one, even though one must wade through some rather lengthy, arid stretches in order to find the interesting information and the stimulating gems.

The book is a mixture of theoretical discussion—some of it solidly argued and integrated with relevant literature, some highly speculative, with little data or authority—and the presentation of empirical studies which illustrate some of the theoretical points. The mixture is rather haphazardly distributed, and therefore the book does not "build a case" but meanders around a series of ideas, some ingenious, some well documented, some trivial, some that are wild stabs.

The author's first empirical chapter deals with the waiting-time structure of hospital emergency services. The data, from records produced by others, are thoroughly and interestingly analyzed, and the chapter is one of the better ones. But the fact that Schwartz is puzzled to find that increments in staff and increase in differentiation of function do not reduce patient delay shows the disadvantage of dealing with secondary data. One who has had direct experience with emergency services is not likely to be puzzled by such a result. Differentiation of function (which is intended to benefit the staff, not the patients), for example, leads to more queues for patients to wait in and therefore is likely to cancel out (sometimes more than cancel out) the advantage of more efficient use of staff time.

The chapter which follows deals with manuscript queues at the *American Journal of Sociology*, where he was on the editorial staff and obviously had full cooperation from the day-to-day staff. Here he is on top of his data at all times and is never puzzled by the outcomes. The advantage of being inside the data-producing process shows up clearly when one compares this chapter with the discussion of emergency-service records. He elicits some important and interesting generalizations from this data and, by drawing comparisons with other settings, the kind of generalizations about which one is likely to say, "Yes, of course," once they are called to one's attention but which one had not taken into account previously. Thus, deadline tasks receive priority over tasks which may be considered more important and which require greater expertise in the total procedure. Servers may at first work faster to try to catch up with a small queue, but when the queue gets very long (how long?), they give up and slacken their pace. Servers must choose between stability of the service process (which means that the length of the queue will vary) and stability of the queue (which means that the speed of the service will vary).

There follows some interesting discussion of the principle of order of arrival and how it is applied in various situations and a chapter on formal and informal priorities in an emergency medical system. After that, the book goes downhill.

There is a chapter on the theory of distributive justice in which flimsy data on trivial actions are used for testing broad hypotheses. Next is a chapter in which doubtful propositions about Catholic-Protestant differences in dissatisfaction about waiting are checked with survey data in which I, for one, have no confidence whatever. When the results are not as predicted, the author comes to the rescue with an ad hoc explanation. This kind of game is typical of one-shot survey studies, but it is unworthy of the imaginative and critical discussion which marks the early part of this book. Then there is a chapter which wallows in a psychoanalytic morass which leads nowhere. Readers would probably do themselves and the author a favor by stopping after the first five chapters.

Cooperation: An Experimental Analysis. By Gerald Marwell and David R. Schmitt. New York: Academic Press, 1975. Pp. xviii+209. \$16.00.

Richard Ofshe

University of California, Berkeley

This book is constructed from a series of reports of experiments executed under ABA design conditions. The experiments are built around a piece of equipment (a version of Cohen and Lindsley's apparatus) that permits information transmission between two physically separated subjects. They receive information about their own and the other's earnings from past and current sessions; own and other's earnings for working on an individual or joint, time-constrained, plunger-pulling, and button-pressing problem; and other's preference for joint or separate work. Each subject has the opportunity to shift from joint to individual work and to express a preference to move in the reverse direction. In some experiments, subjects are given the opportunity to take varying numbers of points (fractional money) from one another. The subjects are resident in several geographically different populations (north central or northwestern United States, or Norway), but no information is provided on whether these populations have been bred for maze-bright or maze-dull characteristics. Other situational variations include introducing same- or mixed-dyad construction, differing amounts of information about the other, and differing sorts of prior relations between dyad members.

The authors assert that they are interested in understanding the causal structure of cooperative behavior. They find, as many others have before them, that the choice between cooperative or individual task activity may be influenced by the payoff structure for alternative actions, equity considerations, risk, characteristics of the participants, etc.

Given the following advertisement from the authors' preface (p. xi), "This book represents our attempt to take a hard new look at cooperation,

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and contains the fruits of some six years of programmatic research on the topic," one is led to suspect that a powerful theory is about to be unveiled, some ripe hypothesis will be offered, or data that are able to scratch a diamond will be reported. I was unable to find anything but a series of ABA designs in which various factors were shown to have significant effects on the dependent variable, task-type choice. The authors' new look at cooperation is new only because they choose to call it new.

The research reported in this volume reflects either an unwillingness or an inability to define issues that can be resolved through research efforts. Consider, for example, the work on the role of communication in maintenance of cooperative behavior. The problem is stated with the introduction of a bit of folk wisdom, followed by a series of form references to previous publications (but no serious discussion of what was found and what is at issue) and the demonstration of a number of experimentally induced effects on behavior. In this case the opening bromide is: "'Keep them talking instead of fighting.' 'The problem is really one of breakdown in communication.' Explanations such as these for conflict and its reduction are probably the most common to be found in both the professional and popular literature" (p. 127). There then follow eight references to studies of matrix games that found communication to be important and one to the Deutsch and Krauss trucking game. No discussion of issues raised in these papers is offered. Unlimited talking is then introduced into the experiment, and it is shown to have a significant effect on reducing the disruptive effects of risk on cooperation. In other words, subjects could verbally agree not to steal from one another when such an option was presented. It is interesting to note that, in the review of the results of their program of research in chapter 9, "Conclusions and Speculations," the role of communication is treated as follows: "Channels of communication, usable warning systems, and certain types of past relations are examples of objective factors that seem to increase the possibility of sustained cooperation" (p. 182). Perhaps I am missing the essential point of the two experiments on communication that were reported in the text, but it seems to me that the point of departure (the bromide) and the end of the trail blazed by the research are difficult to tell apart.

Since the work proceeds at this level throughout, the accumulated product is disappointing. Although the research is executed with technical craftsmanship, it lacks sufficient development along any line to constitute much of a contribution in any direction.

Population and America's Future. By Joseph J. Spengler. San Francisco: W. H. Freeman & Co., 1975. Pp. xi+260. \$9.00 (cloth); \$3.95 (paper).

Henry S. Shryock

Georgetown University

The title of Professor Spengler's latest book indicates that he has drawn fairly heavily on the *Final Report of the Commission on Population*

Growth and the American Future (Washington, D.C.: Government Printing Office, 1972) and on the accompanying six volumes of research papers. At the same time, this book is in the mainstream of his own writings on the interface between economics and population which have been enriching the literature for about 45 years. In *Population and America's Future* he discusses the immediate determinants of population growth, its past and prospective future, and various of its effects and implications. Three aspects of the growth process are discussed: population growth, growth of numbers and of economic product, and changes in occupational and product composition.

In his discussion of the future mortality trend in this country, Spengler contends that death prevention can impose heavy costs of several kinds on advanced countries such as the United States. The worst of these is a state of "medicated survival" for millions, especially the aged. A comment on fertility is that, as family income rises and families can afford more recreation, the resulting increase in the value of time militates against the reproduction of children, who are time-intensive goods. Strangely, the discussion of the role of immigration makes no mention of illegal immigrants.

Spengler contends that untrained immigrants from abroad and from the South will find it difficult to find jobs because minimum wages exceed their initial productivity and that migration will not contribute to growth of aggregate output in the future as in the past. I note, however, that the poorly educated migrant accepts necessary jobs that are disdained by those born in big cities, that internal migration remains high, and that the proportionate contribution of net immigration to national population growth is on the rise again.

Both population growth and increasing GNP make demands on the environment. Since 1953 the United States has been a net importer of nonrenewable resources. Spengler believes that the cost of population control need not interfere unduly with efforts to increase the flow of energy and materials. Considerable attention is paid to the nature and consequences of an essentially stationary population. For example, the potential productivity per capita is viewed as higher in a stationary than in a growing population. A population ages as a consequence of becoming more stationary, and the effects of inflation tend to become more severe with age. Retired persons on fixed income lose not only from inflation but also from not sharing in national income growth imputable to public investment. Hence, the older person's most attractive alternative is to maintain his right to employment up to about age 75. Because of lack of modern experience, however, our knowledge of the behavioral propensities of stationary populations is limited.

Concerning population distribution, Spengler feels that a city's development is greatly influenced by unintegrated individual decisions. We seem to be evolving toward the "spread city"—an amorphous texture lacking strong nodal points combined with the exclusion of the poor and blacks from the growing parts and with the degradation of the environment. The

rate of changes here and elsewhere may be too fast for man's capacity to adjust.

Informed population policy has three principal objectives: optimum size, optimum distribution, and optimum quality. The disparity between actual and optimum population size does not necessarily create a corrective feedback, through the price system or otherwise. Formulation of policy involves not only choice of means but also considerations of ethical and welfare principles bearing on both ends and means. Population quality is more important now than in the 19th century, when skills were narrower in range and easier to learn.

These are but a few of the informative and challenging statements made in the book. One may have doubts about some opinions, for example, that reductions in infant and child mortality are dysgenic and will lead to future declines in the well-being of the elderly, that the amount of education offered will be cut back, or that immigration is easily controlled by legislative and administrative instruments. However, in the demographic field at least, I spotted few errors. Overall, the book is imbued with much wisdom and mordant wit and is eminently readable. It could well serve as a text in a course in general demography.

Male Fertility Survey: Fertility Knowledge, Attitudes, and Practices of Married Men. By William H. Spillane and Paul E. Roger. Cambridge, Mass.: Ballinger Publishing Co., 1975. Pp. x+191. \$14.00.

John F. Kantner

Johns Hopkins University

The subjects for this brief monographic study were 424 currently married men, residents of Pittsburgh, Pennsylvania, who, at times which were arranged so as not to conflict with the Pittsburgh Pirates 1971 World Series playoffs, presented themselves in groups of seven or so for a 65-minute interview covering the topics indicated in the book's title.

The study is novel in two respects: it puts to males the kinds of questions that in American fertility studies have generally been asked of women, and it abandons the private-household interview in favor of a group-administered questionnaire, a procedure which the authors feel has significant advantages. After each respondent's interview was examined for completeness, he was paid \$20 by check. The most troublesome part of the procedure, according to the authors, was the dissatisfaction displayed by some of the poorer respondents who preferred to be paid in cash. Since survey research is rapidly pricing itself out of the market, at least the market that most academic sociologists can enter, it is noteworthy that the average cost per completed interview "was \$31, including the \$20 respondent fee." It is not clear, however, what else this covers beyond the fee and the running of the group sessions.

The decision to study males was dictated by the evident gap in our knowledge about the other half of the decisional process. The use of the

group interview came about because of a flare-up of protective sentiments on the part of social caseworkers who were asked to provide information about their clients for purposes of developing a sampling frame. The first two chapters, short ones happily, and two appendices are devoted to recounting the frustrations which led to a recasting of the study. Years ago, Florian Znaniecki asked how much of the "cuisine scientifique" should be published. Obviously it is a matter of discretion, but in my judgment the authors recount their problems in launching the survey far more than is needed to understand their data or their interpretations.

There are more serious problems with this study. The completion rate works out at 56%. Moreover, the sampling frame is admittedly several years out of date and suspected of being "incomplete primarily with respect to recently married couples, younger couples living with parents, multiple families within the same home, migrant families recently arrived into the city, and low-income families in general" (p. 18). The sample thus represents nothing but itself, making the title of the volume more pretentious than is usually the case in such matters.

The group-administered questionnaire, which undoubtedly has some of the advantages the authors claim for it, also has some possible drawbacks. Its design must inevitably be simpler than the conventional questionnaire, with its elaborate skip patterns which permit a skilled interviewer to get a custom fit to the particular life circumstances of the respondent. Thus, for example, answers to questions about the number of children ever born, expected, or considered ideal need to be interpreted in relation to marital history. A person's view of these things is likely to differ from a first to a second or third marriage. Usual practice is to employ a branching format which allows for the breaking off of subgroups for whom questions can be asked in proper context. But this would result in chaos if tried with a group-administered instrument. No doubt because of this limitation, the present study makes no distinction between respondents by number of times married; it cannot tell what children are being referred to when the respondent reports on "children ever born"; respondents who use contraception as well as those who do not are asked how confident they are of the efficacy of the methods they use; and so on.

In many instances it is necessary to go into the field prior to the survey to list and screen households, just to develop the sampling frame. This can be a considerable fraction of the total cost, depending on how common the events are for which one is sampling and the design features of the sample. Once this expense has been incurred, the cost advantages of the group versus the individual interview are no doubt considerably reduced.

There are some interesting though frequently underdeveloped findings in the book. For example, mixed Protestant-Catholic marriages are the most likely to use contraception and are more inclined to use oral contraception than any other assortive combination except marriages in which one of the partners professes no religion (table 6-11). For Protestants, more education leads to greater contraceptive use and much greater preference for medical methods of contraception (chiefly the pill); among Cath-

olics, education makes much less difference in either extent of use or type of contraception, even though the great majority of Catholics use contraception (table 6-16). Perhaps because medical methods (again, primarily the pill) can be talked about more readily, the sampled couples who do not use contraception or who use nonmedical methods are less likely to know about their friends' use of contraception (table 6-18). Catholics who know about their friends' use have exceptionally low levels of nonuse (table 6-20). In fact, knowledge of friends' contraceptive use is much more closely associated with own use and the tendency to use modern methods than is education (table 6-21).

These findings are little more than suggestive, given the small sample with its unknown biases. In general, they are consistent with what has been learned from surveys of women. This leads to the suspicion that the barrier to advancement in our knowledge of fertility behavior consists, perhaps, as much in the failure to ask proper questions (even the proper improper ones) as in asking them of the proper people.

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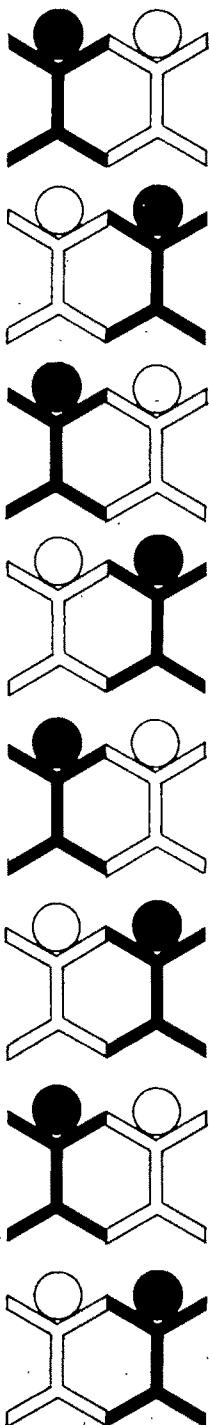
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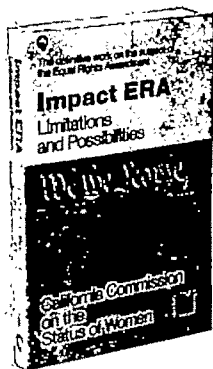
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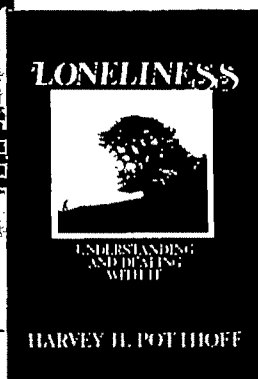
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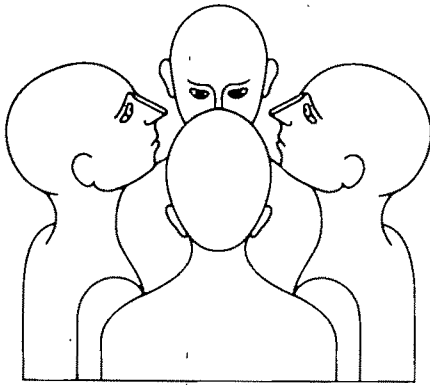
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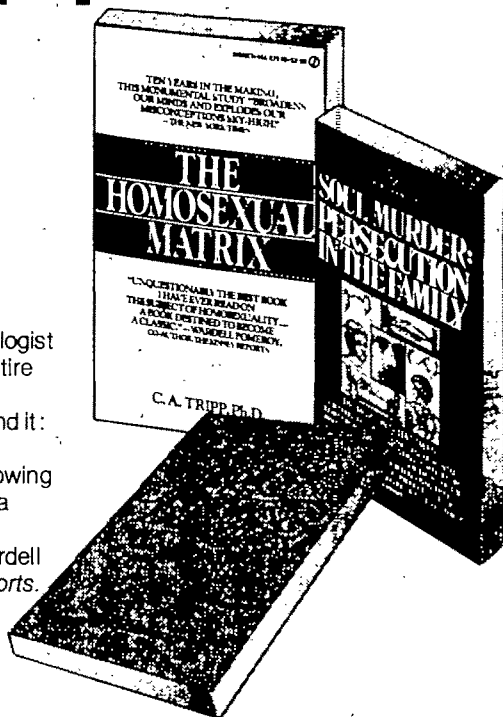
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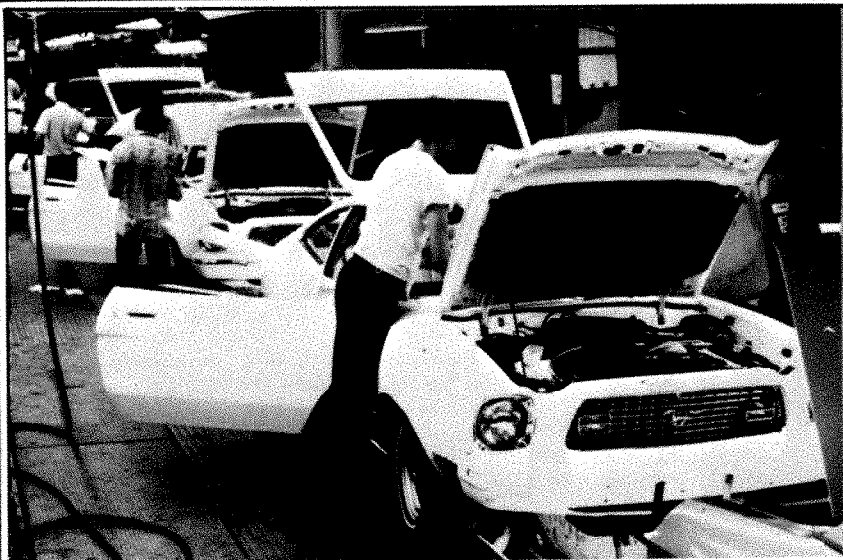
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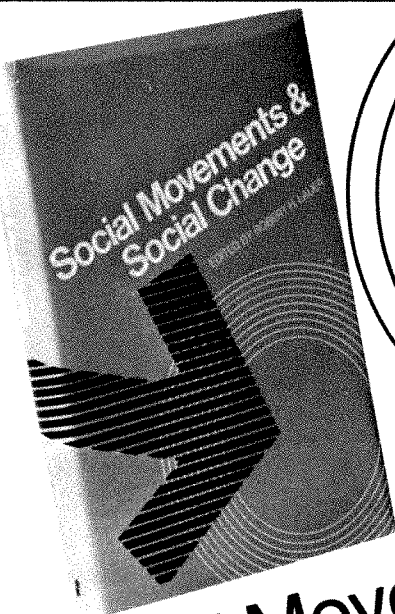
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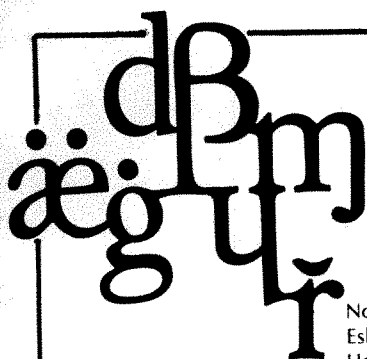
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